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**THE CATCHER IN THE RYE-VIEW?  
SOME REFLECTIONS ON REVIEWER'S RESPONSIBILITY**

In the eleventh issue of *Studia Etymologica Cracoviensia* (SEC 11, 2006, pp. 193-205) Dr. Wojciech Sowa published a bizarre review of my book on the Indo-European cereal names (Witczak 2003) under the deriding title "The Catcher in the Rye?". It is an ostentatious example of a text, which being written intentionally contains no unbiased assessment of the author's achievement. There are numerous incorrect or false statements in Sowa's review and multiple strong opinions are expressed with no justification. The reviewer demonstrated clearly that his knowledge of Indo-European linguistics is too modest to prepare a valuable assessment of a book devoted to historical-comparative problems.

The aim of my reply is to demonstrate Sowa's objections and critical remarks that are based on his exaggerated and unmotivated hypercriticism. First of all I must verify Sowa's mistakes of minor importance. Firstly, he wrote that "This book is based on the author's doctoral thesis defended in 1995". As a matter of fact it was finished in October 1995 but defended in March 1996. Secondly, on p. 194 he refers to Mallory (1994) as "quoted in Witczak's bibliography". Five contributions by J. P. Mallory (published in 1973, 1982, 1983, 1989, 1996, respectively) and one co-authored by him, *Encyclopedia of the Indo-European Culture* (Mallory, Adams 1997), are mentioned in my bibliography. No work, dated 1994, appears in my book. Thirdly, Lycian *xqqase* 'Futtermittel' (p. 197) should be corrected as *xθase*.

My book is written in Polish. This language does not belong to the international languages, thus it is obvious that my monograph will be hardly available to a broad circle of linguists or Indo-Europeanists. The reviewer, who presents such a book in an international language, should summarize conscientiously and correctly the views, arguments and conclusions of the author. This is not what Sowa did.

In footnote 1 (pp. 193-194) he refers to my views on the Proto-Indo-European homeland problem in the following way: "Witczak contests the Kurgan hypothesis of Gimbutas and follows the views of Gamkrelidze-Ivanov, Dolgopolsky and Sevoroshkin and especially Renfrew, who are looking for the home-

land of the Proto-Indo-European among the agricultural tribes of Anatolia (the neolithic centre in Çatal-Hüyük). [...] Witczak follows these opinions tendentiously, taking a priori someone's side in the discussion of the problem, which in fact does not bear much on the questions of Indo-European grammar". It is an imperfect presentation of my ideas. I accept the Kurgan area as a homeland of the Indo-Iranians, one of the main nations of Indo-European origin. Thus – in my opinion – it was one of the secondary homelands of the Indo-European people. As far as I know, it is now the most popular interpretation, accepted by numerous scholars, both the adherents of Gimbutas's hypothesis and those who localize the Indo-European homeland in other places. It is not true that I completely and "tendentiously" follow the views of Gamkrelidze, Ivanov, Dolgopolsky, Shevoroshkin and Renfrew. Firstly, in my book I presented five most popular opinions as to the location of the Indo-European homeland (Anatolia, Balkans, Eastern Europe, Central European lowland, Black Sea steppes, pp. 29-30), referring to as many as 27 authorities. What is more, I indicated (in footnotes) some extraordinary locations (suggested by W. Merlingen or J. Hodge) and the Central Asiatic hypothesis, present in the Polish scientific literature (H. Łowmiański, Z. Gołąb). Thus the reader was informed (pp. 29-32) that the Indo-European homeland problem remains unsolved, that my view should be treated as possible or probable, but not certain. Secondly, my opinion was formed on the basis of the archaeologically established centers of the neolithic agriculture and independently from Renfrew's book (1987).<sup>1</sup> According to Shnirelman (1989), four or five main areas of the neolithic agricultural culture can be distinguished:

- (1) Southern Anatolia (VII-VI mill. BC) – I place here the Indo-Hittite homeland and also the homeland of the Anatolian languages.
- (2) Balkan and Danubian Area (VI-V mill. BC) – I place here the homeland of the Central Indo-European tribes, who spoke so called Balkan-Indogermanisch.
- (3) Eastern European and/or steppe centre (V-IV mill. BC) – Indo-Iranian (and Tocharian) homeland.
- (4) Central European area (V-IV mill. BC) – the North-Western tribes of the Indo-European family, who used the traditional Indo-European language, as codified by J. Pokorny (1959).

Sowa makes a FALSE statement, indicating that my vision does not take in account the Indo-European grammar. I must stress in this place that my work referred mainly to the cereal names, thus I could not express my opinion on the Indo-European homeland at full scope. But the linguistic arguments are not ab-

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<sup>1</sup> During a Polish conference of young linguists organized in 1990 in Wenecja, I presented the same or similar view on the Indo-European homeland (Witczak 1995a), though I bought and read Renfrew's (1987) book one or three years later, during my research stay in Greece (in 1991 or 1993).

sent from my research. For example, I distinguish four Indo-European subgroups, connected with four different localizations of the neolithic centers, taking into account the laryngeals and their vocalic equivalents:

- (1) Anatolian – the Anatolian languages present both laryngeals and their vocalic allophones.
- (2) Balkan – the Palaeo-Balkan languages lose laryngeal, but they preserve three different reflexes of the laryngeal vocalic allophones (\**ǵ*<sub>1</sub>, \**ǵ*<sub>2</sub>, \**ǵ*<sub>3</sub>), also in the initial position (so called “prothetic” vowels).
- (3) Indo-Iranian (and Tocharian) – these languages express the loss of the vocalic allophones of the laryngeal sonants also in the diphthongs (\**ǵi*, \**ǵu*).
- (4) North-Western Indo-European – here the laryngeal vocalic allophones are completely lost initially (but not in diphthongs), but usually (sometimes for a time) preserved medially.

This one example (not included in my book) demonstrates clearly that my view on the Indo-European ethnogenesis is also confirmed by arguments taken from Indo-European phonology.

Sowa continues (fn. 1, pp. 193-194): “Witczak does not bring the arguments of Gimbutas to the discussion [...] Works such as M. Gimbutas, «Die Ethnogenese der europäischen Indogermanen», Innsbruck 1992, or M. Gimbutas, «Das Ende Alteuropas», Innsbruck 1994, are not mentioned at all. Witczak does not quote any argument against Renfrew’s opinion [...], e.g. the similarities between Proto-Indo-European and Uralic, and he does not understand the absurdity of the presumed existence of a language ancestral to Greek in Greece at around 6500 B.C. [...] Witczak passes over in silence the heroic epic poetry among the Indo-Europeans (indogermanische Dichtersprache), which could point to the warrior (nomadic) character, even if not of the whole Indo-European population, then at least among the aristocratic class”. It is a flawed argument. I quote in my bibliography four earlier works of Gimbutas (published in 1963, 1970, 1982, 1985) and one citation (on p. 33). Thus the scientific achievements of M. Gimbutas were known to me and two above-mentioned books contain the same arguments, which are presented in her earlier works. The similarities between Proto-Indo-European and Uralic are, of course, expected in sense of a distant “Nostratic” relationship. There are numerous similarities and mutual borrowings between Indo-Iranian and Ugro-Fennic, which can be explained better in the case of accepting the interpretation of the Kurgan cultures as Indo-Iranian. The imputed “absurdity” as to the origins of the Hellenic nation is Sowa’s own. There are some language layers in the area of the continental (and insular) Greece. Some of them are evidently of Indo-European origin, namely of Palaeo-Balkan and Anatolian. A non-Indo-European ingredient was also possible, but its character and origin (from Africa?, Asia?) is unclear. I did not state anywhere in my book that the Greeks are autochthonous in Greece, as it is tacitly

suggested by Sowa. It is evident, however, that the Greek language, which belongs to the Palaeo-Balkan subgroup, had to originate somewhere in the heart of the Balkan Peninsula. It is worth adding that the heroic epic poetry is a late phenomenon (evidently late Indo-European or even post-Indo-European) and, pace Sowa, it points hardly to the nomadic character of the Indo-European society.

Passing to the main theme of my book Sowa states as follows (p. 194): “Without any doubt the author worked hard to find the rich comparative material from so many (not only Indo-European) languages. But the general impression after reading the book is that in fact it was vain work [...] the book is a mere enumeration of forms found in etymological dictionaries, with some non-linguistic, botanical, or archeological commentaries. Unfortunately, they, and not the linguistic comments, are the advantages of this study”.

In my book I collected and explained as many as 36 Indo-European reconstructed archetypes referred to the cereals and I discussed etymologically as many as 49 additional cereal names, which are attested in only one language or in one language subgroup. Even if some reconstructed forms are faulty or doubtful, most of them may be accepted with no problem. I published my Ph.D. dissertation in order to convince modern, especially young linguists, of the complete incorrectness of the words expressed by the well known American Indo-Europeanist that the Proto-Indo-European language demonstrates “the lack of specific terms for grains or vegetables” (Lehmann 1973). This opinion is – as I tried to demonstrate – incorrect, but Sowa says nothing about it. At the same time his words “vain work” suggest that he agrees with Lehmann’s statement. The cereal terminology is ample and very archaic in most Indo-European languages. It suggests that cereals were known to our Indo-European ancestors.

Further remarks by Sowa are peremptory and sharp like a razor (pp. 194-195): “Reading Witczak’s book, one quickly gets the impression that the author actually misunderstands the methods of comparative Indo-European linguistics. As the main criterion of the Indo-Europeanness of a form he treats its occurrence in Hittite and other IE languages or the existence of related Nostratic forms (!). The author totally ignores morphological analyses; most important to him seem to be semantic similarities and phonetic correspondences between the languages”. However, Sowa ascribes to my analysis his own ignorance and misunderstanding of the comparative and lexical studies. It is a well known fact that the oldest Indo-European languages, such as Hittite, Luwian, Sanskrit (especially Vedic), Avestan, Ancient Greek (especially Mycenaean Greek) and the like, contain a wealthier and relatively more archaic vocabulary than the younger or modern languages. What is more, the Anatolian subgroup (with Hittite as the most representative member) must be treated as being in opposition to all the remaining non-Anatolian subgroups (and languages). If an Anatolian appellative is related to some close or exact equivalent(s) attested in the European languages,

then we may expect that it belongs to the oldest layer of the Indo-European vocabulary. It is not true that I ignore morphological analyses, but it should not come as surprise that I lay stress on the phonetic and semantic questions. Morphology can rarely indicate whether the analyzed word is a borrowing or a cognate form, but the regular or irregular phonology decides the question with a high degree of probability. However, the reconstruction of an Indo-European archetype is not possible without both the phonological and morphological analyses. The semantic analysis of the cognate words makes it possible to reconstruct the original meaning of the reconstructed lexeme, as well as to explain multiple changes of the sense in some particular Indo-European languages.

Nostratic parallels are included for that reason that they give possibility to explain better the history of the reconstructed words. They may represent loanwords (also typical *Wanderwörter*) or accidental convergences or some distant cognates. The present-day phase of the development of the Nostratic studies gives relatively few possibilities to determine the result in the positive way. Anyway, Nostratic parallel(s) may be neither the main nor additional criterion of the Indo-European character of a lexeme. This is my opinion, though it seems that Sowa thinks differently.

Here I would like to return to my morphological analyses and to the credibility of impressions of the reviewer. In another place Sowa repeats his statement “The author totally ignores morphological analyses”, indicating “the absolute lack of strict morphological analyses” (p. 199). However, in yet another place Sowa writes: “author does not quote any bibliographical references on the occasions of the (rarely) mentioned grammatical (phonological or morphological) problems”. The reviewer suggests to the reader that there are no morphological analyses in my book, although later he is forced to admit that they are “rarely” mentioned. Thus it is clear now that the tendentious biased expressions like “TOTALLY ignores” or “ABSOLUTE lack” were introduced contrary to facts. All the words and reconstructions are explained from the morphological point of view in the standard way by means of abbreviations, e.g. \***pūrós** m., rarely \***pūróm** n., with indication (in my comments) of verbal root \**pu-* plus the suffix *-ro-*. The stem (in this case *o*-stem) is always demonstrated in necessary cases. Morphological analyzes are present, but Sowa seems to ignore them. Should the reader compare Sędzik’s book *Prasłowiańska terminologia rolnicza* (1977) and my book, he might easily conclude that the commentaries and detailed morphological analyses in both these works are similar and analogous. Sędzik renounces the morphological analysis and additional information in such cases, which are obvious for each Slavist. By analogy, I introduced explanations and comments in some ambiguous or uncertain cases. Words or reconstructions, which are, or should be, clear for each Indo-Europeanist, are deprived of superfluous information or analysis. Sowa wants to have all the basic information, as he is a begin-

ner in the field of Indo-European studies. He does not take into account the fact that the lexical entries in my dissertation are formed as headings in the standard comparative and etymological dictionaries. To the best knowledge of mine there is no comparative or etymological dictionary, which would give more lexical materials, ample comments and further information on the morphology, structure and derivation. A comparison of my book with other publications of similar type (e.g. Sędzik 1977, Rogowska 1998) clearly demonstrates that Sowa is wrong.

The next reproach is formed in the following way (p. 195): “One misses also detailed commentaries on individual forms, and on the kind of evidence which allows him to bring together all kind of forms at disposal in (mostly old) dictionaries. Cf. e.g. Gk. Thess. *δάρατος*, Delph. *δάρατα*, Maced. *δράμις* ‘bread’ (p. 84), which are quoted without pointing out the source of the forms”. Sowa demands the exact sources of the data, which are completely superfluous from the comparative point of view. Each classical philologist can easily find the exact references to the literary sources, as well as to the published inscriptions. Also linguists or Indo-Europeanists, not educated as classical philologists, should obtain necessary data without any problem. The words in question do not denote a cereal plant, thus they remain beyond the scope of our theme. Therefore it is obvious that detailed comments on these individual forms might be omitted.

In the next passage Sowa introduces subsequent fiction (p. 195): “Words are cited without differentiation into more archaic forms and younger innovations, into inherited forms and loan-words, and so on”. This sentence gives strong impression that Sowa did not read my study at full extent. Chapter X (pp. 121-124) is devoted to the stratification of the Indo-European cereal terminology. I differentiate the following groups: 1. Nostratic heritage; 2. “Indo-Hittite” cereals; 3. Ancient wandering phytonyms; 4. Cereal names of Indo-European origin; 5. Innovations; 6. Borrowings. Also, all the lexical headings in chapters III-IX are realized in the following way: C. Cereal names of Indo-European origin (here archaic names are listed first, the probable innovations follow them); D. Other names attested in particular languages (here possible cereal names with an archaic structure are mentioned before obvious innovations, whereas wandering names and borrowings are listed finally). Also when analyzing lexical items I consistently indicate archaisms, innovations and borrowings, discuss and present different opinions of scholars on these questions.

Sowa tries to instruct the author (p. 195): “For IE etymologies it seems absolutely useless to quote forms from all modern Indic languages if a form is attested as early as Sanskrit; the same applies to old and modern Iranian languages and to Latin and the Romances languages. What is more, the modern forms very often lack an intermediate reconstructed basis, so that the impression arises as if the Kafiric forms would be direct heirs of Indo-European (or Indo-Aryan)”. I cannot agree with Sowa’s words for three important reasons. Firstly, the modern

names demonstrate numerous semantic changes (and this part of my study is praised by the reviewer, p. 200: “The semantic developments of cereal terminology form the really interesting part of this book”). Secondly, not all the modern names come back to the Sanskrit or Latin archetypes. Thirdly, Avestan was not a prototype for all the Iranian languages, as well as Sanskrit was not a language from which all the languages of India derive. Obviously the reconstruction of an intermediate form seems superfluous if the Sanskrit or Latin form is quoted above. However, I introduced the intermediate forms whenever it was possible or necessary (e.g. in the case of the Germanic, Celtic, Slavonic, Iranian, Nuristani, Dardic languages and so on). Sowa’s term “very often” associates with the complete exaggeration (his words remain with no concrete example). Again and again he does not want to see these elements which are present in my work.

Sowa begins his polemics referring to the lexical and etymological questions (pp. 195-196): “This lack of commentaries, except for semantic ones, unfortunately has the consequence that in many cases Witczak’s etymological proposals have to be rejected, due to morphological, phonological shortcomings or due to problems of the history of the languages involved (language contacts, loan-words)”. Below (p. 196) he lists 6 examples taken exclusively from Celtic languages.

On page 196 Sowa writes as follows: “Welsh [g]wenith, Bret. gwiniz ‘wheat’ is probably not derived from the word for ‘spring’, as suggested by Witczak on page 42 (with a question mark; according to Witczak from \*wesH<sub>2</sub>aros, cf. however commonly accepted \*h<sub>2</sub>wes-r/n-).” Sowa quotes two contradictory etymologies of this term: from Celtic \*wo-nikto- ‘das Ausgesiebte / sieved out’ or from Welsh gwen adj. ‘white’ (< Celtic \*windo-). According to him, two these explanations are better founded than the one preferred by me. Sowa questions my reconstruction \*wesH<sub>2</sub>aros, which can be securely reconstructed on the basis of the Tocharian, Armenian and Baltic appellatives. It derives evidently from the Indo-European name of ‘spring-time’, \*wesH<sub>2</sub>r (gen. sg. \*wesH<sub>2</sub>nos), but Sowa maintains that the “commonly accepted” reconstruction must contain initial \*H<sub>2</sub>-. Unfortunately, this statement is false. Sowa follows undoubtedly Smoczyński (2001: 106), who derives the Indo-European name for ‘spring(-time)’ from the root \*h<sub>2</sub>wes- ‘rozjaśnić się / to clear up, brighten’. Smoczyński’s reconstruction is impossible for two reasons. Firstly, the suggested derivation is semantically improbable (\*h<sub>2</sub>wes- denotes ‘dawn’ or ‘morning’ or ‘tomorrow’, never ‘spring-time’). Secondly, the reconstruction remains in complete disagreement with the lexical data. Initial \*H<sub>2</sub>- should be preserved as /a-/ in Palaeo-Balkan languages, whereas both Gk. ἔαρ n. ‘spring’ (< \*wesH<sub>2</sub>r) and Arm. garun ‘id.’ (where gar- derives from \*wesH<sub>2</sub>r) demonstrate no trace of the vocalic laryngeal allophone before \*w-. Thus Sowa’s words “commonly accepted” are obvious misinterpretation.

The Brittonic term for ‘wheat’ (Welsh *gwenith*, Bret. *gwiniz*) documents a different vocalism than *-o-* in the initial syllable, thus the first derivation for *\*wo-nikto-* seems doubtful. Its connection with Celtic *\*windos* adj. ‘white’ is possible, but hardly certain. The etymology introduces the geminate *-nn-* (< *\*-nd-*) and the development *\*i > \*e* in the position before nasal, cf. W. *gwyn* ‘white’, *gwenith* ‘wheat’. The alternative explanation from the oblique stem *\*wesn-* ‘spring-time’ > Brittonic *\*wenn-* (see below, Mod. Ir. *errach* ‘id.’) is possible as well.<sup>2</sup> The final element *-ith* can derive from Celtic *\*[p]itwo-* ‘cereal, crop’, cf. OIr. *ith* ‘grain, corn’, W. *id*, Bret. *ed* ‘cereals’ (all from Celt. *\*[p]itu-*). Finally, I prefer the original semantics ‘spring cereal’ than ‘white cereal’ for some reasons presented in my paper on *Lithuanian Cereal Names* (Witczak 2000).

Sowa refers next to the second Celtic cereal name (p. 196): “OIr. *eorna* ‘barley’ (page 43f., 54) is compared with OIndic *yávah* ‘grain, corn, barley’, with a question mark as possible Celtic continuation of a formation *\*yewH<sub>1</sub>os-* (m.) in a general meaning ‘grain’ (in fact, *yávah* seems to be *\*jéyo-*, without laryngeal, cf. also Gr. *ζεῖαι* < *jey-jeh<sub>2</sub>-* Watkins 1978, 595, in Witczak without any comment on word-formation); cf. however the proposal of De Bernardo Stempel, who interprets it as an heteroclitic *\*esor-n-yā:* to Irish *errach* ‘the season of spring’ (< *\*es-en*, *\*os-en* ‘Erntezeit’ [...]).” The question must be differently clarified for four reasons. Firstly, Mod. Irish *errach* ‘the season of spring, spring-time’ (and Sc. Gaelic *earrach*) derives probably from *\*ferrach* (< Celtic *\*wesrāko-* < PIE. *\*wesH<sub>2</sub>r* n. ‘id.’) by the irregular loss of initial *f-* in the sandhi conditions. This process appeared as early as in Middle Irish phase, e.g. Mod. Ir. *áinleóg* ‘swallow’, Scottish Gaelic *ainleag* and *fainleag* ‘id.’ (< OIr. *fannall* ‘swallow’ < Celt. *\*watnello-*, cf. W. *gwennol*, Corn. *guennol*, Bret. *gwennelli* ‘id.’); Sc. Gaelic *fath*, dial. *ath-thalmhain* ‘mole’ (< Celt. *\*watu-*, cf. W. *gwadd* ‘mole’, Corn. *god*, Bret. *goz* ‘id.’); Middle Irish *furáil* ~ *uráil* ‘incitement, command’ (< OIr. *iráil*); Mod. Ir. *fabhra* ~ *abhra* ‘eyelid’; *fuair* ~ *uar* ‘cold’. As Old Irish cereal name *eorna* ‘barley’ shows no trace of initial *f-*, de Bernardo Stempel’s etymology cannot be treated as acceptable. Secondly, the traditional derivation from *\*yewo-* is given in many dictionaries and monographs (e.g. Stokes, Bezenberger 1894: 223; Trautmann 1923: 107; Walde, Pokorny 1930: 202-203; Pokorny 1959: 512; Gamkrelidze, Ivanov 1984: 655; MacBain 1998: 156 < *\*yewo-rnyo-*). Thirdly, the existence of a laryngeal is firmly established on the basis of Lithuanian facts. Lith. dial. *jáujas* ‘barn, granary’ (1 AP) derives from IE. *\*yówHyos* or *\*yéwHyos* (Illich-Svitych 1979: 54), whereas Lith. *javaĩ* (pl.) ‘grain’ (2 AP) represents *\*yówHos* or *\*yéwHos* (Illich-Svitych 1979: 26). This laryngeal seems to be *h<sub>1</sub>*, as suggested by the cognate Greek and Indic forms, listed below (both Common Gk. *\*ζεFεhia* and Indic *\*yavasī-* indicate

<sup>2</sup> It is uncertain whether W. *cann* ‘white, clear’ derives from *\*kandos* or *\*kasnos*.



clearly *\*yewh<sub>1</sub>es-iə<sub>2</sub>*). Fourthly, both Greek ζεά, ζειά, ζέα, ζέη (f., usually in pl.) ‘Triticum dicoccum’ and Cretan δηαί (f. pl.) ‘barley’ represent the archetype Common Greek *\*ζεϝ-εη-ια* < *\*yewh<sub>1</sub>es-iə<sub>2</sub>*, which is confirmed by the Marathi form *javśī* (f.) ‘the flax plant’ (Turner 1966: 603, No. 10436), the feminine variant form of Marathi *javas* (m.) ‘linseed plant’, (n.) ‘linseed’ < Skt. *yávasa-* (m./n.) ‘grass’ < Skt. *yávaḥ* m. ‘barley’ (these two Sanskrit forms go back to IE. *\*yéwh<sub>1</sub>eso-* ~ IE. *\*yéwh<sub>1</sub>o-*, respectively). Fifthly, according to my opinion, the Indo-European cereal *\*yewh<sub>1</sub>os* belonged originally to the masculine *o*-stem nouns. Sowa gives the wrong spelling of Sanskrit *yávah* [sic! twice repeated!]. Sowa seems to suggest to the reader of his review that these mistakes are present in my book. However, Witczak introduced the correct forms.

Sowa comments (p. 196): “OIr. *sacul*, MIr. *seagul* (p. 112) are obviously borrowings from Latin *sēcāle* through British Celtic. In Witczak’s book such a possibility has not been signalled at all”. Unfortunately, the reviewer misleads the reader twice. Firstly, the lexical entry begins with the following information: **3.** Lat. *sēcāle* ‘rye’ (> Gk., ??Alb., Celt., Rom., Gmc.). The sign > informs clearly that the Latin term was adapted into Greek and Albanian, as well as into the Celtic, Romance and Germanic languages. There is no necessity to repeat this obvious information by an additional comment. Secondly, I stressed in my analysis that “the distribution of the [Latin] word in question is undoubtedly connected with the Roman economic activity, thanks to which the rye was introduced to the cultivation in Italy, Greece, Pannonia and Anatolia” (in Polish; my translation). Even if Witczak did not signal the Latin origin of the Goidelic words for ‘rye’, it is clear that the above-mentioned phrase suggests it. But Sowa says outright that my opinion is different and incorrect.

Sowa agrees with me (p. 196) that OIr. *arbor*, gen. sg. *arbe* ‘grain, corn’ (gen. pl. *arbann*) represents an old heteroclitic noun, but he prefers “the traditional reconstruction” *\*h<sub>2</sub>erh<sub>3</sub>u<sub>r</sub>* (Sowa’s transcription) and derives it further from the verbal root *\*h<sub>2</sub>erh<sub>3</sub>-* ‘to plough’. However, Pokorny (1959: 63) gives the secondary nominal root *\*ar(ə)u-*, cf. Gk. *ἀροῦρα* (Myc. *a-ro-u-ra*) ‘Ackerland / corn-land’. This derivation was quoted in my book (p. 83) as uncertain.

According to Sowa (p. 196), W. *wtr* ‘light corn, light grain’ “seems to be a ghost-word”. If so, then this ghost-word was introduced to the literature not by Witczak, but by earlier scholars, who were quoted on p. 119.

Sowa admits as follows (p. 196): “The equation of Hittite *parḫuena-* m. ‘sort of wheat’ and Gallo-Lat. *arinca* f. ‘wheat’ must be false, too (p. 103). Witczak gives ‘sort of wheat, probably Triticum dicoccum Schrank’ as the original meaning, noting that it should be interpreted as an Anatolian-Celtic isogloss. Both forms are supposed to ultimately go back to *\*p<sub>r</sub>Hwen-*, the immediate Proto-Celtic pre-form being *\*parwen-kā*. The equation is problematic, on the one hand due to the dubious status of the Gaulish form (actually it is not clear if

it is really Celtic [...]). On the other hand the Hittite form is not clear either, first of all semantically. It seems to mean something like ‘all kinds of seeds’ and appears with quantities”. Both these problems, indicated by Sowa, are doubtful. In my comments I cited Pliny’s opinion (*Nat. H.* 18, 81) on this cereal (“arinca Galliarum propria”), thus the Gaulish status of *arinca* seems relatively certain. Both Watkins (1975: 185-186) and Witczak (2003: 107) quote the Hittite noun *parḫuenaš* (orig. *o*-stem) in an extensive list of cereals among other kinds of wheat (e.g. Hitt. *ZÍZ-tar* ‘kind of wheat’, *še-ep-pi-it* ‘id.’, *par-ḫu-e-na-eš*, *e-wa-an* ‘kind of barley’, *kar-aš* ‘kind of wheat (?)’, *ḫa-at-tar* ‘kind of wheat’, *zi-na-il* ‘id.’), thus the semantics of the Hittite word was originally determined and clearly limited to a concrete cereal meaning. The suggested equation is perfect semantically and phonologically acceptable. The loss of initial \**p*- is well attested in Celtic. Sowa admits that “the \**u* of the pre-form should not disappear – thus rather \*\**aruinca* would be expected”. However, there are numerous Continental Celtic forms demonstrating a regular (or irregular) loss of \**u*, especially in the pre-tonical position (Prósper 2002: 407-412), e.g. personal names *Doiterus*, *Doiderus*, *Doidena*, *Doidina*, *Doitena* and *Doviderus*, *Doviterus*, *Dovitena*, *Dovidena*, *Doviteina*, attested in Hispano-Celtic anthroponymy.

Another example of Sowa’s hypercriticism refers to IE. \**knt*- (p. 197), attested both in the Anatolian and European subgroups with a cereal semantics. Sowa tries to explain Anatolian cereal name (Hitt. *kant*- ‘a kind of cereal’, Luw. *kant*- ‘Einkorn, *Triticum monococcum* L.’, he adds here Lyc. \**xada* ‘Getreide’ and the Lycian place-name *Kadyanda*, liter. ‘reich an Getreide’) “as a loan word from Indo-Iranian” (cf. Av. *gantumō*, OInd. *godhūma*-) without explaining the fate of the final element *-uma*-. Further he asks, why Van Windekens’s (1976: 181) etymology of Toch. B *kanti* was omitted in my presentation. The reason is that Toch. B *kanti* ‘a kind of bread’ represents no cereal name. I must stress once more that I discussed the Indo-European cereal names, and not the Indo-European terms for ‘bread’. This is why Van Windekens’s derivation of Toch. B *kanti* from IE. \**gnet*- (or \**gnedh*-) was not mentioned in my book. All the possible equivalents with non-cereal meaning are given only in such a case, if they derive from (or are related to) a cereal name. I quoted Van Windekens’s contribution in the bibliographical reference to the lexical heading, but I decided not to quote his etymology, as it was treated as doubtful by most competent tocharologists (e.g. by Hilmarsson 1996: 78 and Adams 1999: 139).

Sowa adds (p. 197): “Most bizarre is the reference to an unattested Lusitanian protoform (\**kentēnom*) for the Spanish and Portuguese forms. The way [in which] the form is quoted suggests that the proposal goes back to Meyer-Lübke. This is wrong. In Meyer-Lübke there is no mention at all of a Lusitanian origin of *centeno/centeio*. If the author assumes such an origin, he should state clearly [sic!] that it is his own view”. This quotation is followed by a long passage

referring to the Lusitanian language and its attestation. Unfortunately, Sowa's presentation is all wrong in this place. The text in Polish original runs as follows (p. 111, my translation): "Lat. **centēnum** (n.) 'rye, *Secale*' (André 1985, 55); Sp. **centeno**, Port. **centeio** 'id.' (Meyer-Lübke, RomEW nr 1811, 175) < Lusit. \***kentēnom**". It is clear that the reference indicates only evidence, and that the derivation from Lusitanian is suggested on the basis of the characteristic distribution of the Ibero-Romance appellatives and the suffix *-ēno-* (< IE. adjectival *-eino-*, cf. Lat. *-īmus*), whose substratal and Lusitanian (or "sorotapto" in his terminology) character was established many years ago by Joan Coromines (vel Corominas), the well known Catalanian linguist and etymologist. On p. 112 I wrote the following words on Lat. *centēnum*: "The distribution of this word in the Romance languages demonstrates that the term in question was borrowed from the Indo-European Lusitanians, who in antiquity occupied the western areas of the Iberian Peninsula". What is more, I indicated clearly that I disagree with the traditional etymology, which derives Late Latin *centēnum* from Lat. *centum* '100' (referring to Pliny, *Nat. H.* XVIII.16, 40, and to two modern dictionaries by Ernout and Meillet and by Buck, respectively), thus it would seem to be a clear indication that the Lusitanian attribution is my own proposal. What is more, also the words "the connection PROPOSED HERE" (p. 111) demonstrate unanimously that the reconstruction, which includes equivalents taken from Anatolian (cf. Hitt. *kant-* 'a kind of cereal', Luw. *kant-* 'Triticum monococcum L. '), Tocharian (cf. Toch. B *kanti* 'a kind of bread'), Late Latin and Romance (< Lusitanian), as well as Dacian *κορίατα* ('Triticum repens'), represents my own achievement. Further Sowa tries to persuade the readers of his review that Witczak knows little or nothing about Lusitanian (sic!). This action bears witness to Sowa's one-sided criticism. He should indicate that Witczak wrote not only the article on the position of Lusitanian within the Indo-European language family (Witczak 2002), but also the first and only monograph on the Lusitanian language (Witczak 2005), which contains as many as 472 pages.

According to Sowa (p. 198), "The author does not try to verify the forms he cites. This is for example the case with Lat. *spelta*, mentioned as «Pannonian» by Hieronymus, attested for the first time in 301 A.D. (Ed. Diocl.)". The ancient distribution was correctly verified, though Sowa tries to reject it on the basis of single (sic!) Pannonian place name, whose etymology seems extremely doubtful (*Ulcisia* hardly derives from Alb. *ujk* 'wolf' and IE. *wlk<sup>w</sup>os* 'id.'). The ancient sources give sufficient information on the separate status of the Pannonian language, which belonged neither to the Celtic subfamily, nor to the Germanic one (cf. Tacitus, *Germ.* 43.1: "Cotinos Gallica, Osos Pannonica lingua coarguit non esse Germanos"). It is, of course, possible that Pannonian *spelta* represents the basic *e*-grade of the root. I preferred to reconstruct \*(s)pl̥t- on the basis of the cognate Greek and Latin forms.

Sowa repeats with no justification (p. 198): “This lack of detailed analysis as well as the lack of commentaries is the main objection against this book” and he adds that “The only commentary follows Old Prussian *gaydis* ‘Weizen’ on page 99”. In fact, most headings are accompanied by similar comments.

Further Sowa raises new objections to me (p. 198): “In the majority of the examples the author follows etymologies proposed long time ago, e.g. in Pokorny’s IEW. New literature or the modern way of reconstruction have hardly been taken into account. Indo-European reconstructions often look old-fashioned, and unfortunately they do not lack some severe mistakes either. The most obvious example is the use of the laryngeals and apophony. Sometimes we find some monster forms, as e.g.  $*d_f^{\dot{r}}HwaH_2$  (p. 83) with a laryngeal and a long sonant in one root”. Sowa gives such a vague information as to (new) etymologies of the cereals, which were ignored in my book. He does not explain what new etymologies and works are omitted. He does not inform the reader which new publications were not taken into account. Thus his objections hang in the air and therefore I cannot defend myself. However, the reviewer should take into account that the published work was written in 1992-1995 and prepared to publication by the end of 1996. Of course, during the proof phase, which was made in the last quarter of 2003, I introduced a number of new references to some modern and most valuable publications (Mallory, Adams 1997), but the changes (additions) were by nature minimal. As far as I remember, little more than twelve new references were introduced (e.g. Braun 1998, Dolgopolsky 1998, Greppin 1996, Hilmarsson 1996, Jones-Bley, Huld 1996, Mańczak 1997, Renfrew 2001, Taracha 2000, Witczak 1997, 2000).

Sowa testifies to his ignorance, saying that Witczak’s Indo-European reconstruction often looks “old-fashioned”. My reconstruction seems hardly similar to the traditional (Brugmannian) Indo-European reconstruction, used in Pokorny’s IEW, as I accept the progressive reconstruction with the laryngeals. The young linguist from Cracow seems to suggest that his reconstruction is better and newer, thus the reader is informed that I used an archaic, old-fashioned form. But if somebody would compare three ways of reconstruction, then he could conclude that the reconstruction used by Sowa is more traditional and my way of writing is innovative, modern and preferable, e.g.

Pokorny	<i>arə-</i>	‘to plough’	Sowa	<i>h<sub>2</sub>erh<sub>3</sub>-</i>	Witczak	<i>H<sub>2</sub>arH<sub>3</sub>-</i>
Pokorny	<i>ǰeyo-</i>	‘barley’	Sowa	<i>ǰéyo-</i>	Witczak	<i>yewH<sub>1</sub>o-s</i>
Pokorny	<i>ǰlk<sup>w</sup>os</i>	‘wolf’	Sowa	<i>ǰlk<sup>w</sup>os</i>	Witczak	<i>włk<sup>w</sup>os</i>
Pokorny	<i>reidh-</i>	‘to press’	Sowa	<i>rejd<sup>h</sup>-</i>	Witczak	<i>reidh-</i>

The glottalic reconstruction is – in my opinion – very doubtful and unverifiable and Sowa seems to show the full agreement with my position. Thus I can-

not understand why my reconstruction of the Indo-European language “often looks old-fashioned”. Rendering the mediae aspiratae as  $*b^h *d^h *g^h *ĝ^h$  (against  $*bh *dh *gh *ĝh$ ) is an “artificial” innovation (Sowa used both scribal systems: IE.  $*gned^h$ - and IE.  $*ndhi$ , p. 197) and such a modernization of writing remains with no value.

As regards laryngeals, apophony and “monster forms”, Sowa takes the writing  $*d̥^hHwaH_2$  (p. 83 of my book), indicating correctly that this form (“with a laryngeal and a long sonant in one root”) is impossible in Indo-European. However, the reviewer has himself prepared this fiction. The attested writing (p. 83) is  $*d̥^hHwaH_2$  with a long/short sonant (plus a laryngeal) and the successive repetitions of the same Indo-European appellative  $*d̥^hwaH_2$  or  $*d̥^hwaH_2$  (pp. 123, 127, 134, 141) clearly demonstrate that the laryngeal on p. 83 appears as a misprint. The cereal term is archaic and attested in five different subgroups of the Indo-European language family. It is obvious that the Baltic and Slavic appellatives contained originally short sonant  $*r̥$  (which is guaranteed by the Balto-Slavic accentuation), whereas the Indic, Celtic and Germanic forms derived from its variant with the long sonant  $*r̄$ . The variation of  $*r \sim *r̄$  cannot be explained with aid of the laryngeal theory and must be treated as a standard example of the Indo-European apophony.

Another example is IE.  $*pūrós$  m.,  $*pūróm$  n. (*o*-stem) ‘a kind of wheat’, attested in Gk.  $πυρός$  ‘id.’, OInd.  $pūrah$  m. ‘cake’, Lith.  $pūraĩ$  (m.pl.) ‘winter wheat (crops)’, Latv.  $pūri$  (m.pl.) ‘id.’, Slavic  $*pyr̃$  m. /  $*pyro$  n. ‘a kind of wheat’. According to Sowa (p. 198), Witczak “falsely interprets Lithuanian accentuation, stating that «it points to the apophonic length of the root [vowel] -u- and not to the lengthening [so printed! – KTW] caused by laryngeal» (thus  $*pū$  :  $*pu$ ). Consequently, he does not try to comment on the Greek and Indic length at all”. Sowa believes that Greek  $πυρός$  represents IE.  $*puH_2-ro-s$  in the meaning ‘the pure one, der Reine’ (< IE.  $*peuH_2$ - ‘reinigen’) and “Lith.  $pūraĩ$  must surely be related”. He signals that also his mentor Smoczyński (2001, 135) derives Lith.  $pūraĩ$  from  $*puH-ro-$ . Unfortunately, Sowa is completely wrong suggesting that Witczak’s interpretation is based on the (secondary) Lithuanian accentuation. Many years ago Illich-Svitych (1979, 61) demonstrated that Lith.  $pūraĩ$  occurs with a secondary accentuation (4 accentual paradigm [= AP] in the standard language, both 2 AP and 4 AP in dialects). However, if the long vowel  $*ū$  were original (as documented by the Indo-European lexical material) and the Indo-European archetype showed the oxytone stress, as indicated by Gk.  $πυρός$ , we could expect 1 AP as a result of Hirt’s law (which operates when Baltic  $*-ū$ - derives from  $*uH$ ) or alternatively 3 AP (if the long vowel  $*-ū$ - was created by apophony). The original East Baltic intonation is probably preserved in Latv.  $pūri$ . As the Latvian intonation  $^$  indicates an expected length in the root and it corresponds exactly with Lith. 3 AP, and at the same time the Lithuanian nomi-

nals with original 3 AP are frequently replaced by forms with a mobile AP (cf. Illich-Svitych 1976, 65), the conclusion is obvious: East Baltic terms (Latv. *pûri*, Lith. *pûrai*) demonstrate an apophonic length of the root and correspond to Greek *πυρός* in the same way as Latv. *pēds* m. ‘footprint’, Lith. *pėdas* m. ‘track’ (3 AP) and OPruss. *pedan* ‘ploughshare’ (Illich-Svitych 1979: 56) are related to Gk. *πηδός* m. and *πηδόν* n. ‘oar, blade of the oar’ (< IE. *\*pēdó-*, a vr̥ddhi formation derived from the root *\*pēd-*). The apophonic forms in Sanskrit and Greek require no explanation. Both examples, discussed by Sowa, demonstrate evidently that my opinion on the Indo-European apophony and the laryngeal lengthening is correct, and my two reconstructions were convincingly established. Sowa’s objections are, in fact, imaginary.

On the same page the reviewer from Cracow reprimands me: “The same is the case with the equation of OIr. *tuirenn* and Arm. *c’orean* as if from *\*k̑poryanos* ‘wheat’ (pp. 99-110). The author does not seem to understand the proper idea of «thorn», ascribing it a relevant role in the question on the archaic character of the lexeme, which is completely wrong” (p. 199). It is obvious that my view as to the Brugmannian “interdental spirants” differs diametrically from Sowa’s opinion. He believes that “Thorn is rather an innovative phonetic variant, limited to certain contexts”, though these phonemes (IE. *\*p \*d \*dh*) never appear in the innovations (sic!), always in the archaic and basic vocabulary of the Indo-European language (see main IE. terms for ‘earth’, ‘yesterday’, ‘bear’, ‘eye’, ‘fish’ etc.) and demonstrate special, but regular reflexes in most Indo-European languages. Fifteen years ago I proposed my own solution on the basis of three probable comparisons (Witczak 1995: 225-226):

- (1) IE. *\*dh̑em-* ‘earth’ (cf. Gk. *χθών*, OInd. *kṣam-*, Lith. *žemė* ‘id.’) ~ Kartvelian *\*diqa* ‘clay’ ~ Afro-Asiatic *\*diq-* ‘clay, soil, mull’ < Nostratic *\*\*diqE* ‘earth, clay’ (Illich-Svitych 1971: 220, No. 69; Blažek 1989: 204);
- (2) IE. *\*dh̑h-uH-* ‘fish’ (cf. Gk. *ἰχθῶς*, Arm. *jukn*, Lith. *žuvis* ‘id.’) ~ Altaic *\*diga-* (or *\*žiga*) ‘a kind of fish’ ~ Afro-Asiatic *\*dḡ-* (*\*dḡ-*) ‘fish’ < Nostratic *\*\*digä* ‘fish’ (Illich-Svitych 1971: 219, No. 67; Blažek 1989: 204);
- (3) IE. *\*H₂rb-ko-* ‘bear’ (cf. Gk. *ἄρκτος*, OInd. *ṛkṣa*, OIr. *arth*, Hitt. *ḫartaggaš*) ~ Uralic *\*karči* ‘bear’ ~ Dravidian *\*kar(a)ṭi* ‘bear’ < Nostratic *\*\*ṣar(a)ṭi* ‘bear’ (Blažek 1989: 208; Witczak 1995: 225).

The parallelism of these three developments seems striking. Though the Nostratic dentals *\*\*t \*\*t \*\*d* yield normally *\*t \*d \*dh* in Indo-European, the three above examples demonstrate convincingly that the continuation of the same dentals in the position before the Nostratic vowel *\*i* is somewhat different (Nos. *\*\*t > \*t’ > IE. \*p*; Nos. *\*\*t > \*t’ > IE. \*ḍ*; Nos. *\*\*d > \*d’ > IE. \*dh*). Thus it is not impossible to conclude that after its straightforward Nostratic ancestor Indo-European inherited some palatalized variants of the dental stops, which were preserved as the so called “interdental spirants”. My opinion seems to be con-

firmed from the comparative point of view. Sowa's view on the "innovative" origin of the Brugmannian "thorns" remains with no justification. Thus the reader himself may estimate which of two positions is better founded. In this place I would like to emphasize that the dissimilarity of views is frequent in the scientific research, but it is inadmissible to say in a review that the opposite opinion is "completely wrong". In such cases it is preferable to use the phrase "I disagree with ...".

Sowa assumes (p. 199) that "Sometimes the author does not quote a form as he found it in his source". This objection is, of course, perverse. Sowa seems to see a complete chaos, when I introduced the same logical schema of presentation: individual types of cereals, accompanied by lexical material, morphological and semantic commentary, etymologies, extra-Indo-European parallels. But Sowa requires me to introduce another chaos by quoting numerous forms with different way of reconstructions, e.g. glottalic *\*p<sup>h</sup>ūr-* (Gamkrelidze-Ivanov), pseudo-laryngeal *\*puHro-* (Smoczyński), traditional *\*pūro-* (Pokorny) and so on. It is an unusual requirement. Prof. Leszek Bednarczuk as an official reviewer of my Ph.D. dissertation wrote something quite opposite, namely he believed that one uniform and homogenous Indo-European reconstruction should be introduced in all places of my work. I agreed with Prof. Bednarczuk and introduced one homogenous transcription.

The reviewer presents something more than strong criticism, when he writes (p. 199): "Witczak very often only refers to other scholars' proposals but without quoting them". Sowa's words are not accompanied by any examples and they remain an insult to my accuracy and honesty.

Sowa believes that Witczak's "way of analysis can hardly satisfy the needs of modern Indo-European linguistics" (p. 199). He explains nothing, giving the following reference in footnote 8: "These are general objections against the methodology of Witczak presented also in other works, cf. Bichlmeier 2003, 214". This reference should not be introduced by a solid reviewer. Bichlmeier discussed my one article written in Polish (Witczak 2000a). He indicated my alleged methodological mistakes on the basis of the short English résumé. Bichlmeier (2003: 214) wrongly understood that I connect etymologically the Indo-European term for 'meteorite iron' *\*pālaku-* (in my own reconstruction) with *\*peleku-* 'axe', whereas I indicated only a morphological similarity. Of course, IE. *\*peleku-* 'axe' (guaranteed by Skt. *paraśu-*, Iran. *\*parasu-/paraθu-*, Gk. *πέλεκυς*) cannot be explained from the view-point of the reconstructed Indo-European morphology, therefore it was assumed a long time ago as a borrowing (from Semitic?). However, Akkadian *pilakku* has a different semantics ('spindle', according to Falkner 1952: 26), thus there is no non-Indo-European form, which can be treated as a source form. The correspondences between San-

skrit, Iranian and Greek nouns are regular, thus most linguists believe that the word *\*pelekū-* ‘axe’ existed in the late phase of Indo-European.

Also IE. *\*pālaku-* ‘iron’, though attested in Mycenaean Greek *pa-ra-ku* (*u*-stem) ‘a kind of metal’, perhaps ‘meteorite iron’, Sanskrit *pāraśava-* (*o*-stem, orig. *u*-stem) ‘iron’, Old Persian *\*pār(a)θu-vat-* ‘steel’ (orig. ‘full of iron’), Lusitanian *palaga* f., dimin. *palacurna* f. ‘a golden lump’ (according to Pliny), with further possible cognates in Hittite *ḫapalkiš* (*i*-stem) ‘iron’ and Tocharian B *pilke* ‘copper’, seems very strange morphologically (see Witczak 2009). At the same time the double *a*-vocalism suggests a borrowing from some unknown source. However, the regular development of *\*k̂* in the Indo-European languages (see Skt. *ś*, Old Persian *θ*, Greek *κ*, Lusitanian *c/g*, Anatolian *k*, Tocharian *k̂*) documents that the name in question goes back to the late phase of Indo-European as well. It is obvious in any case that Myc. Gk. *pa-ra-ku* corresponds exactly to the Sanskrit and Persian forms, both morphologically and semantically.

Sowa stresses (p. 200) that “The book lacks an index, which in the case of such a study, with quotations from almost every language, is quite strange. The author excuses himself for this shortcoming («Author’s annotation» in the beginning), but this does not help at all the reader who wants to find individual words. What is more, some of the forms have been treated several times in different parts of the book. This gives the impression of chaos”. Of course, I know that the lexical index of the quoted forms is a useful aid, but Sowa’s words that “This chaos is the price for the chosen semantic principle” (p. 200) are completely misguided. It is obvious that C. D. Buck’s (1949) dictionary prepared according to the similar semantic principle, with repetitions of the same forms under different headings and with no index of quoted lexical forms would be chaotic, too. At any rate in Sowa’s opinion.

Sowa finds (p. 200) that “There are also inconsequences, as in the case of Greek *σῖτος* (p. 45). This occurs once in the company of OIndic *sītyam* ‘grain’ as a continuation of PIE *\*sīto-*, *\*sītyo-* ‘cereal, grain’, but strangely being interpreted as a «Pelasgian» element in Gk. On page 105 the form is treated as Minoan. Why then mentioned with an IE protoform? One could get the idea that Pelasgian, Minoan and IE are actually terms for the same concept”. This quotation demonstrates clearly that Sowa had to read my book inattentively. The Greek name in question appears as early as in Mycenaean (Gk. Myc. *si-to*) and Minoan times (according to Ruijgh 1970: 172-173, the CEREAL ideogram, which is attested both in Linear A and B, was derived from the Linear AB sign *si* or alternatively from two signs *si-to*). It is evident that the name *σῖτος* derives probably from some Minoan substrate, attested in the Linear A texts. The so-called “Pelasgian” language is commonly treated as an Indo-European substrate, which was introduced to the Continental and Insular Greece before the arrival of the Greeks (see Danko 2005; 2007). If the word *σῖτος* belonged to the Indo-



European vocabulary, as suggested by my own comparison with Sanskrit *sītyam* ‘grain’ (lex.), Khowar *siri* ‘barley’ and Kalasha *šiltī* ‘millet’ (Turner 1966: 767), then the preservation of the initial *s-* demonstrates non-Greek, but Indo-European origin of the word. The concept of “Pelasgian” seems hardly in disagreement with the suggested Minoan documentation. But the Indo-European language was spoken four or three thousands years earlier than “Pelasgian” or the Linear A language, spoken in Minoan times. My conclusion runs as follows: the word *σῖτος* derives from the Indo-European archetype *\*sītos*; it is attested as a CEREBAL ideogram in the “Minoan” Linear A texts and it seems to be borrowed by the Greeks from a foreign (“Pelasgian”) substrate. The difference can be described in the following way: Sowa excludes *a priori* the Indo-European character of Linear A texts, I find no strong arguments to deny or affirm it.

Sowa says as follows (p. 201): “Probably as typos should be classified statements such as e.g. «the existence of the Hurrian-Urartean substrate in Tocharian» (p. 38)”. In this place I indicated that there are traces of foreign substrates in numerous Indo-European languages (e.g. a Dravidian substrate in India, a Hurro-Urartian one in the tongue of the Tocharians and Armenians, an Elamite one among the Persians). Further I mentioned a possible substrate (or substrata) in Italy, in Scotland (e.g. Picts), in Spain (e.g. Basque), in Scandinavia. He does not reject different ideas, apart from the foreign substratum in Tocharian. However, the hypothesis on the possible non-Indo-European substratum in Tocharian is a long-standing problem (see e.g. Krause 1951: 185-203; Bednarczyk 1972; 1983; Pobożniak 1977; Thomas 1985: 147-154; Winters 1988; Winter 1989), to which I myself contributed seventeen years ago (Witczak 1993: 165). In December 1990 at the third Warsaw Conference on “Orient and Aegean. Language, Religion, Culture” I presented plentiful lexical evidence for the ancient contacts between the Tocharians and the Hurro-Urartean tribes. It is my own hypothesis, based on Henning’s interpretation of the Tocharians as the first Indo-Europeans in history (Henning 1978; Gamkrelidze, Ivanov 1989). My hypothesis on a possible Hurro-Urartean substrate in Tocharian has been recently discussed by V. Blažek and M. Schwarz (2007: 97-98; 2008: 59-60) with some critical (but not negative) remarks. Thus my hypothesis remains a scientific problem and not an illusion, as suggested by W. Sowa.

Sowa denies (p. 201) that the Polish term *archetyp* may refer to reconstructed stems, which “is not normally used in such a meaning” (at least in Cracow). But *archetyp* is commonly defined as ‘pierwowzór, prototyp’ in numerous Polish dictionaries, thus the sense is adequate and understandable. The word *archetyp* is frequently used by the historical-comparative linguists in Łódź, thus the observed difference may be regional.

Sowa states (p. 201): “Inexplicable are the author’s very strong statements concerning the presumed acceptance of some of the used theories. This applies

especially to the problem of Nostratic. Witczak speaks about increasing numbers of western scholars who are convinced of the correctness of this theory. In reality this is not the case at all. Witczak's Nostraticism is rather of an aprioristic, glottogonic nature". Unfortunately, Sowa totally distorts my opinion on the Nostratic theory and presents wrongly the use of the Nostratic parallels in my work. Contrary to Sowa's insinuations, nowhere in my book is the Nostratic theory treated as certain and completely credible. What is more, asking about the origin of the first farmers I presented the Nostratic theory (pp. 20-22) with great caution, calling it "controversial" (p. 21) and using the expressions like "It seems / Wydaje się" (p. 22). If these phrases are "very strong statements", then Sowa can safely say that Witczak is an uncompromising adherent of the Nostratic theory. My Nostraticism is neither "aprioristic" nor "glottogonic" (I do not believe that Nostratic was the first language on the Earth or the first tongue was invented by the neolithic farmers, as Sowa tacitly suggests). I accept this version of the Nostratic hypothesis, which is based on the comparisons and reconstructions given by Illich-Svitych. In E. P. Hamp's opinion, the Nostratic theory, created by Illich-Svitych and propagated by the Russian linguists, represents far-fetched comparative studies accompanied by solid phonological rules and scientific rigour. In his opinion, it cannot be proved positively today, but it cannot be rejected as false neither (cf. Hamp 1997). The Nostratic literature increases every year. To the best of my knowledge, 18 collected studies on the Nostratic themes were published during the last twenty five years in Germany and the U.S.A., thus the Nostratic studies are developing successfully and the number of scholars, who are interested in Nostraticism, is expanding, though Sowa denies it.

As I explained in the introduction (and briefly in the English summary), under the heading "Nostratic parallels" I quote "external lexical parallels taken from non-Indo-European languages together with a discussion as to whether we deal with a possible common Nostratic ancestry, as opposed to general-cultural terms adopted or borrowed, or with accidental correspondences" (p. 140). Thus in the case of extra-Indo-European cereal names I tried to distinguish: [1] loanwords in both directions (e.g. IE. > Semitic or Semitic > IE.), [2] similarities as to form or shape or meaning, listed especially in the older, non-Nostratic literature (I treated most of them as caused by chance), [3] possible or probable Nostratic equivalents (I accept ONLY 8 comparisons, partially as possible alternatives). It should be emphasized that a Nostratic derivation was taken into account mainly in such a situation, when no Indo-European etymology was acceptable, e.g. IE. **\*dhoHnaH<sub>2</sub>** f. (*ā*-stem) 'grain, corn' was many times explained as a Semitic borrowing **\*duhn-** ('Sorgum vulgare Pers.') or a source form for Semitic or a related form. Thus my Nostraticism is neither exaggeratedly optimistic nor "irrelevant", as was suggested by Sowa, who gave the following comment in the final part of his review (p. 202): "absolutely irrelevant Nostraticism could lead

the reader to the false idea that actually one may compare anything with anything, and that such is the right methodology". He suggests to the reader that Witczak compares "anything with anything", but the real situation is quite opposite.

Sowa adds that Witczak "quotes Semitic examples such as Arab. *hintat* 'wheat' (< *hanata* 'ripen') and Chad (Hausa) *gundu* 'sort of millet', without specifying what Hausa *g* and Semitic *ḥ* could have in common. Then again he does not explain how it is possible to connect Sem. *ḥ* with «the fluctuation between *g* and *sk*»" (p. 201). Sowa presents the situation in the false light and thus he exhibits my Nostraticism as if in the distorting mirror. He does not inform the reader that my position is wholly sceptical (sic!) and that all the extra-Indo-European parallels are classified as "ambiguous / dwuznacny" or "unclear / niejasny" (p. 97). The reviewer passed over in silence the fact that the comparison of the Semitic and Chadic forms was not made by Witczak, but quoted after Orel and Stolbova (1988: 80). What is more, Sowa had to know that in my opinion the discussed Hausa equivalent is far from being certain, as in footnote 1 I conscientiously stressed that the Semito-Chadic lexical connection was later rejected in a different work of the same authors (Orel, Stolbova 1995: 281, No. 1272), who preferred here a Semito-East Kshitic comparison (cf. Hadiya *hiṭe*, Kambatta *hiṭe*, Bambala *hanṭe* 'grass') instead of the Semito-Chadic one. I used the Hausa term (or alternatively the East Kshitic word for 'grass') to demonstrate the Afro-Asiatic status of the Semitic name for 'wheat' and at the same time to reject Ivanov's doubtful hypothesis that the Semitic *ḥant*- 'wheat' (cf. Arab. *hintat* 'id.') is a borrowing from Indo-Iranian *\*gantuma*- 'a kind of wheat'. Sowa agrees with this rejection, though he prefers an internal etymology of the Arabic term in question. It should be emphasized, however, that Sowa himself (and not Witczak) connects the Indo-European fluctuation *\*g* ~ *\*sk* with the Semitic phoneme *\*ḥ*.

The Indo-European fluctuation *\*g*- ~ *\*sk*- is not an unknown question. There are several appellatives of Indo-European origin, which show this variation in the initial position, e.g. numerous names for 'hornbeam (or oak)' come back to IE. *\*grābr̥* ~ *\*skrābr̥* (my reconstruction), cf. Bulg. *gābār*, dial. *gāber* & *gābar*, Maced. *gaber*, dial. *gabar*, SC. *grābar*, *gābar*, *grāb* 'hornbeam, Carpinus betulus', Sloven. *grāb*, *gāber*, dial. *grāber*; Slovak *hrab*, Cz. *habr*, OCz. *habr*, *hrabr* 'id.', Pol. *grab*, LSorb. *grab*; BRus. *hrab*, Ukr. *hrab*, Russ. *grab* (< PSl. *\*grabr̥* 'hornbeam'); Palaeo-Maced. *γράβιον* n. 'torch', orig. 'Eichenholz'; Umbrian *Grabouie* dat. sg. 'Eichengott'; Mod. Gk. Epirotic *γάρπος*, *γάρβος* m., *γάρβουνα* f. 'hornbeam'; OPrus. *scoberwis* 'hornbeam', Lith. *skróblas* m., *skroblà* f., *skrobl(i)ùs*, Latv. *skābardis*, *skābarde* f. 'hornbeam'; Alb *shkózë* f. 'hornbeam, Carpinus betulus' (< PAIb. *\*skābardyā* = Latv. *skābarde*). It is clear that the Slavic, Umbrian, Epiro-Macedonian forms go back to IE. *\*grābr̥*,

the Baltic and Albanian items to IE. *\*skrāby*. The alternation *\*grāby* ~ *\*skrāby* ‘hornbeam’ seems unexplainable from the view-point of traditional Indo-European phonology. However, the Nostratic hypothesis gives a possibility of explaining the problem of the initial alternation *\*g-* ~ *\*sk-*. It was suggested by Illich-Svitych and Nostratic linguists that Nostr. *\*k* yields regularly IE. *\*g*, whereas Nostr. *\*sk* is preserved with no change (IE. *\*sk*). Thus the Indo-European alternation *\*g-* ~ *\*sk-* may be caused by the *s*-movable effect, created as early as in Nostratic. Similar alternations *\*h-* ~ *\*sk-*, *\*p-* ~ *\*st-* and *\*f-* ~ *\*sp-*, which existed in Proto-Germanic, seem to result in Indo-European variants with and without the *s*-movable.

In Sowa’s opinion each distant comparison is doubtful and worthless. His position reminds me of the opinion of one group of Slavists, who believe that two Slavonic languages are mutually related to each other and derive from a Proto-Slavonic idiom, but at the same time they completely reject the Indo-European relationship. The same observation can be made about a group of Turkologists, who are able to abandon the Altaic theory with no distress. In opposition to W. Sowa I accept no extremism.

Sowa finishes his review with hope that his “suggestions would lead to gain a fresh view on this doubtlessly interesting material, and could help others analyse these terms, which play a very important role in every society”. I am afraid that no “new fresh view” was given by the rye-viewer.

A correct and fair review of my book on the Indo-European cereal names was given by Vaclav Blažek (2005). Sowa should learn to write unbiased reviews from the Czech linguist.

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