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On Latin-ProtoSlavic Language Contacts. Some Remarks on a Recent Paper by Salvatore Del Gaudio

In a recent paper written on the occasion of the 15th International Congress of Slavists, Salvatore Del Gaudio tackled the problem of linguistic relations (mainly at a lexical level) between Latin (later Romance) and Slavic. His article contributes some interesting data and ideas concerning the ethnic and historical context and provides a good opportunity for a reflection on the possibility of using different tools in research concerning the past of South-Eastern Europe. Unfortunately, the paper is much less solid, at least in my opinion, from the linguistics point of view.

For the sake of clarity, I'll divide my observations into five points:

1. *Author's introductory remarks and the state of the art*

Maybe the main cause of dissent lies in the first paragraphs. The author says (Del Gaudio 2013: 49):

Latin elements are primarily investigated in Church Slavonic textual sources, also as a consequence of a lack of written evidence in the early Slavic vernaculars. For this reason, in the literature, the early Latin borrowings are often associated with the appearance of Slavic writing and the formation of the distinct Slavic languages.

However ethno-linguistic research on ancient toponymy, along with archaeological evidence, has demonstrated the fallacy of such an assumption.

According to the author, then:

- a. until now, scholars have (at least “primarily”) investigated Latin borrowings in the first Slavic written documents, especially Old Church Slavonic¹ (hereinafter: OCS);
- b. research on ancient toponymy etc., along with archaeological evidence, can contribute new, useful data;
- c. we can then obtain entirely new results in the field of Latin (Protoromance?)-Slavic linguistic relations.

¹ To be precise: the author speaks of “Church Slavonic”.

That such disciplines as those mentioned in point b above can contribute useful data, is true². By contrast, however, it is not true that the problem of Romance-Slavic pre-documentary linguistic relations has not been investigated. There is a considerable amount of bibliography on this subject³. Pre-documentary relations were and are investigated with the usual tools of historical linguistics, first of all the comparative ones. To deny this possibility is tantamount to saying that pre-documentary linguistic stages cannot be investigated at all. Nor can archeological or toponymic data replace the comparative method (though they can complement it). Why then do we only know a relatively small number of borrowings of Romance origin that we can treat as Protoslavic⁴? Because linguistic comparison does not provide us with sufficient linguistic evidence to prove such deep, extended loan relations; probably, such deep relations simply did not exist. Moreover, I do not think it is methodologically correct to infer wide lexical relations⁵ from data that are insufficient and partly contradictory.

In addition, the following passage (Del Gaudio 2013: 50) is somewhat unclear to me:

For this reason an approach to this topic can only follow if we examine the problem according to two main directions: one that deals with the presumed “direct” oral contacts; the other that assumes the cultural mediation of other languages. Nonetheless an overlapping of the two approaches is conceivable, due to the practical difficulty of operating a clear-cut distinction between two aspects of the same issue. The oral contacts can be distinguished as: a) pre-historic contacts; b) historic contacts.

What we really find here (leaving aside what is said in the last line, certainly true, but rather self-evident) is an unjustified overlapping of two quite different problems: a) is the contact popular / oral or cultural / bookish?; b) is the contact direct or not? A popular but indirect contact (i.e. with mediation) is quite conceivable, and, for instance, “oral” Romance-Slavic contacts through Gothic mediation are attested in several cases.

2. *Periodization and the like*

Like every scholar wanting to shed light on Slavic pre-documentary linguistic development and the earliest phase of the documentation, Del Gaudio is concerned here with

² It is generally well known that the study of hydronymy has been used for a long time with reference to the problem of the first Slavic homeland; cf. Shevelov 1964: 20.

³ See for instance Meillet 1902: 179-187; Lehr-Splawiński 1929; Bernštejn 1961: 100; Shevelov 1964: 621 (and elsewhere); Boček 2010; Id. 2014: 343-357.

⁴ Let's also note that, notwithstanding the author's suggestion to the contrary, many among these borrowings come to Slavic through the mediation of other languages (especially Germanic, first of all Gothic); cf. Bernštejn 1961: 100.

⁵ Even less convincing is the attempt made by Mario Enrietti to demonstrate the strong influence of Protoromance and Old Romance dialects on Protoslavic (especially at the level of syllable structure). I hold firm to my criticism for the reasons I illustrated in Caldarelli 2012. On the lively debate between the Turin slavist and myself, see note 11.

certain problems involved in defining and periodizing Proto-Slavic. What he says on this subject is basically acceptable. Personally I would not say that “the Slavic peoples and their Proto-Slavic ancestors were present in Eastern Europe from at least the first millennium B.C.” (Del Gaudio 2013: 52). Moreover: I do not think it is possible to demonstrate anything about the Slavic peoples or their ancestors for such early periods; but naturally these problems remain open. Del Gaudio himself correctly acknowledges (*ibid.*: 51) that we are mainly dealing with “theories of a speculative nature”; he is right also when he points to the importance of the investigation of hydronymy, toponymy and the like and I also agree on the importance of research into ethnogenesis, at least as long as it is based on solid and verifiable data. What he says on the original homeland of the Slavs is acceptable too. It is highly likely that Slavs, starting from an area located on the middle Dnieper (maybe quite close to the Pripet marshes?), moved towards the central Danube (Holzer 2006)⁶ and further on to their later settlements. But on the subject of protolanguages, I have two objections: a) it is a rough and partly misleading simplification to say (Del Gaudio 2013: 52): “The concept of Proto-Slavic in fact is a theoretical abstraction, since unlike Latin, in relation to the Romance languages, Proto-Slavic was never recorded”. Although the latter statement on Proto-Slavic is undoubtedly true, here the author fails to take into account a very long and lively debate on the relations between literary Latin / Vulgar Latin / Proto-Romance⁷ etc.; b) (which for us is the main point): although it is true that “the concept of Proto-Slavic is in fact a theoretical abstraction” (Del Gaudio 2013: 52, words already quoted) and “the concept of Proto-System has been repeatedly questioned” (*ibid.*: 52, n. 10)⁸, the author seems close to completely denying the real value of the protolanguage as a scientific tool. Protolanguage is a way of expressing a complex set of genetic relations (and the only possibility to do so), notwithstanding the difficulty of a historical interpretation of the construct. However, it is a complete mistake to quote Georg Holzer among the adversaries of the heuristic possibilities of Proto-Slavic since he defends exactly the opposite opinion (Holzer 1996, 1998, 2006). Holzer firmly believes in the possibility of, and the need for, a rigorous reconstruction of protolanguages. Only after this technical operation is it worth investigating the historical frame in which we can include this construct (he accepts the concern with the problems of ethnogenesis and the like; he also indicates a very

⁶ I’d only be a little more cautious on the allegedly frequent mentions of the Danube “in ancient Slavic songs, folklore, rituals etc.” (Del Gaudio 2013: 51). *Dunaj* and the like often refer to something different from the huge river nowadays known as the Danube. Such is clearly the case in Igor’s tale; see Saronne’s remarks in Saronne 1998: 233.

⁷ See for instance Tagliavini 1982: 209-266 (operating entirely without the concept of “Proto-Romance”) and, for a totally different approach, Hall 1950.

⁸ But there is a huge difference between the situation in the field of IE linguistics and what the Slavist has to deal with. Where is a unitary IE reconstructed morphology? I think we only really have just fragments of a picture here. But a Proto-Slavic reconstructed morphology does exist, with a good degree of coherence.

precise chronological position for Proto-Slavic, i.e. around 600 A.D., cf. Holzer 1998: 57-58). There is certainly a relation between the older stages of Slavic linguistic development and later periods. But to speak of continuity without a clear vision of what is common and what is not, is misleading.

3. *Older stages of Slavic linguistic history*

I think this concept of continuity, which in my opinion is not very clear, may lead to a number of misunderstandings. Such is the case with the alleged isoglosses (V.V. Martynov) linking Italic and Slavic. This idea undoubtedly deserves a discussion, which cannot be held here. Anyway it couldn't be rejected *a priori*. Baltic can also be involved in the discussion, although it is far from clear which position it may occupy in this frame of linguistic relations. The author fails to quote a key study on the prehistoric relations between Latin and Slavic (Pohl 1977), but I think this is not the main problem. There is rather a fallacy in the author's reasoning, i.e. (again) the confusion between chronological phases that are quite different. If there effectively was solidarity between (the linguistic ancestors of) Italic (including Latin) and Slavic, say in the second millennium B.C. or at the beginning of the first millennium B.C. what has it to do with historical or socio-linguistics problems relating to the second century B.C.-second century A.D.? These are completely different questions. Even if Martynov and others are right in pointing to early (clearly prehistoric; say, in the first millennium B.C.) Italic-Slavic relations, chronologically prior to Irano-Slavic (Scythic; around the end of the first millennium B.C.), this in no way excludes the possibility of a wave of later Iranian influence (Sarmato-Alanic? first centuries A.D.) which may well, in turn, have had a chronological priority over a further historical wave of Latin (Romance) influences and eventually borrowings (last centuries of the Roman Empire / age immediately after its fall). Why then say (Del Gaudio 2013: 51): "He [i.e. Trubačev] dates the beginning of the first Slavic-Italic (Proto-Latin) contacts (and not Slavic-Iranian) to a very early period"? Finally, I don't understand why, as proof of Roman-Slavic military and diplomatic contacts, he quotes "a few conflicts ... between the Sarmats and the Roman legions of the Danube area" (*ibid.*: 55). Sarmato-Alans are a very important presence in the frame of this complex ethnic system. Clearly they had relations with the Slavs⁹, but equally clearly they were *not* Slavs.

4. *Linguistics, history, ethnogenesis. On the ethno-linguistic situation in South-Eastern Europe, 200 B.C.-200/300 A.D. and later*

Modern linguistic research on our problem starts with the contribution of Antoine Meillet, who, though partly outdated, still deserves to be read (Meillet 1902: 179-187), as does the whole of this great scholar's scientific work. Tadeusz Lehr-Splawiński (1929) tried

⁹ On the problem of the Iranian influences on the Slavs see particularly Reczek 1991 (esp. 10-11 on ethnical relations mentioned above).

to increase the number of possible Latin borrowings in Proto-Slavic. He also proposed an interesting criterion for the distinction between direct borrowings and words that could come to the Slavic area through Germanic mediation. His criterion is based on prosodic considerations; though not always fully reliable, this criterion has the merit of being grounded in the search for an objective solution in a field where the temptation of arbitrary assertions is always present¹⁰. However, nowadays some of Lehr's propositions seem untenable. The attempt to investigate Latin and/or Protoromance borrowings in Proto-Slavic was never abandoned by modern research. It goes without saying that there is a great variety of opinions. I've already mentioned the rather extreme position of Mario Enrietti, who admits not only large loan relations but even a deep Romance structural influence on Proto-Slavic¹¹. On the other hand, several much more cautious positions do exist. For instance, Luciano Rocchi in his well documented work (Rocchi 1990) investigates Romance borrowings in South Slavic languages and clearly states that there is a very limited number of Romance borrowings in Proto-Slavic. In the great etymological dictionary directed by the late O. Trubačev only a few among currently proposed borrowings are accepted as possibly belonging to the "praslavjanskij leksičeskij fond"¹². I think we can say that, although the question of the Romance lexical influence remains open¹³ (indeed, opinions on the relevance of Romance lexical influence on Slavic vary), it is safer to assume a rather limited number of borrowings. Leaving aside the quantitative question, I cannot agree in particular with Del Gaudio's opinion that there was a significant amount of early direct borrowings from Romance to Slavic (i.e. without the intermediate role of any other language such as Gothic). Firstly, there are too many phonological problems to think systematically of such a borrowing process (I repeat: an *early* and *direct* one). I'll quote here just the example of *ocītŭ*, "vinegar". If it has come from lat. *acētum* (naturally through a Protoromance stage),

¹⁰ Lehr's attempt could be usefully discussed today, after the progress undoubtedly made in the investigation of Slavic prosody by P. Garde, V.A. Dybo and others.

¹¹ Basing himself initially on Bonfante's suggestions, Enrietti has largely developed the idea of very intimate loan relations from Romance to Slavic. See Boček 2014: 348-353 for the debate between Enrietti and myself (the Czech linguist criticizes Enrietti's opinion, then points to a lack of sufficient theoretical frame concerning contact linguistics, in both Enrietti and myself).

¹² Trubačev seemed particularly restrictive in this regard (not without good reason, in my opinion). See however Trubačev, Žuravlev 1974-, x: 134-135 for **kolęda*, maybe the most certain direct borrowing. The Romance origin of South Slavic *kračun*, proposed by Miklosich and accepted e.g. by Rosetti 1986: 270, is now currently rejected. The lexeme (Proto-Slavic, also attested in Novgorod) has a quite different origin, see Fasmer 1986-1987, II: 336, s.v. *koročun*. It is worth remarking that even in the case of an old direct borrowing so widely accepted as **čeršina* (Trubačev, Žuravlev 1974-, IV: 78-79; see 77-78 for **čerša*), Trubačev does not exclude the hypothesis of a direct derivation from Indo-European in the Black Sea area (Georgiev-Hamp), although in the end he prefers the hypothesis of borrowing. Sometimes we have to face particularly complex problems, as for sr.-cr. *lōčika* < lat. *lactūca*, old but not Proto-Slavic according to Skok 1972: 312.

¹³ For the state of the art see Boček 2010; Id. 2014: 343-357.

we have to face two important problems, concerning vocalism (why Prsl *ĩ* < lat. *ē*?) and consonantism (why Prsl *c* [ts] < Lat. [k] – the primary phonetic value of the Latin grapheme <*c*>?) respectively. I'll restrict myself to briefly tackling the second issue. In the case of alleged direct borrowing from Romance to Slavic we can, according to the frame proposed by the author, only think of a contact in the Danube region. When? There is only one possible answer: after the first Slavic palatalisation (or else we'd have **očtŭ*) and before the second. But this reasoning implies that we would be dealing with a Romance form still displaying a fairly well preserved velar shortly before the date of the second palatalisation. Then we are forced to think of some Romance dialect similar to the ancestor of Dalmatian, spoken in a region where fairly profound contacts with Slavic could have taken place. It is not impossible *a priori*¹⁴, but it is, I think, highly improbable. Through these areal considerations we come to the second problem. Such speculations as Del Gaudio's, though certainly legitimate, presuppose a strong presence of the Romance element not far from Pannonia (better also *in* Pannonia). It seems to me that this is tantamount to accepting the theory of Romance continuity in Dacia, but this is another fairly controversial point¹⁵.

I've already recognized that, notwithstanding shortcomings on the linguistic side¹⁶, Del Gaudio's paper gives a fairly interesting picture of the situation in Southeast Europe (especially near the Black Sea) around 250 B.C.-250 A.D. and later. He's certainly right in saying that Latin was well known even on the Greek side of the Jireček line¹⁷. His comments on the toponymic elements of Latin (Romance) originating in the Black Sea region¹⁸ as well as on the traces of the Roman monetary system are interesting. Basing his assertions mainly on Magocsi 2010 and others (especially on Ukrainian historiography at home and abroad, but also quoting a rich historical bibliography in Russian and English), Del Gau-

¹⁴ The possibility of a later palatalization of velars in Balkan Romance is indeed taken into account by some scholars, cf. Tagliavini 1982: 244, 367-368. Yet it seems improbable that velars could still be pronounced entirely without palatalization at such a late date, cf. also Shevelov 1964: 251 (see also 633).

¹⁵ The continuity of Romance settlement in Dacia is lively defended by Sala 2009: 15-23. Usually a more cautious position prevails, cf. for instance Shevelov 1964: 159.

¹⁶ In a fairly long paper, it is surely normal to find some inaccuracies. However, here some fallacies in technical details are annoying enough, and they should be amended in a possible resumption of the research: a) Ancient Greek is graphically stressed monotonically, like Modern Greek since 1982; of course with no breathing marks at all; *k* is systematically replaced by *c*; b) Del Gaudio 2013: 55, n.18: what does the author mean by: "This root [i.e.: Common Slavic **vent-*] is *the superlative of the adjective* [my italics] 'tall'"; c) there is no slavist named Borys Wiesław; the person referred to is the well known Polish slavist Wiesław (first name) Boryś (family name), author of the *Słownik Etymologiczny Języka Polskiego* published in 2005 (and editor, with J. Rusek, of the volume here referred to under Holzer 1998).

¹⁷ This does not change the fact that the bulk of the Latin borrowings in Slavic is clearly mediated through Greek (on the contrary, it rather confirms it).

¹⁸ As to the toponyms with the lexeme *Trojan-/Trajan-*, Del Gaudio correctly acknowledges that there are also alternative explanations. Cf. Gieysztor 1982: 126-127.

dio underlines the strong Roman influence, contributing several interesting historical data. He is fairly convincing in asserting that, for a long time, the Black Sea was a kind of *mare nostrum*. But can we be sure that this influence had a great impact in terms of linguistic influence (especially of *immediate, direct* linguistic influence)? Inferences drawn from the supposed close correspondence between linguistic and ethnic (or political) data are always dubious¹⁹. Once again, I think we would need more ascertainable linguistic data to draw such conclusions. Moreover: a strong Roman influence seems to be well documented in the Black Sea region mainly between the third century B.C. and the third century A.D., but the Slavic element became really important in South-Eastern Europe around 500-600 A.D., i.e. in a world that had changed completely.²⁰ One thing is sure: late Greek borrowed a significant number of Latin words. The author is right in saying that “the most copious categories of loan-words belonged to the military, the government-administrative (state) and juridical spheres” (Del Gaudio 2013: 57). The rest is much more difficult to ascertain, as Del Gaudio himself correctly recognizes in the preceding lines: “The exact degree of Latin influence on the languages spoken in the former Eastern regions of the Roman Empire and in the border areas is difficult to estimate with any certainty”. Here below I shall try to explain my dissent concerning Del Gaudio’s treatment of some OCS words, partly of Latin origin, partly of different provenance.

5. *Was there a strong Latin lexical influence on OCS?*

In the final part of his paper, Del Gaudio extends his investigation to OCS. He attempts to demonstrate an alleged *continuity* Prsl.-OCS as to the Romance influence²¹. He quotes Pogorelov (Del Gaudio 2013: 64) on possible Latin syntactic influence on the OCS text of the Gospel. Then he underlines (*ibid.*: 65) a “great frequency of Latin words in Mark” [which] “has sometimes been used as an argument for a Roman provenance of the Gospel”. Pogorelov’s remarks are certainly interesting and important for the study of OCS syntax, but not as to OCS vocabulary. However, they have nothing to do with any possible conclusion on lexical influence at the level of spoken language; and the frequency of Latinisms in Mark’s Greek text has clearly no relation with Cyril and his way of working (see below).

In Del Gaudio 2013: 66-67 we find a table presenting eleven examples of words from the OCS version of Mark according to Zographensis. Leaving aside the real need to choose only Mark’s text, this table fails to prove the author’s point. The four columns contain

¹⁹ See for instance the history of the USA or Brazil, not to speak of the current situation in Ukraine.

²⁰ The well known theory of (Common) Slavic as *lingua franca* of the Avar empire (cf. Holzer 2006: 47-48) also points to a quite different situation.

²¹ After pleading for pre-documentary relations to be investigated, here the author discusses an OCS text much more extensively than he did before as to comparative data relevant for pre-documentary epoch. Naturally this would not matter if he came to solid conclusions, but in my opinion this is not the case.

respectively: the Latin form of the word, then Greek, Gothic and OCS. Here we find quite different situations. First I'll exclude *vino* from my considerations, a word already discussed by Lehr-Splawiński (1929: 708) but long recognized (cf. Fasmer 1986-1987, I: 316-317) as an old substratum word and/or as a typical *Wanderwort*, which makes its treatment as a borrowing too problematic to allow any certain conclusion. We then have five words (*pěņędzi*, *tepŭ*, *spōdomŭ*, *stŭklě*, *voinŭ*) the etymon of which has nothing to do with Latin or Greek²². As to the rest, we have at least three clear cases of Greek mediation (*kŭnsŭ*, *kęnŭturionŭ*, *legeonŭ*) where it is impossible in my opinion to see old direct borrowings caused by a situation of bilingualism and contacts in spoken language²³ (less clear is the situation for *prętorŭ*/*pretorŭ*, Hauptová 1966-1997, III: 254, 495, which, however, can hardly be considered outside this contact frame)²⁴.

What Del Gaudio (2013: 68) has to say about *kesari* / *ęsarŭ* is equally unconvincing. That *kesari* is a bookish loanword is certainly true. But as to *ęsarŭ*, the hypothesis of a direct Protoromance-Protoslavlic borrowing involves too many difficulties. For the initial consonant, we are up against the same phonetic difficulty already met in *ocitŭ*. Some think here of an intermediate Gothic stage (Shevelov 1964: 137); others propose different explanations. I would also like to point out that, if we assume that Slavic borrowed the continuation of **c(a)esar(em)* from Balkan Romance, it is rather difficult to explain why Romanian has *impărat*, continuing a quite different lexeme.

I think Del Gaudio's approach fails to take into account the historical position of Cyril and his way of working. Cyril's purpose was to render the whole of Greek lexicon into Slavic as effectively as possible (naturally he also faced difficulties concerning morphosyntax). Borrowings from Latin also belong to the Greek lexicon, but I see no trace of any particular treatment of this group. A word-for-word translation was inconceivable for Cyril, due to his excellent literary and linguistic culture; for the same reason, a passive rendering of foreign vocabulary was inconceivable too. When he could, he created new words merely on Slavic material, as in the case of *sŭtinikŭ* (Hauptová 1966-1997, IV: 356). Anyway, Cyril did not *replace* Latin *centurio* with the Slavic morpho-semantic calque *sŭtinikŭ*; he *translated* the Greek word *κεντουριων* (borrowed from Latin *centurio*) with a morpho-semantic calque based on genuine Slavic material, just as he often did. The problem of the coexistence of genuine Slavic words and Greek (including Greek-Latin) words is undoubtedly an

²² I could not find *tepŭ* in Hauptová 1966-1997 and Cejtin *et al.* 1994, and nor can any related form be found in Havlová *et al.* 1989- or Fasmer 1986-1987. *Zogr* for ξέστης in Mc 7,8 has *krŭčagŭ*, while *stŭklěnica* (not *stŭklě*) translates ποτήριον: cf. Cejtin *et al.* 1994: 296, 633. *Pěņędzi* is a loanword from Germanic, which probably dates back to Protoslavlic, cf. Havlová *et al.* 1989-, XI: 638-639. A derivation from lat. *pondus* for *spōdŭ* (not Instr. *spōdomŭ*), proposed by Miklosich, is now currently rejected, cf. Havlová *et al.* 1989-, XIV: 865-866. For *voinŭ* (once again, neither a Latin loanword, nor in any way connected with Latin), cf. Fasmer 1986-1987, I: 334-335, *s.v.* *voin*.

²³ Cf. Havlová *et al.* 1989-, V: 308, *s.v.* *kŭnŭsŭ*; 308, *s.v.* *kęnŭturionŭ*; 406, *s.v.* *leg'ęonŭ*.

²⁴ In fact Havlová *et al.* 1989-, XII: 702, *s.v.* *pretorŭ*, asserts once more Greek mediation.

old and difficult one. Jagić (1913: 299) already pointed out the complexity of the situation. Now, we know even better that an older calque translation was often replaced later by a loanword (this often happened in connection with the Preslav revision). However, in my opinion, the terms of the problem remain two, not three: Slavic and Greek.

Research into foreign elements in Old Slavic (in all its different forms, including reconstructed protolanguage) remains an essential part of Slavic studies. Ethnolinguistic and, generally speaking, historical considerations can certainly contribute useful data. In this sense, Del Gaudio's attempt is welcome; but I think it is also necessary to avoid chronological and methodological misunderstandings, and to be aware of the complexity of the problems we face.

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Abstract

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On Latin-ProtoSlavic Language Contacts. Some Remarks on a Recent Paper by Salvatore Del Gaudio

The Author discusses a recent paper by Del Gaudio concerning the problem of Latin and Protoromance borrowings in ProtoSlavic and Old Church Slavonic. He appreciates Del Gaudio's attempt to illustrate the historical frame of Latin/Romance – Slavic linguistic contact in a deeper and more insightful way, but criticizes several statements regarding mainly: a) certain methodological issues concerning ProtoSlavic and his reconstruction; b) linguistic relations between the Slavs and other ethno-linguistic groups within the Indo-European family; c) some controversial phonological questions involving lexical items borrowed by the Slavs; d) the treatment of eleven Old Church Slavonic lexical items allegedly directly borrowed from Latin.

Keywords

Romance Borrowings; ProtoSlavic; Old Church Slavonic.