

Raffaele Caldarelli

Open Syllable once again. Endogenous or Exogenous?

In his review of my article on the thorny open syllable¹ problem published in the fifth issue of “Studi Slavistici” (Caldarelli 2008), Mario Enrietti questioned some of my assertions (Enrietti 2009). Below I offer a few observations on his remarks, before going on to tackle one specific issue: whether or not the OS structure is the result of external influence?

Enrietti’s criticism is leveled in particular at three points:

- a) he claims that I am mistaken (Enrietti 2009: 309) in asserting that there is something surprising in the contrast between two tendencies [i.e. the OS and the fall of the jers] that appear oriented in opposite directions (“Indubbiamente pone un problema questo netto contrasto tra due tendenze che appaiono orientate in opposte direzioni” – Caldarelli 2008: 192);
- b) he maintains that I have “compressed into an ‘iron’ phonetic law” several OSS-related phenomena that originated at different times and in different ways (Enrietti 2009: 310);
- c) he asserts that I am wrong to deny the common Slavic nature of the *čě, žě*, etc. groups. “il C., con dubbi, li ascrive all’*Urkirchenslavisch* di Cirillo e Metodio [...], ma nega che essi siano panslavi; [...] le grafie *čě, žě* ecc. [...] non appaiono più così secondarie come egli [i.e. “il C.”] pensa” (Enrietti 2009: 311-312).

For the moment, I shall respond briefly, leaving a broader explanation of my ideas to a paper not yet written.

- a) I agree with Enrietti that there is no reason to consider a phonetic trend ‘everlasting’ (as the author expressed in the title “*Andere Zeiten andere Lautgesetze*”). If my formulation was not clear enough, I wish to underline that there is nothing unnatural in a gradual or even abrupt change of direction in linguistic evolution, or in a conflict between two trends. Nevertheless, as linguistic facts, they require an explanation;

¹ Henceforth: OS for “open syllable”, OSS for “open syllable structure”.

- b) Enrietti's assertion evidently stems from a misunderstanding. After quoting my words about excluding the possibility of a 'phonetic law' ("escludere decisamente la possibilità di pensare a una 'legge fonetica' " – Caldarelli 2008: 195), he insists (2009: 310) that the words "legge fonetica" are mine. This is not so. In fact, for the phenomena under discussion, I stated that we need to think about a certain direction or trend ("Di direzione o tendenza bisogna infatti parlare" – Caldarelli 2008: 195). I think it is clear, therefore, that I never had a phonetic law in mind;
- c) There is no doubt about the chronological priority of, e.g., *čě* (< **kē*) compared to *ča* or of *čě* (< **koi* < **kai*) compared to *ca*. It is also true that the whole Slavic territory must necessarily have experienced the *čě čě* stage. But my point is different. When arguing against the idea that the OS principle implies the need for synharmonism, I wrote as follows: 1. the consistency of Constantine's graphic usage (regularly *čě, čě* and so on) is probable, but not entirely certain, due to the lack of autographed texts (and of 9th century manuscripts); 2. anyway, we cannot exclude that in some areas, at the beginning of a written culture, the groups were already pronounced approximately [tʃa] [tʃa]. In this sense I believe we can safely say that graphic forms such as *ca dza ča ža* should not necessarily be considered secondary ("Altrettanto dubbio rimane quindi il carattere necessariamente secondario di tutte le grafie *ca dza ča ža*"). I have never thought that *ča* < **kē* could chronologically precede *čě* (which would evidently be absurd).

My conclusion is that Enrietti's general conception does not really clash with my own. When he writes that an overloaded vowel system becomes unstable in a certain linguistic situation (specifically with "long words", "parole lunghe"; cf. Enrietti 2009: 310) that leads to the loss of the ultra-short vowels *ʋ, ʌ*, he effectively agrees with some of my own assertions. In particular, with my observation that the OSS in Slavic proved to be unstable because it was neither compatible nor well integrated with the morphological structure, I refer also to cases of vowel loss within the context of a fairly complex word structure.

Going back to the problem of the endogenous vs. exogenous origin of the phenomenon, in Enrietti's words, it seems that we are left without an explanation unless we accept the hypothesis of external influence².

I do not agree. On the contrary, there is no need to resort to external influences to explain the rise of the Slavic OSS structure. First of all, OSS is neither an extraordinary nor a rare phenomenon, as proved by the literature that I myself (Caldarelli 2008: 197-198) and Enrietti (2009: 310) have quoted. Moreover, the evolution of Slavic in the period considered is in full accord with the preference laws for syllable structure described by Vennemann 1988. The CV structure is potentially the most natural one. Certainly, that is

² "Il Caldarelli è contrario alla mia tesi (Enrietti 1982) che la sillaba aperta sia sorta in slavo per influsso del latino di Dacia e promette di darne una spiegazione diversa; aspetto con curiosità di leggerla" (Enrietti 2009: 309, n. 1).

true from a relative, and not from an absolute viewpoint: there are several “preference laws” that may conflict with each other. As Vennemann (1988: 65) affirms:

Even the so-called ideal syllable, CV, with a strong consonant followed by a vowel, is optimal only in isolation; combined with other ideal syllables, it automatically becomes bad in certain positions, viz. by the very strength of its onset, and as such, it becomes subject to local improvements, e.g. weakening, possibly all the way to zero. Evidently, there is no optimal syllable structure, but only syllable structure for which some parameters are optimized.

Several factors may well hinder the trend towards a CV structure, and often they do so. Take, for example, the interplay of phonological and morphological factors, which are not really any different from Enrietti’s (see my considerations above on morphological structure and “long words”)³.

This complex interplay of factors indeed shortened the period in which the OSS dominated. So Enrietti is right in saying that it was by pure chance that the OSS dominated precisely in the years when Constantine and Methodius were active, but I consider it impossible to agree with the following statement that the OSS should be seen as an unusual phenomenon.

Theoretical and typological reasons are, in themselves, sufficient to point to a fully endogenous rise of the OSS as the most probable. There are other important facts which make this hypothesis more acceptable than the other, and undermine even more consistently the assumption of any Romance influence:

- a) the Slavic area had long been moving towards the OSS;
- b) what we know about lexical borrowings from “Romania” to “Slavia” does not justify Enrietti’s assumption;
- c) despite numerous attempts to prove otherwise, we have more and more reasons to reject the possibility of a social and political framework that could justify the hypothesis of a massive Romance influence on syllable structure.

Let’s look briefly at the three aforementioned issues:

- a) There is no doubt that the OSS took a long time to develop in the Slavic area and that it was never fully implemented, as Enrietti (2009: 309) says⁴. However, I cannot agree

³ In his further considerations on the OSS and morphological structure, concerning the rise of e.g. *rabō* from different forms, i.e. nom. sg., acc. sg., gen. pl., Enrietti (2009: 311) is right. However, his statements do nothing to change my point about the weak integration between phonology and morphology. For brevity’s sake, I had no interest in a more detailed account of the question.

⁴ “La formazione della sillaba aperta è un lungo processo, che si estende dal VI al IX secolo, e che non è giunta mai a completo compimento...”

with the date he suggests for the beginning of the phenomenon which, in my opinion, began considerably earlier. Let's observe the question from a quantitative point of view. According to the Slavic phonetic evolution described by Shevelov (1964: 633-634), out of 40 sound changes, 16 go towards the OSS⁵. We have no reason to doubt that a linguistic area such as the Slavic one, with so many speakers, was already moving towards the OSS⁶. Consequently, we cannot ignore the crucial question of why the influence of Dacian Latin should have been so decisive, if the OSS was the result of such a long and complex process. Evidence suggests that the process did not begin as late as the 6th century A.D. However, even if we hypothetically assume this chronological frame, this would not be a decisive argument. There are no grounds for doubting that Slavic, with its numerous speakers, was already characterized by the OSS when analogous (and far less consistent and significant) phenomena started in the Romance domain;

- b) I do not think that the stock of borrowings from Dacian Latin to Slavic is particularly significant. Enrietti's evaluation of the number of borrowings (1982: 63-64) refers mainly to Lehr-Splawiński 1929, but he admits himself that some of the Polish scholar's etymological proposals are uncertain. Moreover, there are serious chronological difficulties. As Rocchi (1990: 33) says, there is only a limited number (at most about fifty) of borrowings of Latin origin in Slavic, which may be really old, i.e. dating back a time before the 10th-11th centuries⁷.

Besides the issue of determining the origin of these Romanisms more precisely, there are terminological difficulties when Enrietti uses different terms, such as "Daco-Latin" in some cases and "Rumanian phenomena" in others, though these are probably not the most important problems⁸. My main objection is that we would need far more evidence (and surely a much larger stock of borrowings) to be able to prove beyond doubt the existence of any such strong and decisive influence. For the case in question, such parallels as the influence of Oscan-Umbrian on the Latin vowel system are not decisive (cf. e.g. Enrietti 1998-99: 91). From a structural point of view, Oscan-Umbrian did not influence the Latin syllable structure. Among other things, this was not possible because the syllable structure did not show any significant differences between the two languages. In the Romance-Slavic hypothesis, we would

⁵ There are also two sound changes going in opposite directions: no. 4 (split of syllabic sonants) and naturally the loss of *jers*.

⁶ See at least several processes of *coda* lightening and/or transformation of consonant clusters, occurring very early (sound changes 6, 8, 11-14 in Shevelov 1964: 633).

⁷ "[...] i latinismi slavi che possiamo far risalire con sicurezza ad un'epoca anteriore al X-XI sec. sono al massimo una cinquantina". Rocchi asserts too that these (All-Slavic) words of Romance origin came to the Slavs through a Germanic mediation. I won't go into this problem here.

⁸ I will not try here to deal with the problem of the birthplace of Proto-Rumanian (probably on the right bank of the Danube or even further south): cf. Banfi 1985: 142; Tagliavini 1982: 373-374.

be concerned with a completely different situation: strong influence exerted in the field of syllable structure, which I feel would be rather unique between two Indo-European subfamilies in Europe⁹.

- c) Since 1982, the year of Enrietti's most relevant work on the OSS as an outcome of Romance influence, we have gained more information about the demographic and social conditions of the Lower Danube region around the 6th century¹⁰. What we now know contributes nothing to confirm the hypothesis of a strong Romance (Daco-Latin) influence. The picture facing us testifies to a multicultural and multiethnic society which was defining its own identity thanks to a common language. Common Slavic was the lingua franca of this rather complex society and probably secondarily the lingua franca of the Avar empire (Holzer 2006: 46-47). I do not see how this socio-ethnic background can justify the Romance-Slavic hypothesis. Enrietti's words about alleged "Roman" prestige in this context do not appear to be well founded.

We should not overlook another problem. Holzer demonstrates with very good arguments and impressive technical mastery the existence of a uniform Common Slavic language at a fairly precise time, around 600 A.D.¹¹ If this is correct, it implies that the OS issue no longer concerns Common Slavic, but a post-common period. This does not explain what happened on Slavic ground between 600 A.D. and, say, 1000 A.D.¹² There are still many common phenomena throughout the Slavic space. How are we to explain this? Enrietti (1982: 68) gives great importance to mutual influences (extra-Slavic, in our case). I would suggest taking into due account parallel developments, which may apply to a large number of cases¹³. Many problems remain to be solved. Hopefully, further discussion will be useful.

Let me go back to Enrietti's arguments in favour of the Romance-Slavic hypothesis. In his opinion the strongest argument in favour of the Romance-Slavic hypothesis is the territorial distribution of forms. I think, however, that it is precisely the territorial data that significantly undermine his point. I will focus here on five phenomena which, according to Enrietti, give evidence of the Romance origin of the OSS and are specifically concentrated in the SE Slavic area: a) nasal vowels; b) monophthongization of *-j*, *-w* diphthongs;

⁹ I'd like to add that the role of syllable structure in the frame of phonological interference is still relatively little known and further investigation on this subject is necessary.

¹⁰ See the bibliographical data in Holzer 2006: 30-32.

¹¹ Among the writings Holzer has devoted to his reconstruction of Common Slavic see for instance Holzer 1998 and Holzer 2003.

¹² The problem of the lower limit of the common phase is very interesting and several solutions are possible. Enrietti prefers a border about 1200. There is sure a broad space left here to discussion.

¹³ I believe (Caldarelli 2008: 195) that an approach in terms of multiple, parallel *drift* (Sapir 1921, ch. 7-8) could explain some important phenomena of the period considered.

c) evolution of groups with liquids (*tart*) in order to avoid closed syllables; d) evolution of the earlier groups *ir ur* in *rb r̄b*; e) evolution of *tl dl* groups. In general, Enrietti's explanations are not convincing. For instance, it is hard to understand the very complicated (and quite illogical) mechanism that would explain the interference: we are told that the trend towards the OSS, once started among "Rumanians", was continued and strengthened by Slavs¹⁴. An analysis of the five phenomena mentioned above will show that the hypothetical irradiation centre located south east of the Slavic area is not convincing.

- a) Nasal vowels are rather less characteristic in the Romance area than in the Slavic one, which would imply that it is not their place of origin. Moreover, Enrietti (1982: 72-75) gives no indication about a possible Romance model for vowel nasalization. Neither Rumanian (cf. Lausberg 1979: 243, 248) nor any Romance dialect of the Balkans could offer such a basis for nasal vowels. Besides that, the clear presence of this feature in precisely the opposite area, i.e. the North West (Lechitic), does nothing to support Enrietti's theory. I am aware (Caldarelli 2008: 194) that Lechitic, and particularly Polish nasals were always liable to a diphonemic interpretation (cf. Birnbaum 1979: 127-129) but this does not justify the Romance-Slavic interpretation. Vaillant had already made a sound evaluation of the situation (1950: 150):

Ces traitements divers des langues slaves attestent que les diphtongues nasales de la fin du slave commun étaient des sons complexes et instables, et dont la prononciation pouvait varier selon les dialectes. Leur transformation en diphtongues ou voyelles orales dans la plupart des langues a eu lieu vers le X^e siècle, antérieurement aux plus anciens textes, et l'on n'en suit pas le détail.

It is therefore clear that we cannot give an interpretation in terms of a single decisive influence (especially in view of the fact that there is no factual element proving such a unilateral influence). If we are to attempt a territorial interpretation, the facts suggest that both the NW and SE areas, with the nasal vowels represented in Bulgaro-Macedonian dialects¹⁵, are probably remnants of an all-Slavic (surely endogenous) nasal vowel feature (that occurred in several ways throughout the Slavic territory).

- b) *-j, -w* diphthongs. According to Enrietti (1982:67), the monophthongization of *-j, -w* diphthongs is the only phenomenon present all over the Slavic area (except the fall of the final *-s, -x*). This situation allegedly exists because the *-j, -w* monophthongization was taken very early from the Romance area and affected the entire Slavic space. In my opinion this reconstruction is not reliable. First, the idea that the Slavs took

¹⁴ Enrietti (1982: 67). See also (p. 67, n. 7) Reichenkron's equally complicated hypothesis, which suggests the existence of a "Dakoslawisch" having properties which remain unclear to me.

¹⁵ Cf. data from Suho near Salonica, the Macedonian enclave in Albany near Koritsa and the Bulgarian dialect from Transylvania which died out in the 19th century (Vaillant 1950: 152).

over an already accomplished Romance monophthongization conflicts with several difficulties. Rumanian has a number of forms (surely not of a scholarly type) without monophthongization (cf. Lausberg 1979: 249-250, Tagliavini 1982: 241, 331-332). Slavic (namely, Croatian) forms such as *Lovret*, *ovrata* (Stieber 1989: 24) can be explained much better if we assume that the Slavic process of monophthongization was more advanced in the borrowing area than in the Romance one¹⁶. As to chronology, maybe this is not the main difficulty, but if Shevelov's dating is correct (6th-7th century) and we must assume Romance influence, the process of its spreading throughout the Slavic world after the Slavs reached the Balkan Peninsula would appear far too fast.

- c) *Tart*-groups¹⁷. The well-known fact that remnants of closed syllable structures were located mainly in the North West might suggest that their transformation started spreading from the South East. However, it is no less important to remember that such remnants can be found in the south east as well: forms like *baltina*, *zaltarinŭ* or *maldičie* are well represented in John the Exarch (van Wijk 1931: 55-56; Vaillant 1950: 161)¹⁸.
- d) The case of the reflexes from IE syllabic liquids is interesting, but an interpretation in terms of their having spread from the south east is unlikely. Enrietti has repeatedly (and correctly) stressed that closed syllable sequences are to be found in the Eastern as well as in the North-Western areas¹⁹. It should not be forgotten, however, that clear examples of closed structures are known all over the south east (Duma 1990). Moreover, as also already mentioned for nasal vowels, it is particularly complicated to specify the phonemic status of the forms in every single case. It is more likely that OSS was unstable throughout Eastern Slavia, certainly including the Eastern area in the traditional sense, but partially also the South East.
- e) As to the *tl*, *dl* clusters, one could think there is at least one case of geographic distribution favourable to the Romance-Slavic hypothesis. Unfortunately, once more the parallel between Romance and Slavic facts leads to serious contradictions. Enrietti explains the survival of *tl*, *dl* in Polish, Czech and elsewhere mainly in the NW area with a syllabic re-structuration which Slavic would have taken from the Romance

¹⁶ Otherwise Enrietti (1982: 69, n. 10). Admitting that he is right, why not simply **Loreč* (**Loret*), **orata* and the like?

¹⁷ The *art*-cases will be left aside here.

¹⁸ Some of these forms quoted by van Wijk can imply various problems. Some of them show secondary vowels opening some syllables, see e.g. *mal'dičie* or *pal'tŭ*, *palitŭ* alongside *paltŭ* (the apostrophe renders here the *pajerak* or *paerčik*). I think it does not change a situation too complex to be interpreted in terms of a spread to the NW (what about the *polnoglasie* in the East Slavic?). Enrietti (1982: 79) quotes such forms without metathesis, but maintains they have no territorial relevance in the sense taken into account here. I cannot agree with him.

¹⁹ I share Enrietti's doubts as to the assertions of Shevelov referred to in Enrietti (1982: 80, n. 33a).

area. Several objections contradict this hypothesis (see Furdal 1961: 30-31): 1. According to Enrietti, Slavic took syllabic re-structuration from Romance dialects, but the diffusion wave from SE stopped before it reached the (N)W area which preserved *tl*, *dl*. Once again, the geographic distribution of the clusters under examination is by no mean sure, and it does not fit in the frame proposed by Enrietti. Furdal (1961: 28) refers to a very detailed study of Tesnière (1933)²⁰, that proves beyond any doubt the complexity of the facts concerning the clusters *tl*, *dl*: they underwent a different evolution in different phonetic contexts, and their geographic distribution is very complicated (see for instance the situation of the Slovene and Slovak areas in Tesnière 1933: 87). Even a Rumanian form *močirle* from SE Slavic *močidlo* is attested (Tesnière 1933: 83). 2. What is more, here again Enrietti's approach is absolutely atomistic and does not take into account that we are in front of three roughly coinciding isoglosses: *tl/dl*, *kv/gv*, *š < x'* (2nd palatalization of the velars). For the second and the third phenomenon none would think of a Romance influence going in SE-NW direction. We have a bundle of isoglosses created by a dialectal situation about which we know very little and which probably has completely other grounds. 3. It's undeniable that the Balkan Romance area preserved much more consonant clusters than Slavic²¹. So we would be forced once more to assume that a "weak" Romance trend was "strengthened" on Slavic ground. In my opinion this must be considered as highly improbable.

In conclusion, I would suggest that neither general principles nor empirical evidence help to support the Romance-Slavic hypothesis as an explanation of the origin and development of the OSS. There is no doubt that interferences between Slavic-speaking and Romance-speaking peoples existed, but I am convinced that there is no clear evidence to support the idea that Romance languages exerted a strong or long-lasting influence on the formation of Slavic languages. As things stand at the moment, I agree that OSS was a fairly long-lasting feature in Slavic and that it underwent various historical changes. However, I cannot but consider the OSS as a Slavic linguistic feature. Further investigation on interferences will hopefully shed more light on the question, which still remains open.

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²⁰ Enrietti quotes Tesnière's article just once (1982: 90): I think it deserves more attention.

²¹ Note also that the shift of syllabic border referred to by Enrietti 1982: 67 is impossible in a very large amount of cases, cf. e.g. Lausberg 1979: 315 (*iarbă*), 317 (*înalt*), etc.

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Abstract

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Open Syllable Once Again. Endogenous or Exogenous?

After a brief reply to some critical remarks raised by Mario Enrietti in a paper published in “Studi Slavistici”, VI (2009), the Author tries to explain why Enrietti’s theory of an exogenous, namely Romance origin of the Slavic open syllable structure should be rejected.

Keywords

Common Slavic; Phonology; Syllable.