

**COMEDY OF ERRORS OR INEXORABLE ADVANCE?
EXPLORING THE DYSFUNCTIONALITY OF THE DEBATE
ABOUT THE PROGRESSIVE PALATALIZATION OF SLAVIC**

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1. Embarrassment

Bring up the progressive palatalization in a company of seasoned slavists and an embarrassed silence is bound to ensue. Yet although the progressive palatalization is more complex than the average Slavic sound law, a viable solution has been around for generations, long enough for the news to have spread to the remotest corners. There would seem to be no objective reason for the pervasive feeling that something is wrong, let alone for the avalanche of publications that have continued muddying the waters since the problem was basically clarified on the eve of the outbreak of the First World War.

It is the purpose of this contribution to tentatively explore some of the historical sources of the confusion. The stress is on “tentatively” and “some”, for whereas I am convinced that the progressive palatalization can be grasped by ordinary human beings, the labyrinthine complexity of the scholarly literature puts anything approaching exhaustive comprehension beyond the powers of a mere mortal.

Since most of the problems that still beset the discussion are older than is generally realized, this article will focus on the earliest phases of the debate, up to and including Roman Jakobson’s *Remarques sur l’évolution phonologique du russe* of 1929 and its aftermath. Even within the early period, some issues have deliberately been left out of consideration, in particular everything to do with absolute chronology. A coherent evaluation of the literature that came after the *Remarques* would require a separate effort.¹

¹ This contribution continues Vermeer (2000); issues dealt with there will not be taken up here unless that is necessary for the continuity of the story. For an introductory overview of ideas having to do with the progressive palatalization see Vermeer (forthc.). I am very grateful to the editors of *Studies in Slavic and General Linguistics* for their criticism.

2. The stage is set

As is well known, it was in the mid seventies of the nineteenth century that the requirement that sound laws do not have exceptions was explicitly formulated. Even before then, linguists had been keen on finding regularities, but the presence of exceptions had not been regarded as traumatic. Spectacular successes like Verner's law now showed that exceptions may turn out to harbour underlying regularities of great subtlety.

The chase was on. Linguists started scouring the landscape for hidden regularities that might immortalize their name. For all they knew, their very own sound law could be waiting just around the corner. The pace of discovery quickened. Matters of priority came to occupy more attention than at any time before or since.

2.1 Начнем *ab ovo*

In 1878 the third edition of Franz Miklosich's *Altslovenische Lautlehre* appeared. The book is an exhaustive inventory of Old Church Slavonic facts relevant to the historical phonology of Proto-Slavic. It is superbly structured and beautifully written and like everything Miklosich did it invites the use of superlatives.

More to the point, it offered a promising quarry for eager young linguists because Miklosich, who belonged to the older generation, did not operate with neogrammarian sound laws. His treatment of the Slavic palatalizations is a case in point. He recognizes that there are two types of palatalized reflexes of the velar obstruents *k/g/x*, in modern terms:

- (1) *č, ž, š*
- (2) *c, dz (> z), s/š*

Miklosich assumes that both types of reflexes were triggered by elements following the velar, either front vowels or **j*. The two distinct types are accounted for by positing two distinct chronological layers, with *c/z/s* held to be more recent than *č/ž/š* (257), in the sense that the presence of *c/z/s* in a morpheme proves that it must have arisen at a relatively recent stage, e.g. about the suffix *-bce*: “*c* beweist die Jugend dieser formen” (248).

Since this conception was problematic from a neogrammarian point of view, there was work to be done. An obvious first step was to examine the front vowels involved in the palatalizations and see if they could be made responsible for the difference. Several scholars discovered an important connection: if the front vowel is the reflex of the diphthong **oi* we only find reflexes of the second type

(*c/z/s*), if it is not, reflexes of the first type (*č, ž, š*). The first to publish this insight appears to have been Aleksandr Afanas'evič Potebnja (1879: 358).²

Potebnja's discovery showed that the Slavic palatalizations can only be understood in terms of phonological systems that preceded the one attested in the earliest texts. It presupposes the reality of the following chronological stages:

1. The stage at which the first palatalization took place. At this stage the reflex of the diphthong **oi* was still such that it could not palatalize a preceding velar because it had not yet developed into a front vowel.
2. The stage at which the reflex of **oi* changed so that it became capable of palatalizing a preceding velar. It is traditional to identify this stage with the monophthongization that caused the diphthong to be reflected as *ě* and *i* in the attested material.³
3. The stage at which the second regressive palatalization took place.
4. The beginning of the Old Church Slavonic tradition.

Ever since, this chronology has constituted the backbone of any attempt to come to grips with the prehistory of Slavic phonology.

Independently from Potebnja, the link between the second layer of reflexes and the former presence of diphthongs was also noticed by Hermann Collitz. He announced the discovery in a footnote to an article about an entirely different subject (1879: 203n), promising to bring proof elsewhere. On the very next page, however, he admitted to having read Potebnja and he never returned to the point.

What Collitz did and how he did it is typical of the heady times he found himself in: he claimed a discovery before he was in a position to properly substantiate it, merely as a warning for others to keep out; for that purpose a single sentence in a footnote sufficed. Such a high-powered contemporary as Vatroslav Jagić respected Collitz's footnote as a legitimate expression of a claim "zum Schutz seines geistlichen Eigenthums", which he (Jagić) did not feel free even to paraphrase (1879: 728). As we shall see, scholars were perfectly willing to wait for many years to respect such claims.

² I say "appears" because the way Potebnja introduces the point conveys the suggestion that it was widely known already, which it may well have been, if only in the lecture halls of Russia. Peterson (1946: 29) reports that the insight was first formulated by Fortunatov in lectures, but he is not specific enough about the time element for us to be sure that Fortunatov was in fact earlier than Potebnja.

³ The standard interpretation is slightly simplistic in that the palatalization would seem to show no more than that the first element of **oi* had been fronted, so that it may well have preceded the monophthongization, let alone the merger with the reflex of **ě* (cf. Meillet 1900: 8). As far as I can see, the point makes no difference to the issues discussed in the present contribution.

2.2 Baudouin de Courtenay's embargo

Potebnja's solution leaves a residue consisting of examples of reflexes of the second type which cannot be attributed to a following front vowel that reflects **oi*, e.g. *отъсь*, *lice*, *овьса*, *dvidzati*. This residue constitutes the set of examples Potebnja's law fails to account for and that eventually was to be explained in terms of a progressive palatalization.

As we have seen, early investigators assumed the presence of a **j* following the velar to explain those cases in which reflexes of the second type are not followed by a front vowel, e.g. *овьса*, Gsg *отъса*, *lica*. This was a natural assumption because nouns like *овьса*, *отъсь*, and *lice* have the same sets of endings as nouns in which the ending is preceded by a consonant originally followed by a **j*, i.e. endings showing the workings of umlaut. Natural or not, now that neogrammarian methodology had arrived this assumption became problematic, because if it is true that sequences of velars plus **j* yield modified reflexes of the first type, they cannot without further ado be assumed to have yielded reflexes of the second type, too. Potebnja tried to save the situation by positing the previous existence of two different types of **j*, the second of which palatalized preceding velars to *c/z/s*. However, since this idea was plainly ad hoc, it was universally rejected (or more commonly ignored), so that the problem remained open.

Given the spirit of the times, the odds are that scholars eagerly examined Potebnja's residue. They cannot have failed to see that in each case the *c/z/s* is preceded by a front vowel (nearly always **i*, **b* or **e*, never **e*, only very occasionally **ě*), so that it made sense to assume that palatalization must have been triggered by a preceding **i*, **b* or **e*. Yet for the time being, nobody published this insight, undoubtedly because they must also have seen that counterexamples are way too numerous to ignore: alongside R. *licó* there is *lik* (Gsg *lika*), alongside *učenica* there is *učeník* (Gsg *učeníká*), alongside *knjaz'* (Gsg *knjázja*) there is *knjagínja*. So if indeed the palatalization seen in examples such as *licó* is due to the presence of a front vowel preceding the velar affected (and that conclusion is inescapable), mechanisms have to be devised to explain the counterexamples.

Not long after Potebnja's article appeared, Jan Baudouin de Courtenay started spreading the rumour that he had found a solution, obviously with the purpose of preventing others from publishing first. This way of putting it may smack of malicious gossip, but fortunately he tells us all about it himself in one of the articles he eventually published about the subject (1894: 45 with footnote). The principal features of his solution were published once or twice by his pupil Mikołaj Kruszewski (e.g. 1887: 182n; the publication, by the way, was edited by Baudouin de Courtenay himself).

It took Baudouin nearly fifteen years to get round to publishing his solution. During that time, his colleagues' hands were tied. Everybody respected the embargo. Or nearly everybody. In two publications that appeared in the late eighties,

Jagić (1887: 192-193) and Sobolevskij (1889: 30-33 = 1891: 114-117) wrote that the palatalized velars of Potebnja's residue are due to a preceding front vowel. Jagić provided some solid argumentation and Sobolevskij a systematic collection of examples. What made them do this?

It turns out that neither scholar had much of a choice. Jagić's article is a lengthy review of Karl Brugmann's *Grundriss* (1886) intended to show that Brugmann's grasp of Slavic left a lot to be desired; Sobolevskij's is an introductory overview of the phonological history of Proto-Slavic. In both cases it would have been out of the question just to report Miklosich's understanding of the problem as if it had not long ago been superseded *de facto* if not *de jure*. However, both authors scrupulously limited the discussion to the observation that the palatalized velars of Potebnja's residue must have been due to a preceding front vowel; anybody could see that by glancing at Miklosich's collection of data in the light of Potebnja's law. They carefully refrained from revealing more about the problem of the counterexamples than that it existed: "Смягчение гортанных 2-го вида далеко не было обязательнымъ для всѣхъ словъ обще-славянскаго языка, въ которыхъ передъ гортаннымъ находился мягкій согласный" (Sobolevskij 1889: 32 = 1891: 116). In other words: they went out of their way to leave Baudouin de Courtenay space to discuss the real problem posed by the progressive palatalization.⁴

2.3 Baudouin speaks out

Eventually, Baudouin de Courtenay published the promised solution twice, first in brief in an article purporting to reproduce a letter he wrote in January 1893 to biologist Fedor Petrovič Keppen (1833-1908), and then in greater detail (but omitting or condensing several points) in *Indogermanische Forschungen* (1894).

Baudouin assumes that palatalization was triggered by, in his words, "palatal sonants" (1893: 15), by which he means high front vowels (cf. 1894: 47). At the time, the predecessor of ь must still have been a high vowel (short *i̯). The element that ultimately was to yield ɛ̯ was assumed to have consisted of a syllabic nasal consonant accompanied by an [i]-like vowel written approximately as *ɲi* (1893: 12, 15, 17n, 1894: 47, 50). The only specific example Baudouin gives is the Germanic borrowing **kūnɲig-*, later *kъnędzъ* (1893: 17n).

Although Baudouin's understanding of the nasal vowel differs from what was to become normal afterwards, his formulation goes on making phonetic sense if it is modernized: palatalization is assumed to have been triggered by the predecessor of *ɛ̯ at a stage when it was or contained a high front vowel, e.g. **in* or **i̯*. It was the high front vowel that triggered palatalization. This would seem to imply either

⁴ Jagić's and Sobolevskij's remarks were reported by Gebauer in the first volume of his *Historická mluvnice jazyka českého* (1894: 314).

that palatalization took place before the reflex of the syllabic nasal (**in*) merged with that of the tautosyllabic sequence **en*, or that the product of the merger was a high front vowel for a time. Later investigators were by and large to prefer the former alternative. Unfortunately Baudouin's own view remains unclear. I am inclined to believe he would have agreed, but he does not say so explicitly.⁵

Despite considerable unclarity, Baudouin's formulation embodies an important lasting contribution to our understanding of the history of Proto-Slavic phonology, consisting in the insight that at the stage the second palatalization took place, the vowel system must have been significantly more archaic than the one attested in the earliest texts. It adds a further chronological layer to the ones implied by Potbnja's law:

1. First palatalization of velars (1893: 12-13).
2. Monophthongization of diphthongs, or, in Baudouin's even more sweeping words: "периодъ устраненияъ всѣхъ слоговъ съживающихся или закрытыхъ" (1893: 14, cf. 1894: 47-48).
3. Second palatalization of velars, including the progressive palatalization (1893: 14-16, 1894: 49-50).
4. Late innovations involving the vowel system: development of reconstructed **ī/ĩ* into attested *i/b*; development of reconstructed **ŋi* into *ę* (possibly implying in modern terms: merger of the reflex of **in* with that of **en*, ultimately yielding *ę*).
5. Beginning of the Old Church Slavonic tradition.

As for the exceptions, Baudouin explained them by assuming that palatalization took place only if the syllable immediately following the velar was stressed (1893: 15, 1894: 48), e.g. R. *licó* vs. *lik*, Gsg *líka*. Even if he had not told us himself (1894: 48), it would have been obvious that this rule was inspired by Verner's law.

In the earlier article the accentual rule is illustrated by means of three pairs of examples from Russian, here quoted as listed by Baudouin (1893: 16):

лѣк, о́блик / лицó
 кли́кать, кли́кнуть / восклицáть
 двѣгáть / подви́заться

⁵ Baudouin assumes that palatalization was triggered not only by the reflex of syllabic **n*, but also by those of syllabic **r* and **l*. As far as I can see, the subsequent discussion about this problem (which eventually led to a virtual consensus that these elements do not trigger palatalization) has no bearing on the problems the present contribution is about and will not be taken into account. By the way, earlier I wrote that Baudouin may also have been aware that palatalization takes place after **i* if it reflects **ī*, but not if it reflects **ei* (Vermeer 2000: 7). I now see that interpretation rests on a misunderstanding of Baudouin de Courtenay (1894: 47-48).

It goes without saying that this is not at all conclusive, especially because it is easy to find counterexamples.

In the later article the following corroborating examples with palatalization are listed: *scat'*, *-klicát'*, *-nicát'*, *-ricát'*, *blístát'*, *-dvizát'*, *brjacát'*, *-sjazát'*, *-tjazát'*, *derzát'*, *terzát'*, *zercá-*, *licó*, *vincó*, *kol'có*, *derevcó*, Gsg *otcá*, Gsg *kupcá*, Gsg *borcá*, Gsg *žecá*, *ovcá*, *stezjá*, *nel'zjá* (48-49). Whereas corroborating examples with unmodified velars are not listed at all, two lists of counterexamples are added:

- Modified velar where the rule does not generate palatalization: *sérdce*, *sólnce*, Gsg *stárca*, Gsg *xlébca*, *ptíca*, *devíca*, *bol'níca*, *górnica* (49).
- The reverse: Gsg *stariká*, Gsg *pečniká*, *-klikát'*, *-nikát'*, *-prjagát'*, *-sjagát'* (49).⁶

Baudouin's treatment of the material consists of mere lists of examples which are not explicitly related to the proposed rule, followed by the promise that the numerous counterexamples will some day receive an analogical explanation. There is no hint as to what such an explanation might look like (48). Since the link, if any, between the stress rule and the linguistic facts is not made explicit, the rule has the status of an unsubstantiated claim.

3. Towards a viable solution (1894-1911)

Although Baudouin's articles represented a notable advance with respect to earlier treatments, they cannot have failed to disappoint those who had held their breath for more than a decade waiting for a comprehensive discussion of the problem, if not the final truth. Obviously matters could not be left as they stood. Numerous gaps had to be filled in and there were chronological and other implications that had to be brought to the surface and faced.

Most of this was accomplished in the two decades following Baudouin's articles. It is a fascinating story in which most of the great names of Slavic historical linguistics put in appearances. The principal subjects discussed were:

- the stress rule, which was eventually to fade away (3.1) to make room for the assumption that palatalization was blocked if the velar involved was followed by a back vowel (3.2, 3.3);
- the reflex of **oi* after velars modified by the progressive palatalization (3.4, 3.5);
- the chronology of umlaut in its relation to the progressive and second regressive palatalization (3.6, 3.7, 3.8);

⁶ In the examples the transcription has been modernized.

- the etymological source of the front vowel preceding the velar and triggering palatalization (3.8, 3.9).

In order to formulate a sound law that would account for the facts and make phonological sense, linguists had to learn to take into account more aspects of the prehistory of Slavic than would normally have been the case, such as the history of the vowel system, the structure of the nominal and pronominal *o*- and *ā*-stems, and many problems of relative chronology. The fact that all of this turned out to matter is what makes the progressive palatalization so much more important than the average sound law. It is not just a sound law, but a window on the prehistory of the phonological system of Slavic. Unfortunately it needs sensitive handling if it is to realize its potential.

Since the many problems thrown up by the progressive palatalization did not appear on the scholarly agenda sequentially but more or less in parallel, the following discussion of the main lines will take the different strands one at a time.

3.1 The demise of the stress rule

Despite the plainly preliminary nature of the stress rule as propounded by Baudouin de Courtenay, even highly critical scholars took it very seriously. Herman Hirt reported it without reservations in *Der indogermanische Akzent* (1895: 90). Antoine Meillet (1902: 165) and Holger Pedersen (1905: 383-385) operated with it as if it were quite respectable and solidly established.

This is an example of a phenomenon we shall be coming across time and again. At the time, scholars appear to have made no clear distinction between two types of statements:

1. Those that have been explicitly related to the relevant facts and/or other serious statements, in other words those that have been properly argued or substantiated.
2. Those that are just preliminary claims or shots in the dark or authoritarian assertions of faith or pieces of sheer unconstrained speculation or empty rhetoric.

It goes without saying that sooner or later statements of the first type may turn out to be wrong (they usually do), and that statements of the second type can serve useful functions, e.g. in directing research, and may even be converted into statements of the first type by dint of luck and hard work (they sometimes are). Nevertheless it is obviously wrong to ignore the difference.

However, that was not generally felt at the time. Even Meillet and Pedersen were persuaded by an assumption about Proto-Slavic historical phonology that was based on a list of unanalyzed examples from nineteenth-century Russian plus an empty promise.

Those less sanguine than Meillet and Pedersen were few and timid. Vatroslav Oblak, in his review of Baudouin's first article (to which we shall be returning in section 3.4), griped briefly about the many analogies he assumed the stress rule would imply if fully elaborated, but failed to make clear what exactly he had in mind and in the end accepted it (Oblak 1895: 320). Another relatively early dissident was Václav Vondrák, who however did not get beyond a curt "Allein damit kommt man nicht aus" (1906: 266).

Unease remained muted until after the middle of the first decade of the new century. This may have been due in part to Jan Rozwadowski, who demonstrated elegantly that the suffix **-ica* (where the stress rule generates ***-ika*) can be explained away as a recent formation and hence as irrelevant to the problem (1897).

Rozwadowski's analysis held out the promise that similar explanations would eventually emerge to account for the rest, which led an unreal existence anyhow because they had nowhere been publicly collected and discussed. Indeed, given the tentative state Slavic accentology was in at the time, chances are that it would not have been possible to compile a reasonably useful collection of examples and counterexamples. Anyhow nobody tried. This did not diminish the enthusiasm with which the accentual rule was embraced by nearly all concerned.

As the decade was drawing to a close, however, Pedersen (1907: 216-217) and Meillet (1910: 372) came to realize that the stress rule was too strong and tried to amend it in such a way that palatalization would be expected also in certain unstressed syllables (Pedersen), or perhaps even regularly unless the immediately preceding syllable was stressed (Meillet).⁷

Even the latter formulation led to nasty difficulties, as is shown by Meillet's attempt to account for the alternation found in *кѣнѣдзѣ* vs. **кѣнѣгыни*, Russian *knjaz'* (Gsg *knjázja*) vs. *knjaginja*.⁸ Since an analogical explanation of the alternation would seem to encounter grave difficulties, this is a potentially crucial example. If taken at face value, it turns out to conflict strikingly with the stress rule: if the attested accentuation corresponds to the one that obtained when palatalization took place, the stress rule (even as watered down by Pedersen and Meillet) generates ***кѣнегѣ* vs. ***кѣнѣдзи́ни*, R. ***knjag* (Gsg ***knjága*) vs. ***knjazínja*. Obviously there was a problem here.

Meillet attacked the problem head-on. In order for the rule to work, he needed to reconstruct a stage in which the *-e-* was unstressed in **кѣнѣдзѣ* but stressed in **кѣнѣгыни*. To accomplish that, he assumed (plausibly) that **кѣнѣдзѣ* was origi-

⁷ Meillet appears not to have realized that he was modifying Baudouin's version of the stress rule. He always reports his version of it as if he is paraphrasing Baudouin de Courtenay (e.g. 1905: 337, 1910: 372, 1924: 83).

⁸ The absence of **кѣнѣгыни* from the Old Church Slavonic corpus has generally been regarded as accidental and the word has always been treated as Proto-Slavic.

nally stressed on the first syllable, continuing the accentuation of the source language, but (more problematically) that **kьnegyŋi* was stressed on the second; if those assumptions are correct, the attested stress can be ascribed to the loss of the jer in the case of **kьnędzь*, and to de Saussure's law in that of **kьnegyŋi*. This solution implies that the progressive palatalization preceded de Saussure's law. To quote Meillet's own words: "L'exemple *kūnędzī* est remarquable; l'accent tombait originairement sur *ū* puisqu'il s'agit d'un emprunt au germanique *kuning*; par suite *g* après *ę* est devenu *dz* d'après la loi de Baudouin de Courtenay; le féminin *kūnęgynji* devait au contraire avoir l'accent sur *ę*, d'où le maintien du *g*; puis, comme l'*ę* était intoné doux (serb. *kněz*), l'accent s'est déplacé et a passé sur *y* (intoné rude), d'où r. *knjaginja*, s. *kněginja*."⁹

Meillet does not make clear why he thinks **kьnegyŋi* had to be stressed on the *ę*; the hidden premise is probably that the suffix **-yŋi* caused the presence of this kind of stress, more or less in the way the imperfectivizing suffix *-yva-* is responsible for the place of the stress in Russian examples like *vybrásyvat'*. However, this should have been made explicit and substantiated. As it is, it remains a mystery why **kьnegyŋi* was not simply stressed on the same syllable as **kьnędzь* (i.e. on the *ь*), especially in view of such examples as Russian *inokinja* 'nun', which keeps the stress of *inok* 'monk'.

Chances are that Meillet was aware of all this and uneasy about it. The logical next step was to jettison the stress rule altogether and that is what he did. In the first edition of *Le slave commun*, which appeared in 1924, but most of which was finished by 1915, he states: "L'hypothèse de M. Baudouin de Courtenay suivant laquelle *k, g* auraient subsisté après voyelle accentuée, et seraient devenus *c, dz* après voyelle inaccentuée, n'explique pas tous les faits sans violence" (1924: 83).

This reads like yet another of the unsubstantiated assertions the reader of the literature on the progressive palatalization is expected to swallow all the time. Strictly speaking it is one. However, recalling Meillet's unequal struggle with **kьnędzь*/**kьnegyŋi* we may have some idea of what he had in mind when he used the word "violence".

Around 1910 nearly everybody appears to have decided that the stress rule was beyond redemption. And just as it had been embraced in the mid nineties without explicit justification, it was now dropped like a hot potato, without anybody taking the trouble to explain what was wrong with it.

⁹ Note that in the quotation, *ī* and *ū* render late Common Slavic or Old Church Slavonic *ь* and *ѣ*, rather than the short vowels of the system preceding the rise of the jers, as in the transcriptions used by Baudouin de Courtenay and most later scholars.

The only investigator to operate with the stress rule after 1910 is Trubetzkoy (1922), to whom we shall be returning in section 4.2.¹⁰

3.2 Šaxmatov's blocking principle and its precipitous downfall

Soon after Baudouin de Courtenay had published his two articles, Aleksej Aleksandrovič Šaxmatov (1896: 703-705) put forward a different mechanism to accommodate the counterexamples.

He draws attention to the vowel following the velar, assuming that back vowels blocked palatalization, or, in his words, “что гласныя передняго ряда уподобляли себѣ слѣдующія за ними задненѣбныя согласныя [i.e. caused palatalization] въ томъ случаѣ, когда лабиализація ихъ не была защищена положеніемъ передъ гласными задняго ряда” (703). He does not list all vowels, but his examples show that he assumes that a following **-a* did not block palatalization, but **-o*, **-ъ*, **-u*, or **-ŕ* did; *y* is not mentioned.

It must have been clear immediately that the blocking principle was going to be easier to operate with than the stress rule, but not by a large margin. Any conceivable version of it implies that the progressive palatalization gave rise to paradigms in which modified and unmodified velars alternated in stem-final position, depending on the (first) vowel of the ending. On the basis of Šaxmatov's list we expect Gsg **отъса*, but Dsg **отъку*, NAsg **отъкъ*. Since such alternations are not attested even in the earliest textual material, we have to conclude that they were analogically eliminated before the onset of the tradition. In other words: if Šaxmatov's formulation of the blocking principle is correct, the phonologically regular forms **отъку* and **отъкъ* must have been replaced analogically with the attested forms *отъси* and *отъсь* (cf. Šaxmatov 1896: 704).

Well-nigh any conceivable version of the blocking principle necessarily implies subsequent levelling of the alternations involving the final stem consonant to which it gives rise.¹¹ This greatly complicates the task of determining exactly which vowels blocked palatalization and which did not. After all, since the progressive palatalization happens to be progressive, since in Slavic nearly all words are inflected and since most stems are monosyllabic, the overwhelming majority of velars potentially affected by the palatalization happen to stand in stem-final position and hence may or may not be analogical. There is no obvious way of telling in which of the following examples the *-c-* is phonetically regular: NAsg/Gpl *отъсь*, Gsg *отъса*, Dsg *отъси*, Dpl *отъсетъ*, Apl *отъсе*, Ipl *отъси*.

¹⁰ Mikkola (1942: 171) is not a genuine exception because his *Urslavische Grammatik* was conceived before 1910.

¹¹ Unless one manages to reconstruct the shape of the endings in such a way that they all start in vowels that do not block palatalization. An attempt to accomplish this was undertaken much later by Lunt (1981: 15-17).

Given this situation, progress is bound to be complicated and slow, but would seem to be possible along three lines:

- Search for examples in which the reflex of the velar involved (whether modified or not) cannot reasonably be attributed to analogy. This means looking for examples that stand outside the system of inflectional paradigms (e.g. adverbs or conjunctions), or in which the velar is not stem-final. Examples of either type are bound to be few.
- Search for phonetic and phonological plausibility. The resulting list of blocking vowels must make phonetic sense in terms of the system in which the progressive palatalization is assumed to have taken place. If it were to be concluded for some reason or other, say, that **-a* blocked palatalization, but **-u* did not, one would be entitled to conclude that something has gone wrong somewhere.
- Search for morphological plausibility. If the progressive palatalization gives rise to alternating paradigms, the resulting levellings must be shown to be plausible.

Šaxmatov's specific proposal was built exclusively on considerations of phonetic plausibility.

Many years later, Šaxmatov came up with a slightly different formulation, according to which **-b* did not block palatalization; nor did **-y*, which was now taken into account for the first time (1915: 65). Unfortunately he does not say that he has changed his mind, let alone why.¹² However, by this time so much had happened that only a much more explicit discussion of the point would have made any sense.

Šaxmatov's blocking principle was duly reported by Josef Zubatý in one of the book notices he contributed to the *Anzeiger für indogermanische Sprach- und Altertumskunde* appended at the time to *Indogermanische Forschungen* (1899). Otherwise, however, it was completely ignored and it sank to the bottom without leaving ripples.

3.3 The resurrection of the blocking principle

In 1910 (i.e. fourteen years after Šaxmatov's original proposal), Zubatý argued that the stress rule fails to account for doublets like **stbdza/*stbga* (of which there are several) and that it might make sense to look elsewhere for a solution.

Strictly speaking this was much too fast: any adherent of the stress rule would have argued that an alternation would have been perfectly natural in case of words

¹² According to Ljapunov (1923: 256), changing his mind without telling the reader was a feature of Šaxmatov's scholarly style.

with mobile stress patterns, e.g. Nsg **stbɔza* vs. Asg **stbgɔ* with the accentuation of R. *ruká/rúku*. Subsequently, paradigmatic levelling could easily have given rise to doublets. Indeed (or so they would have argued), such doublets could be interpreted as precious evidence about the Proto-Slavic stress paradigm of the words involved. For a case in point see Meillet (1902: 165) or, for that matter, Zubatý's own discussion of **lɔɔza/lɔga* (1910: 148-149). However, at this stage nobody was willing to take up the case of the stress rule any more.

As a promising alternative, Zubatý (who may have remembered the book notice he had contributed more than a decade ago) now drew attention again to Šaxmatov's blocking principle.¹³

One may wonder why it took the blocking principle so long to resurface, or, more to the point, why it was ignored in the first place. I suspect that Šaxmatov's colleagues interpreted his early article the same way they had Baudouin's actions in the eighties: as a claim to priority intended to buy time to develop the conception in more detail. By 1910 they must have decided there was no point in waiting.

While adopting the blocking principle, Zubatý argued that the specifics proposed by Šaxmatov needed to be amended because in the meantime Paul Kretschmer had demonstrated on the basis of loans that the Slavic **o* was [a]-like until a fairly late prehistoric stage, hence unlikely to block palatalization (Kretschmer 1905).

Kretschmer's article revolutionized the understanding of the prehistory of the Slavic vowel system. It showed that the evidence provided by the languages Slavic interacted with during the centuries preceding the earliest texts implies that the vowel system reflected in the texts must have been the outcome of some fairly recent innovations. It is not the place here to describe the reception of Kretschmer's discovery, which was complex and contradictory and has tended towards extremes. For the purposes of this contribution it is sufficient to note that it influenced thinking about the progressive palatalization decisively by removing **o* from the list of blocking vowels.¹⁴

On the basis of phonetic plausibility, Zubatý assumes that palatalization took place before **-a* and **-o*, but was blocked by **-u*, **-y*, **-v*, and perhaps **-ɔ* (151). In addition he points out that the unmodified velar in **kɔnɛgyŋi* (contrasting with *kɔnɛɔzɔ*) shows conclusively that palatalization was blocked if an **y* followed the velar.

This is a clinching example of the type one would have hoped for: if the progressive palatalization took place before **y*, that would have yielded ***kɔnɛɔziŋi* (R. ***knjazinja*, SCr. ***knèzinja*), which no power on earth could have changed

¹³ According to Lehr-Splawiński (1911: 144), Kul'bakin did the same independently from Zubatý in a publication I have not had access to.

¹⁴ One of the rare exceptions is Vondrák (1923-24: 21-22), for which see further section 4.3.

back into **kьnegyŋi* again, because there was no model for such an analogy, let alone a motivation.

If one recalls how difficult it was for Meillet to reconcile this very same example with the stress rule (section 3.1), one will appreciate the superiority of the blocking principle and understand why it found itself in the saddle now. It has rarely been challenged since, although the specific lists of blocking vowels proposed by different investigators have continued to differ. But we are running ahead of developments.

3.4 The reflex of **oi* after velars modified by the progressive palatalization 1: Jagić's leap

The earliest of Baudouin de Courtenay's two articles was briefly reviewed by Vatroslav Oblak (1895), who brought up two points of criticism.

To begin with, he objected to the fact that **ɛ* is held to have triggered palatalization whereas **ě* is not: "Auffallend bleibt es auch, dass nach *ɛ* diese Palatalisation eintritt, nach *ě* aber unterblieb z.B. *lěkъ, lěkà*. Wir würden eher das Umgekehrte oder wenigstens eine gleiche Behandlung erwarten" (320). Since obviously Oblak did not understand Baudouin's sophisticated conception of this point (see section 2.3) it does not need to be discussed.

More importantly (and in hindsight more ominously), Oblak drew attention to the fact that in the nominal *o*-stems the reflex of **-oi* after velars modified by the progressive palatalization appears to be *-i* (undergoing umlaut), whereas we find *-ě* after velars modified by the second regressive palatalization, as in Lsg *tocě* vs. *otъci*, Lpl *tocěxъ* vs. *otъcixъ*, DLsg *roqě* vs. *ovъci*. He concluded that there must have been a phonetic difference, or, to quote his words: "aber warum kein *otъcě, otъcěhъ* (aus *-ikoi, -ikoisu*) im loc. wie *vlъcě, vlъcěhъ* bei Gleichheit der Betonung? Der Unterschied wird in der Qualität des *c, z* begründet sein, die durch vorausgehenden palat. Voc. bedingten *c, z* waren weicher und deshalb konnte nach denselben nicht *ъ* stehen" (320).

Oblak's observation was not new. Some fifteen years earlier, Potebnja (1879: 361-362) had adduced the same facts to argue for the original presence of a **j* in such cases (see section 2.1). What was new was Oblak's conclusion that the second regressive and the progressive palatalization yielded phonetically different outcomes.

Matters did not stop here. A few years later, Vatroslav Jagić adopted Oblak's interpretation of the *-i* in *otъci* etc. as the only piece of evidence to conclude that the second regressive and the progressive palatalization must have belonged to different periods, with the former assumed to have taken place significantly earlier than the latter.

This is the earliest attempt to assign the progressive and the second regressive palatalization to different chronological stages. It is the ultimate source of the tra-

dition of calling the progressive palatalization the “third”. It is a historic moment. Let us step back to savour Jagić’s authentic words:

“Nach meinem Dafürhalten war auch der Ce-Palatalismus bereits entwickelt [i.e. the second regressive palatalization had taken place], als zuletzt noch die so beliebten Suffixe њць, њца, њце, ица, ице, аць, азь aufkamen. Ich folgere diese Reihenfolge der Entstehung aus der physiologisch [i.e. phonetically] verschiedenen Weichheit dieses suffixalen *c-z* gegenüber dem üblichen *c-z* des Ce-Palatalismus. Das letztere *c-z* galt nicht für so weich in der Aussprache, dass nicht die Lautgruppe цѣ, зѣ möglich gewesen wäre, man sprach ржцѣ, нозѣ, висоцѣ, блазѣ, тыцѣте u.s.w. Dagegen bei allen mit den oben erwähnten Suffixsilben gemachten Wortbildungen gilt der Consonant *c* und *z* für so weich, dass man nur отьци, овьци, лици, кѣнази, пользи sprach. Es ist nicht abzusehen, warum die Sprache an diesem feinen Unterschied zäh festgehalten hätte, wenn nicht das letztere *c-z* lautphysiologisch von dem ersteren verschieden gewesen wäre. Ich meine daher, dass die jetzt so sehr beliebten zahlreichen Ableitungen vermittelt der Suffixe њць, иць, њца, ица, њце, ице die jüngste Entwicklungsstufe in der Ausgestaltung des slavischen Sprachtypus repräsentiren, hervorgegangen aus *k-g* erst dann, nachdem schon längst der Če-, aber auch der Ce-Patalalismus entwickelt war” (Jagić 1901: 128).

In my view this is unworthy of its author. There is no trace of anything that might begin to resemble reasoning. A phrase like “es ist nicht abzusehen” cannot change the fact that Czech has held on to “diesem feinen Unterschied” until the present day without any phonetic motivation anywhere in sight, cf. Lsg *jazyce* vs. *měsici*, DLsg *ruce* vs. *lavici*. And Jagić does not explain why the progressive palatalization came last instead of first, limiting argumentation to the single word “daher”.

If one examines the evidence for *-i < *-ě* Oblak and Jagić relied on, what strikes one more than anything else is that it is limited to the inflection of the *o*-stem and *ā*-stem nouns. Conditions elsewhere in the system were pointedly ignored. And yet, being the grandmasters of Slavonic philology they were, they surely did not need to be told about the pronouns and verbs that offer counterexamples with *-ě*, e.g. Imp2pl *rbčete*. For some reason the existence of that evidence remained unacknowledged.

3.5 The reflex of **oi* after velars modified by the progressive palatalization 2: Meillet’s *-ě*-

Even before Jagić jumped into the void (but presumably too late for him to notice before he was in mid air), Antoine Meillet had removed the only reason for doing so by analysing the Old Church Slavonic pronouns *sicъ* and *vbъbъ*. The text starts as follows:

“La série de formes v. sl. dat. plur. *sicěmŭ*, instr. plur. *sicěmi*, gén. loc. plur. *sicěchŭ* est sûrement ancienne; car le singulier *sicī*, *sicego* tendait à faire substituer des formes en *-imī*, *-imi*, *-ichŭ* à celles en *-ěmī*, *-ěmi*, *-ěchŭ* suivant l’analogie de *jŭ*-, *mojŭ*, etc., mais aucune analogie ne permettrait d’expliquer la substitution de *-ěmŭ*, etc. à de plus anciens *-imŭ*, etc. Par suite le locatif pluriel *srŭdŭcichŭ* de *srŭdŭce* est analogique de *krajichŭ*, etc. et tient la place d’un plus ancien **srŭdŭcěchŭ*. Le locatif pluriel en *-ěchŭ* n’a pu subsister dans les substantifs de ce type parce qu’il était entièrement isolé; au contraire le *ě* des formes pronominales *sicěmŭ*, *sicěmi*, *sicěchŭ* à survécu parce qu’il se trouve à tous les cas obliques du pluriel de *sicī*.

“Le caractère ancien des formes en *-cě-* résulte immédiatement de ces considérations morphologiques” (Meillet 1900: 8).¹⁵

Since the point brought up by Meillet has proved to be difficult to grasp and fraught with potential for misunderstandings, it may be instructive to reproduce his reasoning using more modern language.

After velars modified by the progressive palatalization we find two reflexes of **-oi*: *-ě* in pronouns (e.g. GLpl *sicěxъ*, *vsěxъ*) and *-i* in nouns (e.g. Lpl *otъcixъ*, Lsg *ovъci*). Since these cannot both be phonologically regular at the same time, one of them has to be analogical and it has to be determined which analogy is more plausible: introduction of *-ě* into paradigms that originally had *-i* (e.g. replacement of **sicixъ* with *sicěxъ*) or the reverse (replacement of **otъcěxъ* with *otъcixъ*). Since *-ě-* is much more firmly entrenched in the pronominal paradigm than *-i-* in its nominal counterparts, nominal *-i-* is much more likely to be analogical than pronominal *-ě-*. It follows that it is *-ě* that has to be regarded as the phonetically regular reflex of **-oi* after velars modified by the progressive palatalization.

We have seen that Meillet expressed himself more strongly, stating that “aucune analogie ne permettrait d’expliquer ...”. That may be felt to be just a bit too strong for the evidence he adduced, but ten years later Zubatý (whose role in reinstating and updating Šaxmatov’s blocking principle we have come across already) showed that Meillet’s evaluation was right after all by making clear how utterly unlikely replacement of *-i-* with *-ě-* really would have been. Zubatý’s reasoning has to do with the system of nominal and pronominal inflection types.

¹⁵ Meillet’s analysis of *sicъ* became possible only after the special status of the most archaic texts within the Church Slavonic tradition (Leskien’s canon) had been recognized, which happened in the late eighties. It is those texts that show conclusively that forms like *sicěxъ* go with *sicъ* and that *sikъ* (which occurs in less archaic material) is a recent analogical formation.

In addition to the familiar hard and soft *o*- and *ā*-stems, which were of old standing, the progressive palatalization must have given rise to an intermediate type (in Zubatý's terminology "poloměkké sklonění") comprising nouns with a stem ending in a velar potentially subject to modification by the progressive palatalization, e.g. Nsg **otbkъ*, Vsg *otbče*, Gsg *otbca* or Nsg *stbza*, Gsg **stbgy*, DLsg **stbzě* (these are Zubatý's examples).

As we have seen (section 3.2), any alternations involving the final stem consonant the blocking rule gave rise to (e.g. **otbkъ/otbca*) must have been analogically eliminated before the earliest texts. In the case of those nouns in which it was the modified velar that was generalized, we find the endings of the soft subtype (e.g. *otbcb*, Dpl *otbcmъ*), despite the fact that they originally must have belonged to the hard subtype, ending as their stems did in velar consonants with no pre-desinential **-j-* anywhere in sight. Nevertheless we do not find ***otbcb*, ***otbcmъ*, with retention of the original endings of **otbkъ*, **otbkomъ*. This needs an explanation: there must have been something that made this happen.

Such an explanation had been proposed ten years earlier by Meillet, who put forward the idea that we find *e*, *ь* and *i* because, quoting his words, "*o, ū, y n'existaient après aucune consonne palatale*" (1900: 9, note again that in Meillet's transcription, *ū* means *ъ*). Using slightly more modern language, Meillet assumed that at the stage involved, umlaut was a synchronic phonotactic rule converting any *o*, *ъ* or *y* that happened to find itself after a palatal consonant into *e*, *ь* and *i* respectively.¹⁶ Zubatý agreed with this, adding that since **-ě* was a front vowel already, we do not expect it to undergo umlaut, all the more so because at the stage involved such forms as Lsg **bodzě* show that **-ě* was perfectly possible after the reflexes of velars modified by the palatalization.

Analogical replacement of *-ě-* with *-i-* as advocated by Meillet and Zubatý results in elimination of the "poloměkké sklonění" and hence is perfectly motivated. On the other hand, if we assume that the phonetically regular reflex of **oi* was *-i-* even in the case of words with a velar modified by the progressive palatalization, we have to explain why pronouns with a stem ending in a velar modified by the progressive palatalization imported *-ě-* from the hard subtype, complicating the language by giving birth to a "poloměkké sklonění".

Put differently: whereas the system was such that replacement of **-ě-* with **-i-* (as favoured by Meillet) reduced the number of distinct inflectional types and by that token simplified the language significantly, the reverse (as implied by Oblak and Jagić) would have introduced a new inflectional type, in other words a complication that would have been inconceivable without a convincing motivation.

The central problem confronting the Oblak/Jagić option consists in the necessity of finding a motivation that would explain how such forms as Isg **vbsimъ*

¹⁶ Earlier (Vermeer 2000: 13) I incorrectly attributed this idea to Belić (1922-23: 135).

could be analogically replaced with *vsěmь*. As long as such a motivation has not been found, we are fully entitled to regard the replacement as something that “aucune analogie ne permettrait d’expliquer”.

In this context, adherents of the Oblak/Jagić scheme like to refer to Ščepkin’s review of Leskien’s *Grammatik der altbulgarischen (altkirchenslavischen) Sprache* (1909), making it seem as if Ščepkin demonstrates convincingly that the analogical replacement of **vsimь* with *vsěmь* actually took place. However, what Ščepkin says there is merely that the *-ě-* of such examples as *vsěxь*, *vsěmь* is “offenbar nicht phonetischen Ursprungs” (1910: 215). Since he limits argumentation to the single word “offenbar”, his view has the status of a shot in the dark and cannot possibly be treated on a par with the elaborate reasoning by which Meillet and Zubatý showed that *-ě-* has to be phonologically regular. To the best of my knowledge nobody has ever seriously tried to establish a case that would vindicate Oblak/Ščepkin, let alone succeeded in doing so.

As a bonus, Meillet’s and Zubatý’s view offered the opportunity to explain the isolated form *lbdzě* as a phonologically regular DLsg which became unstuck from its paradigm before the analogical extension of *-i* took place. Note however that this example has the status of a bonus. If one prefers to explain *lbdzě* differently or not at all, that does not affect the validity of Meillet’s *-ě-*, which is primarily based on *sicěxь*.

A second bonus consists in the possibility to regard the entire inflection of *vsь* as phonetically regular on the basis of a Proto-Slavic stem form **vbx-*. This point was argued with particular clarity and strength by Lorentz (1904), but cf. also the earlier statements by Ljapunov (1900: 73-74) and Hujer (1903).

Meillet’s and Zubatý’s analysis kicks the flimsy factual basis out from under Oblak’s assumption that there is a phonetic difference between the reflexes of the progressive and the second regressive palatalization. By that token it shows up Jagić’s leap into the void as ill-considered and naïve. More generally it refutes conclusively *any* account that generates ***-i* as the reflex of **-oi* after velars modified by the progressive palatalization.

3.6 Palatalization and umlaut: Pedersen’s chronology

It is impossible to study the progressive palatalization without running into the phenomenon of umlaut all the time.

Taken by itself, umlaut is straightforward enough, consisting as it does in the systematic fronting of back vowels after palatal consonants. However, the way it is interwoven with problems thrown up by the progressive palatalization is not straightforward at all, as we have seen already while examining the problem of the reflex of **oi*.

In that connection we came across a further point that has now to be gone into again and which can best be appreciated by returning to a very elementary level.

NAsg *otčb* and NAsg *lice* end in *-b* and *-e* respectively. If these vowels were of old standing (i.e. if they reflected PIE **i* and **e* respectively), one would expect ***otčb* and ***liče*, by the first palatalization of velars. For that reason if for no other they cannot be old, but must have arisen at some stage later than the first palatalization. How and when did they arise?

The most popular pre-neogrammarian solution was to attribute them to umlaut caused by a hypothetical pre-desinential **j*, as we have seen (section 2.1). However, that approach turned out to be fraught with difficulties, in particular because in that case one would also have expected ***otčb* and ***liče*, so that a new solution has to be devised.

The first to propose one was Meillet, in his remarks on the inflection of *sicb* (1900). As we have seen already, he attributed these vowels to umlaut caused not by **j*, but by the modified velars produced by the progressive palatalization. In order to make this possible he interpreted umlaut as a synchronic phonotactic rule.

That would seem to have disposed of the matter very nicely. Unfortunately at this point the debate took an unexpected turn that was to have awful consequences that are still with us.

In an aside in a monograph-sized article on subjects basically unrelated to the Slavic palatalizations, Holger Pedersen now argued that the progressive palatalization must be considerably older than the second regressive palatalization.

After the Oblak/Jagić fiasco this was the second attempt to assign the progressive and the second regressive palatalization to different chronological layers. There are two non-trivial differences between Pedersen and Oblak/Jagić. First, Pedersen's chronology is the reverse of Jagić's and, second, quite unlike Oblak/Jagić, Pedersen adduces a valid reason: "Jedenfalls ist Baudouin de Courtenay's gesetz [i.e. the progressive palatalization] ziemlich alt, viel älter als die palatalisation in *čěna* [...] u.s.w. Denn die durch B. de C.'s gesetz entstandenen *i*-haltigen laute haben auf ein folgendes *o* *ɔ* und *oi* die gleiche wirkung wie *j* ausgeübt: abulg. *lice*, r. *licó* 'gesicht', abulg. *otčb* 'vater', lok. abulg. *lici otčci*. Sie sind also älter als die monophthongierung der diphthonge" (1905: 385).¹⁷

If one combines Pedersen's findings with what we know already, the following chronology results:

1. First palatalization of velars.
2. Progressive palatalization.

¹⁷ Note the difference between Pedersen's perfectly valid use of "denn" and "also" in this passage and Ščepkin's purely rhetorical "offenbar". Note also that Pedersen was in a strikingly splitting mood that day, going so far as to put the palatalization before **v* plus front vowel later than the second regressive palatalization, ending up with as many as four chronologically distinct palatalizations: "Man wird vier slavische palatalisationen anzunehmen haben, die durch abulg. *četyre*, abulg. *otčb*, abulg. *čěna* und abulg. *cvěťb*, p. *kwiat* exemplifiziert werden können" (1905: 385).

3. Rise of umlaut.
4. Monophthongization of diphthongs.
5. Second regressive palatalization.
6. Late developments in the vowel system antedating the earliest textual evidence: development of reconstructed **i/ĩ* into attested *i/ĭ*; retraction of the predecessor of **o* from **[a]* to **[o]* (possibly also merger of the reflex of **in* with that of **en*, ultimately yielding *e*).
7. Beginning of the Old Church Slavonic tradition.

3.7 Pedersen's chronology and Meillet's *-ě-*

Unfortunately, Pedersen failed to take into account Meillet's demonstration that the regular reflex of **-oi* after velars modified by the progressive palatalization is *-ě* (1900).¹⁸ Since his chronology yields ***vbsixъ*, it produces the wrong output.

Pedersen's chronology has exerted an enormous influence on later scholarship, beginning with Lehr-Splawinski (1911) and Trubetzkoy (1922), and continuing with Jakobson (1929) and Mareš (1956) and many others. Indeed, it can be regarded as the dominant view, to the extent that there is one. However, since it does not account for Meillet's *-ě-*, it is deeply flawed.

By and large there have been two ways of coping with the difficulty: some (e.g. Lehr-Splawinski and Mareš) have just ignored it, contenting themselves with the incorrect output Pedersen's chronology generates if left to itself; others (e.g. Trubetzkoy) have devised alternative solutions that were ad hoc or otherwise problematic (see further section 4.2).

Strangely enough, nobody appears to have been aware of the simple solution that had been available since 1900 and which can be paraphrased as follows.

Meillet's *-ě-* shows incontrovertibly that there must be something wrong with the reasoning Pedersen's chronology is based on, however compelling it may look at first sight: something must have caused the appearance of umlaut attested by the endings of such examples as *otъcb* and *lice*; however, it can only have done so after the monophthongization of diphthongs (which converted the reflex of **oi* into a front vowel unable to undergo umlaut), because otherwise we would be stuck with ***vbsixъ* and ***sicixъ* (and ***lbdzi*) for all eternity.

As we have seen already (section 3.5), what that something was had been intimated by Meillet: umlaut may have been a synchronic rule that persisted in Proto-Slavic for a time, so that the fact that velars modified by the progressive palatalization caused umlaut does not necessarily mean that palatalization preceded the rise of umlaut; it may imply no more than that palatalized velars arose before umlaut *ceased* being a synchronic rule.

¹⁸ Since Pedersen's article was finished as early as "den 25. Januar 1902" (1905: 421), there may well be an innocent explanation for this.

In other words: the facts Pedersen's chronology was devised to explain can be accounted for in a way that incorporates Meillet's *-ě-* by chronologically separating the stage at which umlaut arose (which must have preceded the monophthongization of diphthongs) from the moment it ceased functioning as a synchronic phonotactic rule as envisaged by Meillet. Since umlaut was still a synchronic rule at the earliest attested Church Slavonic stage, the separation is inevitable anyhow and entails no extra cost. This yields the following chronology:

1. First palatalization of velars.
2. Rise of umlaut.
3. Monophthongization of diphthongs.
4. Second palatalization (regressive and progressive simultaneously as parts of a single process).
5. Late developments in the vowel system antedating the earliest textual evidence: development of reconstructed **ī/ĩ* into attested *i/b*; retraction of the predecessor of **o* from **[a]* to **[o]*; (possibly also merger of the reflex of **in* with that of **en*, ultimately yielding *ę*).
6. Beginning of the Old Church Slavonic tradition.
7. Cessation of umlaut.

3.8 Umlaut and the vowel preceding the velar

A third point having to do with umlaut was brought up by (once again) Meillet as an aside in the second part of his *Études sur l'étymologie & le vocabulaire du vieux slave* (1905).

As we have seen, Baudouin de Courtenay realized that the progressive palatalization took place before the earliest attested vowel system arose. He may even have been aware of the fact that it did so before the reflex of **-in-* merged with that of **-en-* (see section 2.3). These insights imply a research program aiming to separate the various origins of the vowels that trigger palatalization, not only *ę*, but *ь* and *i* as well, and see which can be shown to trigger palatalization and which cannot. This program was never explicitly formulated, perhaps because at the time scholars preferred not to formulate questions before they thought they knew the answer, but it is a program nonetheless.

It was known at the time already that all three front vowels after which we find modified velars have multiple origins:

- *ę* (a) < **-en-* and (b) < **-in-*, the latter reflecting an earlier syllabic nasal and the sequence **-in-* in borrowings.
- *ь* not only (a) < short **-i-* (the canonical reflex), but also (b) < short **-u-* with umlaut and in certain cases (c) < short **-e-* (e.g. *i*-stem Npl *-bje* < **-eies*).
- *i* (a) < long **-ī-* and (b) < **-ei-*, but also (c) < long **-ū-* with umlaut.

Now Meillet proposed that the seemingly anomalous retention of the original velar in examples like *ožika*, *bližika* can be explained by the assumption that the suffix could well reflect **-jyka*, which regularly developed into *-ika* by umlaut (1905: 336). Note that the assumption of **-j-* is necessary anyhow to account for the *ž* (<**zj*) in these examples. This idea implied that at the stage where palatalization took place, **y* after palatal consonants was still distinct from **i*. This adds yet another element to the chronological layer consisting of late developments in the vowel system antedating the earliest textual evidence: merger of the reflex of umlauted **y* with that of **i*.

3.9 Lehr-Splawiński's "Nowsze poglądy"

The earliest publication entirely devoted to the progressive palatalization is Tadeusz Lehr-Splawiński's "Nowsze poglądy ..." (1911). The article, which is a milestone in the history of the subject, has been ignored by all but a few later investigators.

Lehr starts off with an informative overview of the literature (141-145). There are some gaps, but few of them are serious. The absence of Oblak (1895) is compensated by Jagić (1898, 1901), the omission of Meillet (1900) is largely made good by the presence of Lorentz (1904) and Zubatý (1910). The most unfortunate omissions are Meillet (1902, 1905) and Pedersen (1907). Indeed, Lehr appears to have missed everything Meillet had so far written on the subject. This foreshadows the later tendency to consign Meillet's contributions to oblivion, a tendency even Meillet himself was eventually to succumb to, as we shall see presently (section 6.2).

Lehr rejects Baudouin's stress rule, which he regards as phonetically unfounded and requiring unlikely analogies, insisting that there never was any objective foundation for the enthusiasm it was received with: "Mimo zupełnego braku uzasadnienia fizjologiczno-fonetycznego – nie widzimy bowiem nigdzie w językach słowiańskich, żeby spółgłoski innym ulegały zmianom w zgłoskach akcentowanych, a innym w nieakcentowanych –, mimo konieczności przyjmowania bardzo daleko idących a mało prawdopodobnych wyrównań analogicznych w zakresie akcentu, ta druga część "prawa Baudouin'a" spotkała się w pierwszej chwili z okólnym prawie uznaniem" (142). Like Oblak before him, and Meillet and others after him (see section 3.1), Lehr fails to specify what exactly is so objectionable about those analogies.

Instead he opts for Šaxmatov's blocking principle as recently dredged up and reanimated by Zubatý and Kul'bakin. He agrees with Zubatý that **o* is unlikely to have blocked palatalization, adding that the same probably holds for **u* and **o̥*, which reflect sequences that began in **o* at an earlier stage (<**ou*, **on*). This leaves **y* and **o̥*, the only vowels definitely known to have been rounded at an

early stage because of the prothetic *v-* they developed in the attested languages in initial position (144).

Lehr's reasoning obviously is too schematic to get rid of **u* and **o*: although it is true that these vowels reflect sequences originally beginning in **o-*, there is no denying that eventually they turned out to differ from **o* in remaining rounded vowels even in umlaut position (Dsg *mčžju*, Asg *doušjō*); for all we know this difference may have influenced the realization of preceding velars at the stage the progressive palatalization took place. What one would have expected Lehr to have done is argue that the palatalization took place before the differentiation arose.

Lehr adds that consonants, too, must have blocked palatalization, a point he is the first to bring up, perhaps because earlier investigators may have found it too obvious to make explicit (147).¹⁹

As for the vowel preceding the velar, Lehr assumes that the progressive palatalisation must have taken place before the merger of **-in-* and **-en-* (146-147). We have seen (section 2.3) that Baudouin de Courtenay may have been aware of the point, but this was the first time it was explicitly formulated. For reasons of space I will not go into Lehr's argumentation, which is not at all conclusive and has left no traces in the subsequent literature.

Neither in the case of the blocking rule, nor in that of the vowel preceding the velar, Lehr discusses factual material that could cast light on the matter. He appears to be unaware of the corroborative value of the alternation in Meillet's and Zubatý's example *kъnędzъ/*kъnęgyňi* (see sections 3.1 and 3.3).

Lehr rejects Meillet's **-ě-*, preferring Ščepkin's view that the *-i-* of *отъсѣхъ* etc. is regular. In doing this, he disregards the fact that Ščepkin's argumentation is limited to the single word "offenbar" (see section 3.5), thereby providing a further example of the failure to heed the crucial difference between statements that have been properly substantiated and shots in the dark.

The assumption that *-i-* is phonetically regular causes Lehr to adopt Pedersen's chronology, forcing him to face the major problem that chronology throws up in addition to not generating Meillet's *-ě-*: since it separates the progressive from the second regressive palatalization it requires a specification of how, why and when the two developments came to have exactly the same reflexes in the attested material.

Lehr solves the problem brilliantly in a way that is closely related to Meillet's interpretation of umlaut as an automatic synchronic rule: he assumes that an early stage of palatalization (**k > *Ķ* etc.) arose before umlaut (and hence the monophthongization of diphthongs) took place and that it remained in the language as a synchronic phonotactic rule; the new palatal consonants caused umlaut of following vowels; when monophthongization took place, velars automatically became

¹⁹ It is implicit as early as Gebauer (1894: 314).

palatalized if followed by the new front vowels (**koi* > **kě* etc.); finally all palatalized velars were assibilated (**K* > *c* etc.). In other words: palatalization of velar consonants in certain contexts was a synchronic rule of the language which originated at a stage preceding the rise of umlaut, but was still present when the monophthongization of diphthongs took place.

However imaginative this may be, it does not remedy the principal inadequacy of Pedersen's chronology, which is its failure to produce Meillet's *-ě-*, cf.: **-ikoi/-ukoi* > **-ikoi/-ukoi* (first stage of progressive palatalization) > **-ikeil/-ukoi* (umlaut) > **-ikī/-ukē* (monophthongization automatically followed by palatalization of velars in palatal surroundings) > **-icī/-ucē* (assibilation) > **-bcī/-bcē* (late developments preceding the earliest texts).

As will be clear from the foregoing, Lehr's article is not perfect. Nevertheless it was a gigantic step forward, if only because for the first time in the history of the subject it offered a comprehensive treatment comprising an overview of the literature and a coherent reconstruction trying to capture all aspects of the problem instead of one or two selected points. In addition it offered several novelties, some of which were to have a great future, in particular a formulation of the blocking rule that was to be accepted or rediscovered by many later scholars beginning with Belić (1921), and a two-phase account of the palatalization that was taken up by Jakobson (1929) and was still going strong in the sixties, although it never proved able to account for Meillet's *-ě-* without crutches.

3.10 Synthesis

In 1911 it was possible for the first time to present a theory of the rise of the progressive palatalization which could be argued to solve all problems in accordance with neogrammarian standards. This could be done by incorporating into Lehr's account three insights that had first been formulated by Meillet:

- (a) Meillet's *-ě-*.
- (b) Meillet's interpretation of umlaut as a synchronic rule.
- (c) Meillet's demonstration that the progressive palatalization is not found after *i* reflecting umlauted **y*.

The chronology that results is the following:

1. First palatalization of velars.
2. Rise of umlaut as a synchronic phonotactic rule.
3. Monophthongization of diphthongs.
4. Second palatalization of velars: palatalization takes place (a) before any front vowel, (b) before **v* plus front vowel (not in West Slavic), and (c) after high front vowels unless the velar is followed by a consonant or by short **ǔ* or long

* \bar{u} , i.e. by the predecessors of \bar{u} and y (and possibly also by the predecessors of * u and * o , which have not been eliminated in a satisfactory way).

5. Merger of the reflex of * in with that of * en ; development of reconstructed * $\bar{u}/\bar{i}/\bar{i}$ into attested $y/\bar{u}/\bar{i}$; retraction of the predecessor of * o from *[a] to *[o]; merger of the reflex of umlauted * y with that of * i .
6. Beginning of the Old Church Slavonic tradition.
7. Cessation of umlaut.

Note that Zubatý's and Lehr's interpretation of the blocking rule explains why we find no palatalization after $i < *y$ with umlaut: at the time of the palatalization the later y was still [ū]-like and even though fronted by umlaut (resulting in an [ū̄]-like vowel) it did not merge with i . The merger took place only when rounding was lost, in other words when the earlier *[ū] developed into y .

All elements of this chronology were available by 1911. It is the first solution that is viable in principle in the sense that it generates the right reflexes in the right places without leaving unexplained examples or giving rise to other serious problems. However, it is still sketchy in a number of respects: uncertainty persists about the precise set of vowels blocking palatalization, not all problems connected with the vowel preceding the velar have been faced, etc.

3.11 Final touches

By 1911, the question as to which vowels triggered palatalization of a following velar had not yet completely been sorted out. As we have seen, it may have been Baudouin de Courtenay's view already that * \bar{e} does not trigger palatalization when it reflects earlier * en ; the point had been brought up again by Lehr-Splawiński (1911: 146-147), but discussion of the relevant factual material remained a desideratum. Along the same lines, Meillet had argued that * i does not trigger palatalization when it reflects * \bar{u} (see section 3.8). The picture was rounded off as follows:

- Referring to Meillet's discussion of cases like *bližika*, Belić (1921: 34) argued that the absence of palatalization in *igo* suggests that palatalization does not take place after * $\bar{u} < *u$ with umlaut. Independently from Belić, and on the basis of more ambiguous material, Trubetzkoy (1922: 230-231) put forward the same idea.
- Trubetzkoy (1922: 227) suggested that * i may not trigger palatalization if it reflects * ei , but he presented no factual material to support this. This was done for the first time by Meillet (1934: 92-93). Since most of the potential examples are ambiguous the point has remained controversial. The situation is probably characterized best by Vaillant: "Il est sans doute conjectural dans beaucoup de cas de distinguer les deux origines de sl. *i*, mais dans les cas les

plus sûrs on voit que la palatalisation apparaît après un ancien *ī, et non après *ei” (1950: 53).

Belić provided Zubatý’s and Lehr’s versions of the blocking rule with factual support, arguing that the retention of the root-final velar found in adjectives like *lbgbkb* (instead of ***lbzgbkb*) supports the assumption that palatalization was blocked by *b (Belić 1921: 25-26).

As we have seen, the solution that was available by 1911 had to be pieced together by combining insights put forward by different scholars. It took decades before the seemingly minor task of formulating a complete explicit statement was finally performed.

Strictly speaking it was never performed at all. Vaillant (1950: 53-55) comes closest, but his account is very brief and fails to argue the very point history had by then amply shown to be most in need of argumentation: Meillet’s -ě- is merely stated as if it is obvious, without discussion or references to literature where explicit argumentation is given (e.g. Meillet 1900, Lorentz 1904, Zubatý 1910, van Wijk 1931: 68-69).

3.12 Excursus on the principal alternative: -j- plus Baudouin de Courtenay

As we have seen (section 2.1), before the idea of a progressive palatalization emerged, palatalization was generally attributed to a *-j- following the velar. In the late seventies of the nineteenth century this assumption got into difficulties because it was seen to conflict with the neogrammarian concept of sound law: since sequences of velar plus *j yield č/z/š, they cannot be assumed to yield c/dz/s too.

Some scholars (e.g. Baudouin de Courtenay) abandoned the idea completely, but some now started to combine it with the assumption of a progressive palatalization in such a way as to make it responsible for the counterexamples, in other words, as an alternative for such devices as Baudouin’s stress rule or Šaxmatov’s blocking principle. The first to do this was Karl Brugmann (Brugmann and Delbrück 1897: 291-292).

Brugmann’s proposal had the status of a preliminary claim. He discussed neither the relevant material, nor any other difficulties, of which there are several, notably the following:

To begin with, in most cases it is difficult or impossible to justify the reconstruction of the required *-j- on independent grounds. The crown witness of the Brugmann approach is the correspondence of Slavic *junьcb* ‘young bull’ with Lithuanian *jaunikis* ‘bridegroom’ (which is a *jo*-stem). Although one or two further Lithuanian facts appear to favour the assumption of a following *-j-, the evidence as a whole is meagre. However, since the Slavic evidence relevant to the progressive palatalization is not exactly abundant either, this has not deterred

scholars from attempting to reconcile the facts of the two languages on the basis of the assumption of a *-j-.

Second, there are counterexamples with the regular reflexes of velar plus *j, e.g. *bičb*. Here again, examples are so few as to invite investigators to look for ways to explain them away on a case-by-case basis.

Finally there is the problem of explaining what exactly happened in phonetic and phonological terms, because if, say, Vsg **otikje* yielded *otbče*, how did Lsg **otikjoi* come to yield *otbci* (or rather **otbcě*, with Meillet's -ě-)?

Despite the difficulties, Brugmann's proposal was accepted by several scholars (e.g. Jagić 1901: 128-129) and kept artificially alive by Leskien's influential *Handbuch* (1898: 47 = 1910: 55 and all later editions until 1990). It was to be the subject of a fairly extensive literature, for which see further section 5.2.

4. Descent into chaos (1921-1929)

Once a viable solution to a given problem is available, one expects most investigators to move on because there is no percentage in staying. Of course there is always a chance an even better solution will turn up, or that new factual material or implications not previously perceived may be discovered; no problem is ever definitively solved. However, that does not change the fact that the presence of a viable solution creates a new situation which will normally result in a cessation of research efforts.

Yet in the case of the progressive palatalization that has not happened: ever more investigators have moved in, squandering scarce resources in the process. Obviously the problem is widely perceived as still in need of a solution.

Let us look at some of the facts. It is not in all respects a pretty story and I apologize in advance for the judgmental language that will in some cases be unavoidable to do justice to the situation.

4.1 Belić (1921)

Aleksandar Belić's "Najmlađa (treća) promena zadnjepčanih suglasnika" (1921) is three times as long as Lehr's "Nowsze poglądy" of ten years earlier, which, by the way, Belić does not refer to at all. In many respects the article resembles a preliminary draft that was rushed into print before the author's thinking had matured.

Belić adopts or reinvents (he ignores Oblak 1895 and Jagić 1898) the view that there was a phonetic difference between the outcome of the progressive and that of the second regressive palatalization. Like Jagić (1901), whom he ignores as well, he interprets this as evidence that the former is more recent than the latter: "Često se pojava ovoga reda suglasnika [i.e. the modified velars resulting from the progressive palatalization] smatra samo kao variant drugoga pretvaranja [i.e. of the second regressive palatalization], ali to je pogrešno. Jer razlika između *c*, *dz* i *s*

ove vrste i *c*, *dz* i *s* drugog pretvaranja u tome je što su suglasnici prve grupe umekšani, t.j. potpuno umekšani, dakle *ć*, *dž* i *š*, dok je *c*, *dz* i *s* drugog pretvaranja tvrdo. Da je to tako, vidi se po tome što se *ě* iza *ć*, *dž* i *š* pretvara u *i* (kao što se *o* pretvara u *e*), dokle iza *c*, *dz* i *s* drugog pretvaranja *ě* ostaje neizmenjeno” (19, with examples).

This passage is representative of the article as a whole: not only does Belić fail to credit his predecessors, but he ignores Meillet’s (1900), Lorentz’s (1904), and Zubatý’s (1910) demonstration that Oblak’s evidence proves nothing, although he knows about all three publications, judging by references to them in less crucial contexts. Nor does he discuss Pedersen’s chronology, which generates a sequence opposite to the one he favours. To all intents and purposes Belić acts as if he can decide for himself what is relevant and what is not, irrespective of the facts of the case.

A novel piece of evidence favouring the assumption of a phonetic difference between the progressive and the second regressive palatalization Belić thinks he has found in the West Slavic reflex of **x*, which he believes is *š* and *s* respectively, as in Polish *wsz-* (progressive palatalization) vs. Npl *Włosi* (second regressive palatalization). Examples of -*š*- in the latter type of cases (Lpl *Włoszech*) are explained away as analogical (30).

As others were not slow to point out (Trubetzkoy 1922: 233-234, Nitsch 1926: 45-53), this is a disgraceful blunder: the second regressive palatalization results in *š* in all cases that cannot possibly be analogical (P. *szary*, Cz. *šedivý*) and on the alternations in *Włosi* and *Włoszech* an entire scholarly literature was in existence Belić obviously had no inkling about. This section of Belić’s article, too, is quite representative of much of the text: it is undergraduate stuff that should never have made it into print and that cannot by any stretch of the imagination be construed as contributing anything of value.

Belić favours a formulation of the blocking rule that is virtually identical to the one proposed by Lehr-Splawiński ten years earlier (whom he ignores), and closely related to the one proposed slightly earlier by Zubatý (1910), to whom he does not refer in this context either, although he has actually seen the article (39).

Much space is devoted to exploring the dizzyingly complex morphological consequences of the Oblak/Jagić approach if combined with the blocking principle, in particular the three-way alternations involving final stem consonants (-*k*- vs. soft -*c*- vs. hard -*c*-) all of this results in, e.g.: “Zamenica *sicъ* glasi i *sikъ* i ima u svima padežima svoje promene gde se kod zamenica javlja *ě* po II^{om} pretvaranju: *sicěmъ*, *sicěxъ*, *sicěmъ*, *sicěmi* i sl. uvek tvrdo *c*; međutim u svima drugim oblicima, t. j. u onima gde se kod zamenica ne javlja *ě*, ova zamenica ima osnovu sa mekim *c*, dakle: *sicego*, *sicemou* i t. d. (da je *c* takvo – pokazuje zamena *o* sa *e*); ali pored tih oblika javljaju se i oblici sa *k*: *sikogo*, *sikomou* i sl.” (26-27).

Since no credible foundation has been laid for the assumption that there were two different kinds of *c*, all of this merely illustrates the truth that it is always possible to devise a more complicated explanation for facts that have already been accounted for in a simple way.

With one or two minor exceptions that were mentioned earlier (see section 3.11), everything Belić is saying is unacknowledged old news or known at the time to be wrong or unnecessary, or some combination of those possibilities. Nevertheless, owing to Belić's treatment of the available knowledge the uninformed reader will get the impression that much that is new is being added. Nowadays it is one of the undisputed classics of the field.

4.2 Trubetzkoy (1922)

More than half of Trubetzkoy's "Essai sur la chronologie de certains faits phonétiques du slave commun" (1922) deals directly with the progressive palatalization and much of the remainder indirectly, which makes it the second longest piece of text devoted to the subject to date.

The available knowledge plays only a very minor role in Trubetzkoy's treatment. Whereas Lehr went out of his way to give an idea of what his predecessors had accomplished, and whereas Belić at least went through the motions (although in actual fact picking and choosing), Trubetzkoy ignores everything written since Baudouin de Courtenay (1894) except Meillet (1902: 48-49). He briefly discusses Belić (1921) in a postscript appended afterwards on the request of Meillet (who had promised to publish the text, acting as editor of the *Revue des études slaves*).

Such an extreme departure from ordinary scholarly standards requires an explanation. By a happy coincidence, authoritative information about the genesis of the article is available in Trubetzkoy's autobiographical sketch appended to most editions of *Grundzüge der Phonologie* and his published correspondence with Roman Jakobson and others (1975). Those sources enable one to piece together the following story.

The "Essai" reproduces part of a book Trubetzkoy was working on during the years of the Russian Civil War and which was conceived as a critique of Šaxmatov's monumental *Očerk drevnějšago perioda istorii russkago jazyka* (1915). The book's title was to be *Opyt praistorii slavjanskix jazykov*. The library of Rostov on the Don, where Trubetzkoy was living for most of the time, was useless for his kind of work, forcing him to go ahead without being able to consult the scholarly literature. He left the unfinished manuscript behind in Rostov in the early days of 1920, but after settling in the Bulgarian capital Sofia later that year he started to reconstruct the text from memory. In 1921 he met Antoine Meillet, who proved willing to publish his work, and he decided to publish parts of the *Opyt* as journal articles.

The “Essai” is Trubetzkoy’s first article to reproduce part of the *Opyt*. The main text as printed in the *Revue des études slaves* is dated “Sofia, janvier 1922” (232). During most or all of the time Trubetzkoy was rewriting his text and adapting it for use as a journal article, he was living in Sofia, where the library was suitable for his purposes. However, his life was hectic at the time and he had much else on his mind.²⁰

Given the circumstances one can imagine that Trubetzkoy decided he could not afford to bring the reconstructed Rostov text into line with the published literature, which would have necessitated major revisions, if not worse. Unfortunately a responsible evaluation of the matter is made more complicated by his known practice of covering his tracks, so that “we often learn about Trubetzkoy’s indebtedness to his predecessors by chance” (Lieberman 1991: 361).

Earlier investigators would have added an apologetic footnote explaining that they had not had access to all relevant literature, exactly as would be normal nowadays.²¹

Whatever the external facts, given Trubetzkoy’s *modus operandi* it is not surprising that he rediscovers several known insights both true and false, e.g.:

- Pedersen’s and Meillet’s relaxation of Baudouin de Courtenay’s stress rule (226).
- Lehr-Splawiński’s (possibly even Baudouin de Courtenay’s) insight that **e* does not cause palatalization if it reflects **en* (227).
- The fact that the unmodified velar in **kъnegyŋi* shows that palatalization was blocked by **y*, which can be interpreted in turn by assuming that **y* was still a rounded vowel at the time palatalization took place (227-228). Both insights are old: the former is due to Zubatý, for the latter see Lehr-Splawiński (1911: 144).
- Pedersen’s chronology by which the progressive palatalization precedes the rise of umlaut, which precedes the monophthongization of diphthongs, which precedes the second regressive palatalization (230-231).

The list could be extended.

Uniquely, Trubetzkoy decides to combine Baudouin de Courtenay’s stress rule with Šaxmatov’s blocking principle, which he must have known from the *Očerk*

²⁰ See for all this Trubetzkoy (1967: 277-278, 1975: 2-24, 449-451).

²¹ Treating the reader as an equal would have clashed with the authoritarian intellectual climate of the interwar years, and no doubt also with Trubetzkoy’s personal values as an admirer of Genghis Khan. Though independent minds still clung to more enlightened modes of behaviour, e.g. Kul’bakin (1921: 106) or van Wijk (1922: 261n.), they were hardly typical of the times. (In this connection one may want to consult Taruskin 1997: 395-410 for an outsider’s view of Trubetzkoy’s extralinguistic activities in the early twenties.)

(Šaxmatov 1915: 65-66), to which he refers elsewhere in the article (222) and which constitutes the primary (or sole) motivation of his work on Slavic historical phonology. It is surprising to see the two principles combined in a single reconstruction and one may well wonder if the gain is worth the added complexity, a point Trubetzkoy has nothing to say about.

It turns out Trubetzkoy needs this step to account for the retained velar in such examples as *lbgʷkʷ*, which he attributes to the place of the stress. Ever since the days of Šaxmatov (1896), scholars operating with the blocking principle had assumed that *ʷ* blocked palatalization. However, as Trubetzkoy explains in his postscript, he rejects that assumption because “Vieux-slave *nicʷ* (< **nikʷ*), qui est un mot tout à fait isolé, et par conséquent non susceptible d’avoir subi quelque influence analogique, montre clairement que les gutturales pouvaient se palataliser aussi devant *ī*” (233; the transcription has been slightly simplified).

As others were not slow to point out (e.g. Belić 1922-23: 136), this is an elementary blunder: very far from being “un mot tout à fait isolé”, OCS *nicʷ* is an adjective with an ordinary declension, e.g. Nplm *nici* (frequent), Dsgm *nicu* (Supr.). Hence the stem-final *-c-* may just as easily have arisen by analogy as the one in *otʷcʷ*.

Trubetzkoy agrees with Zubatý that palatalization was blocked not only by **y*, but also by **u* and **o*, or rather by the earlier (pre-monophthongization) sequences **ou* and **oN*, where he assumes that **o* developed into what he calls a “voyelle tendue” **ō* (**ōu*, **ōN*), which differed from normal **o* among other things in blocking palatalization. Trubetzkoy’s principal reason for introducing his **ō* is to express the fact that **u* and **o* retained their individuality in umlaut position; whereas **o* merged with **e*, he assumes that **ō*, though fronted (**ōu*, **ōN*), remained a distinct entity.

Uniquely, Trubetzkoy assumes that the diphthong **oi* blocked palatalization as well. He is forced to introduce this novelty because he realizes that the *-ě-* of *vbśěxʷ* is phonetically regular and has to be shown to be the outcome of a phonetic development. As we have seen (section 3.7), this is not easy if one sticks to Pedersen’s chronology, which puts the progressive palatalization before the rise of umlaut, generating ***vbśixʷ* unless drastic steps are taken to prevent that from happening. The assumption that **oi* blocked the progressive palatalization is such a step: it enables one to conclude that in such cases palatalization took place as a consequence of the second regressive palatalization and hence after the monophthongization of diphthongs.

Although this produces the correct output, it is phonetically awkward and strikingly ad hoc, facts that Trubetzkoy tries to paper over by introducing the following startling piece of special pleading: “Mais on pourrait bien se figurer que, tout en restant relâché, cet *o* comme premier élément de la diphthongue *oi* présentait un plus fort degré de labialisation que dans d’autres positions: c’était peut-être

quelque chose comme *oi*. Après le changement de *eu* en *jou*, la diphthongue *oi* était la seule en slave commun qui comportât deux éléments hétérogènes, le premier labiovélaire (*o*), le second palatal (*i*). Un renforcement «par contraste» du degré de labialisation de *o* n'aurait donc été que fort naturel pour cette diphthongue" (228). Although the idea that the systemic isolation of the diphthong **oi* could have such consequences is completely novel, it is put forward in a tone of business-as-usual, without any attempt at justification apart from the words "fort naturel", substituting rhetoric for the explicit reasoning and exemplification the reader is fully entitled to at this point, reminding us once again of Šćepkin's "offenbar". Trubetzkoy does not explain how it was possible for his *o* to undergo umlaut (which it did: **oi* > **ei* in umlaut position) whereas his **o* resisted it. Note also that [*oi*] constitutes an inauspicious starting point for the development of the reflex of **oi* into a low vowel *ě* which was to take place soon afterwards.

If we would apply to Trubetzkoy the same criteria we would to any other investigator (but I am not suggesting that would be entirely appropriate), we would be forced to conclude that most of what he is saying about the progressive palatalization in this article fails to conform to elementary professional standards. Given Trubetzkoy's unique status in twentieth-century linguistics that may come as a shock, but if one looks at the biographical facts it is exactly what one expects. He was only just beginning to publish about Slavic subjects and was in a tremendous hurry. Moreover, his work on the *Opyt* (which was the source of the "Essai") had been hampered by lack of feedback and access to the scholarly literature, both caused by the circumstances of the Russian Civil War. The publications responsible for his present exalted status were still in the future.

4.3 Follow-up

As we have seen (section 4.2), Meillet prevailed on Trubetzkoy to discuss Belić (1921) briefly in his postscript (1922: 232-234). Belić (1922-23) responded at length, Vondrák (1923-24) tried to see if he could make sense of the situation, Nitsch (1926) criticized both Belić and Trubetzkoy (particularly the former), after which Belić (1928) reacted to Vondrák's ideas and tried to counter Nitsch's criticism.

Whereas Meillet was the immediate cause of the discussion, a subsidiary cause was the problematic quality of the two original texts, which made it all too easy to spot mistakes crying out for rectification. The highlights of the discussion (such as they are) are the following:

Trubetzkoy (1922) defended and clarified one or two components of his own solution (reiterating among other things why he thought Pedersen's chronology to be inevitable), and refuted briefly but conclusively Belić's interpretation of the West Slavic reflexes of palatalized **x* (see section 4.1).

Belić (1922-23) contributed a lengthy and densely written text, in the course of which he pointed out that OCSl *nicь* is an ordinary adjective, thereby conclusively refuting Trubetzkoy's version of the blocking principle, and by that token his reason for sticking to the stress rule (136); Belić also reinvented Meillet's interpretation of umlaut as a synchronic phonotactic rule (135), in that way highlighting the superfluity of Trubetzkoy's assumption that **oi* must have blocked palatalization. Otherwise I cannot find much here that is immediately relevant.

Vondrák (1923-24) criticized Trubetzkoy for his attitude toward the available knowledge (contributing a brief overview of the literature, unfortunately with nearly everything by Meillet lacking, in accordance with what was becoming established practice) and developed a new version of the blocking principle according to which *o* did and *ɔ* did not block palatalization, the former because of the retained velar in *lgota*, the latter because of *nicь*. In addition to completely lacking phonetic plausibility (which Vondrák has nothing to say about), this proposal is difficult to reconcile with the factual material: the velar in *lgota* (instead of phonetically regular ***ldzeta*) can easily be due to analogical restoration on the basis of **lgɔ* or *lgɔkь* (cf. Vaillant 1950: 54). As for *nicь*, contrary to Trubetzkoy, Vondrák knows perfectly well that that word is an ordinary adjective in which the final consonant can be due to analogical levelling; his reasoning why nevertheless in this specific case the modified velar has to be phonetically regular (22-21) is too complex (and, one might add, too obviously misguided) to be discussed here and I refer the reader to the original text.

Nitsch (1926) pointed out that both Belić and Trubetzkoy neglected the available knowledge and paid insufficient attention to the facts in their eagerness to develop grand theoretical schemes; then he went on to refute in great detail Belić's interpretation of the West Slavic reflexes of palatalized **x*.

Belić (1928) argued that Vondrák's version of the blocking rule is morphologically problematic and tried to counter Nitsch's criticism in a way that generates much more heat than light.

All in all, I can find very little in this body of texts that had not been said by 1911. Most of it would not have had to be written had Belić and Trubetzkoy adhered to ordinary scholarly standards for dealing with the available knowledge.

In my view, the entire episode 1921-1928 is for several reasons regrettable.

To begin with, the impression was created that the problem was still poorly understood, thereby encouraging others to move in instead of spending their precious time on more promising subjects.

Furthermore, both Belić and Trubetzkoy exemplified a highly subjective and authoritarian scholarly style, in which investigators no longer treated the reader as an equal and felt free to decide for themselves what was relevant and when they were

willing to let their contributions be constrained by factual considerations and the knowledge already available.

Although in these respects they were to some extent children of their troubled times, compared with the traditional approach their practice is manifestly a step backwards. If carried to extremes it is a recipe for obscurantism because it pulls the carpet of objectivity out from under the feet of the investigators. Whenever that happens, the possibility of reaching consensus on the basis of rational discussion and evaluation of alternatives disappears. Henceforth, rational criticism will tend to be interpreted as aggression or in political terms.

Belić's and Trubetzkoy's reactions to Nitsch's criticism express this attitude all too eloquently: Belić took recourse to bluster and argument by decibel, whereas Trubetzkoy did not react publicly at all, in private attributing Nitsch's objections (which he calls "vypady", with a telling metaphor) to envy among Polish slavists caused by his (Trubetzkoy's) intellectual superiority (1975: 99-100). The idea that Nitsch might just have been interested in furthering the truth by engaging in rational discussion does not seem to have entered either scholar's head.

Finally, much of the discussion was about the question which came first: the progressive or the second regressive palatalization, or, in terms of the chronologies we have been discussing, between Jagić (Belić) and Pedersen (Trubetzkoy). The merits of the classical conception (say Miklosich – Baudouin de Courtenay – Meillet) according to which there was only a single second palatalization, which was triggered partly by preceding and partly by following front vowels, were kept in the background.

Yet with Ockham's razor as ever at the back of one's mind it should have been obvious to all concerned that the first question to ask should always have been: given that the progressive and the second regressive palatalization have exactly the same reflexes, is there anything that forces us nevertheless to attribute them to two different innovations that took place at two chronologically distinguishable stages? The only investigator ever to have seen the problem in this light appears to have been Nitsch: "Nous n'avons, quant à présent du moins, aucune preuve certaine de l'antiquité plus grande de l'une ou de l'autre mutation. Il est donc prématuré de prétendre différencier la seconde palatalisation de la troisième" (Nitsch 1926: 52; for some more discussion see Vermeer 2000: 8; *forthc.*, section 6). Unfortunately Nitsch's impact on the debate appears to have been negligible.

4.4 Jakobson (1929)

It is the purpose of Roman Jakobson's celebrated *Remarques sur l'évolution phonologique du russe* (1929) to apply the principles of Prague School phonology (as recently developed) to the diachronic study of Slavic, in particular:

- (1) Explicit separation between subphonemic (allophonic) and phonemic innovations, with particular attention for the forces that bring about the transition from the former to the latter.
- (2) Explicit consideration of matters having to do with the structure of the phonemic system as a whole.

It is important to realize at the outset that Prague School phonology does not replace neogrammarian methodology, but enriches it by providing novel constraints. Any solution to be acceptable within a Prague School framework has to conform to the same requirements Leskien, Zubatý and Meillet felt bound by. Conversely, if a neogrammarian analysis turns out to resist reinterpretation in Prague School terms, that provides a reason for looking what is the matter and seeing what can be done.

Although the *Remarques* are not designed to cast new light on such specific issues as the progressive palatalization, several points Jakobson brings up do invite comment.

Jakobson takes Trubetzkoy (1922) as his starting point. After what we have seen (section 4.2), there is no need to argue how unfortunate that is. Jakobson appears to have been unaware even of the literature that was produced in the twenties specifically to correct the most striking inadequacies of Trubetzkoy's account (section 4.3).

If I understand Jakobson's difficult text correctly, his adaptation of Trubetzkoy's version of Pedersen's chronology yields the following scheme:

1. *Subphonemic phase of the first regressive palatalization* (19).
2. *Subphonemic phase of the progressive palatalization*. The positions in which palatalized allophones arise is defined as "devant *o* long ou bref et devant *u* bref [i.e. later *o*, *a*, and *ɔ*]" and "après *i* long ou bref [i.e. later *i* or *ɪ*]"²². The allophones are written as k_1 , g_1 , x_1 . Henceforth, two types of palatalized allophones were present in the system at the same time, depending on position (19).
3. *Rise of umlaut, giving rise to the phonemic phase of the first regressive palatalization*. Since k_1 , g_1 , x_1 caused following back vowels to be fronted, both types of allophones now found themselves before front vowels, so that the difference was no longer contextually determined. As a consequence, the allophones of the first regressive palatalization became distinct phonemes, whereas k_1 , g_1 , x_1 kept their allophonic status (22-23).

²² I assume that the absence of the nasal vowel (or its predecessor) in the formulation of the vowels triggering palatalization is accidental.

4. *Elimination of the diphthong *oi.* This point is handled very differently from all earlier reconstructions, including Trubetzkoy's. The diphthong is held to have been metathesized to *io, which changed into *ie as a consequence of umlaut (which apparently was present as a synchronic rule, quite in Meillet's spirit); subsequently, the long monophthong *ē was diphthongized, merging with *ie (28). Quite naturally, velars preceding the new *ie < *oi became subphonemically palatalized, i.e. developed into the allophones k_1, g_1, x_1 ; this was the subphonemic phase of the second regressive palatalization.
5. *Phonemic phase of the progressive and second regressive palatalization.* The allophones k_1, g_1, x_1 became distinct phonemes as a consequences of processes we shall not be going into (31).

In operating with a chronological difference between two phases separated by umlaut and monophthongization, Jakobson copies Lehr-Splawiński's interpretation of Pedersen's chronology in all essentials (section 3.9).

As for the blocking rule, Jakobson adopts Trubetzkoy's version of it in most respects, including his assumption that *o in the sequences *ou and *on developed into a "voyelle tendue" written as *o (cf. Trubetzkoy 1922: 224). Only Jakobson's treatment of *oi is different: whereas Trubetzkoy assumed that the diphthong blocked palatalization by turning into "peut-être quelque chose comme "oi" (see further section 4.2), there is no trace of any "oi in Jakobson; instead we find only *oi (cf. in particular 24). Since *o is held not to have blocked palatalization and since the allophones k_1, g_1, x_1 cause *oi to undergo umlaut (> *ei), we end up with *-i, e.g. **vbsix̄. As we know by now, this is the undesirable output that is so difficult to avoid if one sticks to Pedersen's chronology (section 3.7), a fact that provides the motivation for Trubetzkoy's desperate assumption that *-oi blocked palatalization (section 4.2).

Compared with earlier treatments, Jakobson's interpretation of the subsequent development of *oi in terms of a metathesis (instead of the traditional monophthongization) is a striking novelty. Despite the requirement that strong claims need strong evidence, Jakobson does not tell in what way his metathesis improves on the traditional view. In an important respect it is a step backwards: whereas in the traditional view the merger of *oi with *ē can be understood as a straightforward consequence of the monophthongization (which is part of the tendency towards open syllables), the diphthongization of *ē which has now to be assumed to account for the merger with the reflex of *oi lacks motivation altogether and is completely isolated. If the metathesis is intended to generate Meillet's -ě- (i.e. as an alternative to Trubetzkoy's "oi), the mechanism remains unspecified and irretrievable on the basis of Jakobson's text, at least for me.

Jakobson does not mention the stress rule, which Trubetzkoy needs to account for the retained velar in examples like *lbg̃k̃* (i.e. to prevent it from surfacing as **l̃dz̃k̃, R. **l̃z̃'kij). In Jakobson's scheme these examples remain unex-

plained. The defect could easily have been remedied by including *ʷ among the blocking vowels, as was done earlier by Zubatý, Lehr-Splawiński and Belić (none of whom Jakobson had cared to read) and which renders the entire development more symmetrical by showing that the short *u that was to yield *ʷ is treated in exactly the same way as its long counterpart *ū (later *y).

The theory about the progressive palatalization given in the *Remarques* is woefully inadequate. It falls short of Trubetzkoy's (already flawed) account, on which it is allegedly based, by failing to generate Meillet's -ě- and the retained velar in examples like *lbgьkь*. It introduces several novelties without any attempt at justification, shifting the burden of proof onto the reader, where it does not belong. The text is so atrociously written that the reader often has to guess at the author's intentions. Given the purpose of the *Remarques*, all this may not have mattered so much at the time, but from a wider perspective it is regrettable because of the immense influence Jakobson has turned out to exert on later generations of slavists.

4.5 The Jakobsonian fallacy

Most of the impact of the *Remarques* on later thinking about Slavic historical phonology does not concern the progressive palatalization, hence falls outside the scope of the present contribution. There are however one or two exceptions, the most important of which has to do with a certain type of logic that can be illustrated by the following step in Jakobson's reasoning.

Although Jakobson starts from Trubetzkoy's reconstruction, we have seen that he does modify it here and there, often tacitly. But sometimes he provides argumentation and one of those cases illustrates the type of logic I have in mind.

Trubetzkoy argues that before the reflex of early Slavic long *ū lost its labialization (i.e. turned into y), umlauted *ū must have been something like an *ū̄, which merged with i (from earlier *ī and *ei) when labialization was lost (1922: 223).²³

Jakobson rejects this part of Trubetzkoy's scheme, assuming instead that umlauted *ū̄ merged with ī immediately upon the rise of umlaut. What is important here is the type of logic he takes recourse to: "on ne possède aucune donnée qui puisse faire supposer que le représentant slave commun de ū après palatale – ī n'ait pris naissance qu'à l'époque de la délabialisation des voyelles longues, et

²³ Obvious as this may seem, it provides a welcome piece of background to the progressive palatalization by showing how it was possible that i from *ū with umlaut did not trigger palatalization despite the fact that at the stage involved, umlaut was already present in the system as a synchronic rule. We may assume that the vowel preceding the velar in examples like *bližika* was [ū̄]-like (see section 3.9). Although Trubetzkoy is unlikely to have been aware of that, he was the first to make this point explicit.

qu'entre \bar{u} et \bar{i} il y [a] ait eu une phase intermédiaire $\bar{ü}$. L'hypothèse d'un $\bar{ü}$ substituée à l'opposition binaire $\bar{i} - \bar{u}$ une triple série $\bar{i} - \bar{ü} - \bar{u}$, et un pareil phénomène constituerait en protoslave un fait isolé, en contradiction avec toute la structure du système phonologique de cette langue" (20).

What Jakobson objects to is the fact that $\bar{ü}$ is part of a triple series, whereas all other vowels are part of series comprising only two elements, either of the type e/o or of the type \bar{o}/o .

In this passage much goes wrong in addition to the point of logic that is at issue here. Since it is the latter that is most important, let us get the former out of the way first.

To begin with, Jakobson has not first shown that there is anything remarkable about a triple series of high vowels $\bar{i}/\bar{ü}/\bar{u}$ in systems no other triple series are present in. Yet that step cannot be skipped because vowel height plays an important role here. Consider, for instance, any triangular vowel system in which a single low vowel a coexists with one or more series of the type e/o and i/u . Such systems are extremely common. Obviously, the single low vowel a is in a sense isolated, but equally obviously it would be silly to deny its existence by arguing that if it existed, it would constitute an isolated fact contradicted by the structure of the phonological system. Along the same lines, systems having a triple series only among the high vowels are not all that rare, e.g. Medieval Greek, Albanian, much of Occitan, and quite a few others.²⁴ If Jakobson were right, such systems could not exist. Since obviously they do exist, his argument, which may well be appropriate in other types of cases, is inappropriate in this case.

Furthermore, Jakobson has misunderstood the phonological implications of Trubetzkoy's reconstruction. As Pavle Ivić was the first to point out (1965 : 45), Trubetzkoy's \bar{o} is a mere allophone of o because it is conditioned by the presence of a following tautosyllabic u or nasal consonant. For the same reason, the dot under \bar{o} has no phonological significance. It follows that in reality the two series e/o and \bar{o}/o together make up a triple series of mid vowels consisting of long and short $e/\bar{o}/o$, so that the series $\bar{i}/\bar{ü}/\bar{u}$ is not isolated after all. This shows that Jakobson's problem (to the extent that it is a problem) does not exist.

Similarly, $\bar{ü}$ is not a distinct phoneme either, but an allophone of $*\bar{u}$ conditioned by a preceding $*j$ or other palatal consonant. From a Prague School point of view the $\bar{ü}$ has no separate existence: it is just an $*\bar{u}$, which goes on being an $*\bar{u}$ (i.e. a distinct phoneme) after the rise of umlaut. The same holds for long and

²⁴ Jakobson may not have known about Albanian or Occitan, but he must have been aware of Medieval Greek, which was constantly being discussed by Moscow slavists in connection with Fortunatov's theory that the Old Church Slavonic letter jo rendered [ü]. It is even more difficult to believe that he was not aware of Old Church Slavonic, with its series of high vowels containing one additional element as compared with the lower series.

short \ddot{o} (Trubetzkoy's \ddot{o}), which are allophones of long and short o . This too shows that Jakobson's problem does not exist.

Finally, introducing the point by apodictically stating that "on ne possède aucune donnée ..." is a transparent attempt to avoid discussing Trubetzkoy's specific reasons for his reconstruction by a rhetorical ploy smacking of Ščepkin's "offenbar".

All of that, feeble as it is, is not what is really important here. That is the logical sleight of hand Jakobson uses: he objects to Trubetzkoy's \bar{i} on the grounds that "un pareil phénomène constituerait en protoslave un fait isolé, en contradiction avec toute la structure du système phonologique de cette langue". What he does here is refute a statement about a low-level detail by a statement of a higher level of generality. It should be obvious that this logic is fallacious. If two statements clash, either one of them can be at fault and we should not pretend that we know which one before we have looked into the matter. What I call the Jakobsonian fallacy consists in the view that in such cases the general statement refutes the specific one. It is the reverse of the Popperian view that it is the specific statement that refutes the general.

It goes without saying that observations about the structure of the phonemic system constitute an important source of information if used properly. To give an example, one may wonder why the long \bar{i} lacks a short counterpart, whereas all other vowels occur in pairs.

At this point, Jakobson might have been tempted to conclude that the \bar{i} did not really exist, arguing that it is isolated and as such contradicted by the structure of the phonological system. But if he would have looked into the matter he would have discovered that the absence of a short counterpart of \bar{i} is a consequence of Trubetzkoy's view that $*\bar{v}$ did not block the progressive palatalization. As we have seen, in Trubetzkoy's reconstruction the blocking vowels (described as "voyelles tendues") are held to have developed into rounded front vowels in umlaut position, whereas non-blocking vowels ("voyelles non-tendues") were treated differently. Since Trubetzkoy's view that \bar{v} is a non-blocking vowel is unwarranted (see section 4.2), it has to be corrected anyhow by moving the predecessor of $*\bar{v}$ from the group of "voyelles non-tendues" to that of the "voyelles tendues", automatically providing Trubetzkoy's \bar{i} with a short counterpart $*\bar{i}$ and removing the anomaly. On closer examination, the asymmetry exposes a weakness in Trubetzkoy's account which does not consist in the presence of \bar{i} , but in the absence of \bar{i} and which comes to light only if one decides to examine Trubetzkoy's theory in terms of the low-level linguistic facts it has been designed to explain rather than in terms of its overall structure.

4.6 The apotheosis of the Jakobsonian fallacy: Martinet's chronology

In 1952, Martinet argued that the progressive palatalization must have preceded the *first* regressive palatalization. This was completely new.

As we have seen, the traditional view held that the progressive palatalization was just part of the same process as the second regressive palatalization (Miklosich, Sobolevskij, Baudouin de Courtenay, Meillet, etc.). Then Jagić proposed that the progressive palatalization came later, unfortunately on inadequate grounds (section 3.4). Then Pedersen argued on much more solid grounds that the progressive palatalization preceded the rise of umlaut and hence also the monophthongization of diphthongs, which made it considerably earlier than the second regressive palatalization (section 3.6). However, since Pedersen's chronology did not generate Meillet's *-ě-* (section 3.7), it could not be the whole story. Subsequently many of those who nevertheless stuck to Pedersen's chronology limited it to the first or subphonemic phase, putting the second or phonemic phase simultaneous with that of the second regressive palatalization, in non-trivial respects combining Pedersen's chronology with the traditional view, but leaving Meillet's *-ě-* unaccounted for (sections 3.9, 4.2).

Now Martinet put the progressive palatalization before the first regressive palatalization. His reasoning is a veritable triumph of the Jakobsonian fallacy:

“Quant à ce qu'on a pu autrefois grouper sous la rubrique de seconde palatalisation des «gutturales», à savoir le passage de **k* à *c* dans *otbcb*, et la même évolution dans *cělč*, il convient sans doute d'y voir deux phénomènes différents qui n'ont en commun que l'identité finale de certains de leur produits. [...] il faut admettre, dans le cas de *otbcb*, une action progressive de la voyelle [i], et un tel phénomène s'intègre mal dans le cadre d'un affaiblissement généralisé de l'implosion, affaiblissement qui se traduit par une anticipation des articulations. Il nous faut donc supposer que la tendance aux syllabes ouvertes ne s'est manifestée qu'après achèvement de la palatalisation qui a donné *otbcb*, qu'on veuille y voir une seconde palatalisation ou une modalité de la première” (162 = Martinet 1955: 366-367).

Martinet does not discuss any specifics, not even Meillet's *-ě-*, or the chronology and motivation of the subsequent merger of the outcome of the progressive palatalization with that of the second regressive palatalization which his scheme entails. His phonetics is much too rudimentary to be compelling: if it is true that the law of open syllables can be validly attributed to an “affaiblissement généralisé de l'implosion”, it remains unclear in what way such a weakening would prevent a high front vowel from influencing the articulation of a following velar stop. And if it is true that a “generalized weakening of implosion” is the same thing as an “anticipation of articulations”, it remains unclear why one might not assume that the

palatalization was triggered by anticipation of the stop articulation of the velars during the high front vowel.

When the chips are down, Martinet's chronology is as much of a shot in the dark as Baudouin de Courtenay's stress rule. And like the stress rule, it has been considerably more successful than it deserves given its skimpy intellectual pedigree. Although it appears to have fallen flat in the Old World, it has proved very influential in the United States, where it became associated with generativist thinking after Jakobson's phonological theory had been incorporated into the generative enterprise (for the results of which see in particular Chomsky and Halle 1968).

Two decades after Martinet, Robert Channon expressed the same view in the following words:

"Slavic underwent a period in which the syllable apparently constituted the basic unit of word-construction, and, in connection with this, there was a rigid rule that palatality or lack of palatality had to be uniform for all segments in a given syllable. This restriction did not, however, apply across syllable boundaries, at least not with any great degree of regularity. The only recognized regular instances that we have of influence of one syllable upon another are the examples of the progressive palatalization, which took place only after a very restricted group of front vowels. [...] In order to explain how a vowel could have caused palatalization of a consonant following it, and belonging to a different syllable, we must assume that the syllable was not the basic unit of word-construction, i.e., that the principle of syllabic synharmony was not operative at the time of the development in question. This means that the progressive palatalization must have occurred either before or after the time of syllabic synharmony. However, it has already been established that the progressive palatalization occurred before the monophthongization of diphthongs; this latter process, in turn, had to have occurred before the end of syllabic synharmony, since it caused the palatalization of preceding velars. Therefore, the progressive palatalization could not have occurred after the period of syllabic synharmony, and must have occurred before it." (Channon 1972: 46).

Astonishingly, Channon does not refer to Martinet.²⁵ Like him, Channon pays very little attention to specific facts and difficulties. He even omits Martinet's phonetics, relying purely on logic, or rather on a verbal trick: he redefines the "period of syllabic synharmony" as "a period in which the syllable apparently consti-

²⁵ It stretches the imagination to believe that Channon and the other American linguists sharing his views did not know they were repeating a conception that had been introduced by Martinet. Not only did Martinet's article belong to the years he was close with Jakobson, but it was reprinted in his *Économie des changements phonétiques* (1955), which was avidly being read all over the world during the late fifties and early sixties.

tuted the basic unit of word-construction”, from which it is deduced that during that period “influence of one syllable upon another” was impossible. That is a non-sequitur, as becomes clear if we transpose it to another domain. If, say, neurons constitute the basic unit of brain-construction, it does not follow that influence of one neuron upon another is impossible. Or, more flippantly, if we define musicians as the basic unit of orchestra-construction, it does not follow that musicians do not influence one another. The assumption that syllables could not influence one another during the period of syllabic synharmony is an empirical matter one cannot hope to solve by resorting to such purely verbal expedients.²⁶

Channon’s account was subsequently criticized by Lars Steensland, who drew attention (among other things) to the defectiveness of Channon’s logic and to the problem of Meillet’s *-ě-* (1975: 96, 99-100).

Unaccountably ignoring Steensland’s strictures, Horace Lunt expressed the Martinet argument some years later in the following words:

“(…) BdC [i.e. the progressive palatalization] invariably operates across a syllabic boundary. In contrast, KI and KAI [i.e. the first and second regressive palatalization respectively] and most other context-determined processes of prehistoric Slavic are regressive and intrasyllabic, some indeed being caused by a shift of syllabic boundary. The distribution of vowel or zero determined by “the fall of the jers” within the OCS period is regressive, but conditioned by sequences of syllables; most of the immediate adjustments to the phonetic loss of weak jers in the early historical period are again regressive and intrasyllabic (i.e. within the newly-formed syllables). Progressive assimilation and changes transcending syllabic boundaries are thus either very old or else after about 1000 and therefore no longer Common Slavic” (Lunt 1981: 25).

Humanly speaking it is inconceivable that Lunt, whose preface is dated “May 5, 1978” (1981:10), missed Steensland’s critique of Channon, which appeared in 1975 in just about the most prominent journal of Slavic studies put out in the United States.

In Lunt’s text, the crucial idea remains implicit. That is the unsubstantiated claim that “changes transcending syllabic boundaries” were impossible during the period Proto-Slavic was carrying through so many intrasyllabic innovations.

Note however that Lunt differs fundamentally from Martinet and Channon in understanding that the principal problem is how to reconcile Martinet’s argument with the specifics. His book is an attempt to investigate all relevant factual issues to see if that is possible.

²⁶ Lawrence Newman, after briefly characterizing the articulatory movements involved, observes: “There is little reason to believe that this could not have occurred also across a syllable boundary” (1973: 253). For some further discussion see Vermeer (2000: 15-17).

Lunt's book was critically reviewed by several scholars, notably Beljaev (1984) and Kortlandt (1984). And whereas Steensland's critique of Channon had been ignored even by Lunt, Kortlandt's gave rise to a lively discussion, cf. in particular Lunt (1985, 1987) and Kortlandt (1991).²⁷

The assumption that during the period of syllabic synharmony, changes transcending syllable boundaries were impossible has all the hallmarks of an urban myth in the sense of a notion that is widely believed to be true though lacking any foundation in fact. I have noticed that if one brings up the Slavic palatalizations in informal settings, Martinet's argument always crops up after a few minutes; people tend to react with disbelief if one tells them that nobody has ever taken the trouble to substantiate it in a way that goes beyond Martinet's phonetics and Channon's logic. It is clear that it has a powerful intuitive appeal, not unlike Baudouin de Courtenay's stress rule, which coming as it did on the heels of Verner's law just *had* to be true.

5. Excursus on other post-1930 developments

A comprehensive description and evaluation of later developments is not intended in this contribution. The following points are added only to give a very approximate idea of what has been left out.

5.1 Relative chronology

Ever since Miklosich, considerations of relative chronology have been uppermost in the thinking of scholars involved in the Slavic palatalizations. Even before the First World War, quite a few important chronological implications of the progressive palatalization were made explicit.

After the war, the elaboration of relative chronological schemes became the concern of the linguists of the Prague School (Trubetzkoy 1922, Jakobson 1929). Unfortunately their understanding of the specific issues raised by the progressive palatalization was often defective and their scholarly style, which tended to favour grand theoretical schemes over attention to detail, was poorly suited for coping with the intricacies of the progressive palatalization (see sections 4.2 and 4.4).

Large-scale chronologies encompassing ever more of the phonological history of Slavic and routinely including the progressive palatalization were elaborated subsequently by several investigators who gradually managed to overcome Trubetzkoy's and Jakobson's limitations, notably Milewski (1932: 243-254), Mareš (1956: 464-468 = 1965a: 36-43 = 1965b: 32-38, cf. 1969: 61), Ebeling (1963), and Kortlandt (1979: 166, 1982: 184). With a single exception, these scholars all follow Trubetzkoy and Jakobson in basically continuing Lehr-

²⁷ A critical evaluation of the discussion would lead too far here.

Splawiński (1911), ignoring Meillet's -ě- or at best accounting for it in an ad hoc fashion. The exception is Kortlandt, who starts from Vaillant (1950: 53-55), continuing in that way the tradition of Meillet (1900).

5.2 Odds and ends

The Brugmann view (see section 3.12) proved particularly tenacious in Scandinavia.

It was the subject of the first monograph to be devoted entirely to the progressive palatalization (Knutsson 1926). Even for those who are not convinced by the Brugmann view, the book is indispensable because it contains a very detailed discussion of the literature (3-47). The present contribution is deeply indebted to it.

Knutsson's fellow countryman Ekblom (1935) also wrote a monograph on it, which gave rise to a leisurely discussion that was still going on in the late sixties, e.g. Milewski (1937), Ekblom (1951), Machek (1954), Thümmel (1967), Jeżowa (1968). As far as I am aware at the moment (but there may be much I have missed), the most recent statement of the Brugmann view is Thümmel (1967), whereas Jeżowa (1968: 182-194) is easily the most comprehensive refutation.

In 1982, Andrej Anatol'evič Zaliznjak argued convincingly that the oldest textual material from Novgorod implies the existence of an early North Russian dialect of Common Slavic which never carried through the second regressive palatalization and in which the progressive palatalization is attested systematically only in the case of *k. Zaliznjak's discovery, which textual material unearthed since then has only confirmed, has given rise to a fairly extensive literature and several proposals to account for it. For further discussion see Vermeer (2000: 17-22, with references to the literature).

Finally it is important to realize that some authors have approached the matter from completely different angles. Some have assumed that the role of morphological factors was far greater than is usually thought, e.g. Otrębski (1948, 1970), Hamm (1969), Beljaev (1986). Others have experimented with strikingly different conditioning factors, e.g. Gunnar Jacobsson (1974). There is probably more of this. Most of it has attracted little or no attention and requires being evaluated.

6. What went wrong?

Two periods can be distinguished: 1879-1911 (Potebnja-Lehr-Splawiński) and 1921-1929 (Belić-Jakobson) plus aftermath (Martinet-Channon-Lunt). The former produced the outlines of a viable solution, the latter one or two solutions to problems of detail, at the same time relinquishing several fundamental insights by committing much of the earlier literature to a *damnatio memoriae*. It also gave rise to the phenomenon of the Jakobsonian fallacy and laid the foundation for the

myth that changes transcending syllable boundaries were impossible during the period of syllabic synharmony.

6.1 The first period

Although the first period produced more or less the growth of knowledge one could reasonably expect (see section 3.10), everything did not go smoothly all the time.

To begin with, two lengthy delays (each of nearly fifteen years) were caused first by what I have called “Baudouin’s embargo” (1880-1893/1894), and then by Šaxmatov’s blocking principle sinking to the bottom without anybody coming to its rescue (1896-1910/1911). The former delay was the product of the courteous attitude of Baudouin’s colleagues towards what they had been given to understand was his spiritual property; the latter is more difficult to interpret for lack of relevant information, but may well have had a similar background. Given the pace at which developments moved at the time, these delays were regrettable.

Furthermore, Baudouin de Courtenay’s stress rule was a preliminary idea with no particular claim to respectability, yet it was treated with the utmost seriousness, despite the almost immediate arrival of a plausible alternative (Šaxmatov’s blocking principle, 1896).

More generally, a certain reluctance of scholars at the time to acknowledge the difference between serious statements and preliminary claims did much to obscure the issues and may well have slowed down progress.

A final problem has to do with the treatment of the progressive palatalization in August Leskien’s celebrated *Handbuch der altpulgarischen (altkirchenslawischen) Sprache*, the first edition of which appeared in 1871.

During the entire first period, the *Handbuch* was one of the very few solid introductions available, perhaps the only one. New editions appeared at brief intervals and Leskien did his best each time to incorporate new developments, though usually hampered by the necessity to avoid too radical changes to keep down costs.

From the second edition onwards (1886), Leskien concentrated on the most archaic texts, some of which had only recently become available, thereby paving the way for a more accurate description of the oldest attainable stage of the language.

In the third edition, Leskien added a separate paragraph about the progressive palatalization (§30a) in which he states that “Die Ursache des Vorganges ist bisher nicht sicher erkannt”, referring the reader to Baudouin de Courtenay (1894) and Brugmann/Delbrück (1897: 291) for more information (1898: 47).

Although this made perfectly good sense at the time, developments were taking place so fast that the text was hopelessly out of date by the time the fourth edition became due, as Lorentz argued conclusively in 1904. Unfortunately, nothing had been done even by the time the fifth edition appeared (1910). Since the book

was reprinted posthumously virtually without change until 1990, we entered the third millennium with an introductory text that had been obsolete for nearly a century.

Whereas adequate updating of the *Handbuch* may have been prevented by financial considerations, Leskien could have made a new beginning in the *Grammatik* he published in 1909. Yet his treatment there is the same as in the *Handbuch* apart from the precise wording: “Eine irgendwie sichere Erklärung [...] fehlt”, followed by the familiar references to Baudouin de Courtenay (1894) and Brugmann/Delbrück (1897: 291), by now completely out of touch with reality. Unaccountably, the treatment of the pronoun *vsb* still ignores Lorentz’s comments. All of this was bound to mislead the intended audience. Reviewers (e.g. Meillet 1910) did not hesitate to point this out, but to no avail.²⁸

Disturbing factors were so numerous and varied that little may have sufficed for the building to collapse. The strain was beginning to show as early as Lehr-Splawiński (1911), who appears to have missed everything Meillet had written on the subject, with unpleasant consequences for his own reconstruction.

But at least there *was* a building. Making allowance for human frailty and Murphy’s law, the first period was a success in that it produced a certain amount of non-trivial knowledge that was more or less kept in circulation and which promised more.

What one would normally have expected next was one or two articles, or perhaps a doctoral dissertation, to take care of residual problems, after which the solution could have been incorporated in the handbooks to enlighten future generations of slavists and enable them to stand on their predecessors’ shoulders. That was not to be.

6.2 The second period

The beginning of the second period was marked by two ambitious publications (Belić 1921 and Trubetzkoy 1922), neither of which conformed to ordinary professional and qualitative criteria. Measured by pre-war standards both articles were deficient on three levels at once:

1. Existing knowledge was ignored or misunderstood.
2. Elementary errors were committed in evaluating straightforward facts.
3. Ordinary methodological standards were trampled underfoot.

On all three counts Trubetzkoy reached even lower depths than Belić: he was unaware of virtually the entire literature, managed to turn an Old Church Slavonic adjective into an adverb (with dire consequences for his – and Jakobson’s – the-

²⁸ Vondrák’s handbooks (1906: 266-268, 1912: 305-307) are even less helpful.

ory), and displayed an ominous predilection for circular reasoning and ad hoc solutions.

The ensuing discussion failed to bring about the clarification naïve onlookers might have felt entitled to. In the meantime Meillet published an authoritative handbook (*Le slave commun*, 1924) in which the entire pre-Belić literature was thrown onto the scrapheap of history, including Meillet's own contributions and such landmarks as Zubatý (1910) and Lehr-Splawiński (1911).²⁹

Since the older literature had not been superseded yet, some of the most important insights receded behind the horizon of all but the most persistent investigators. Zubatý's and Lehr-Splawiński's work remained alive only in parts of the world where West Slavic languages were spoken (cf., e.g., Vondrák 1923-24: 353-355). Meillet's contributions were forgotten by everybody except his co-author in the second edition of *Le slave commun* (André Vaillant), who was to regurgitate them in his *Grammaire comparée*, unfortunately without adding sufficient background to compensate for the near-complete absence of references to the literature, thereby quite needlessly feeding the confusion. For all practical purposes, the net amount of available knowledge decreased.

6.3 Hitting rock bottom

By the time Jakobson wrote his *Remarques sur l'évolution phonologique du russe*, standards had slipped so dramatically that he could afford to narrow his horizon to a single highly problematic article written by a personal friend. Those still aware of the real complexity of the problem were reduced to the ungrateful role of ones calling in the desert practicing damage control. Examples are Knutsson (1926: 3-47), Nitsch (1926), and van Wijk (1931: 67-70), whose combined influence on later scholarship appears to have been nil.

The study of the progressive palatalization has yet to recuperate from the damage inflicted on it by the *Remarques* and may never do so. The thought of what might have happened had Roman Jakobson descended from his cloud for a few hours to peruse Knutsson's overview of the literature brings tears to one's eyes.

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²⁹ I realize that until long after the Second World War, the practice of referring to one's own publications was considered bad form (unless done very discreetly) because it was perceived as a violation of the taboo on boasting. (I vividly recall being shocked as late as the late sixties by Noam Chomsky's constant references to his own work, which struck me as avoidably boastful.) However, Meillet could easily have worked his way around the problem by referring to Lorentz (1904), Zubatý (1910), and Lehr-Splawiński (1911).

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Appendix: Some personalities

Dozens of scholars have materially contributed (or discontributed) to our knowledge of the progressive palatalization. What follows is an alphabetical list of the most important ones treated in the present contribution. The list concentrates on firsts and similar points.

Baudouin de Courtenay, Jan Niecislaw. Claimed the progressive palatalization in or around 1880, but did not publish until 1893. Attributed the exceptions to the place of the stress. Often credited with views he never held, such as the idea that the progressive and the second regressive palatalization were chronologically distinct. His most lasting contribution is the insight that the progressive palatalization took place at a relatively early stage in the history of the Slavic vowel system.

Belić, Aleksandar. Published a lengthy article in which he committed egregious errors of various kinds, treated his predecessors like dirt, and contributed hardly anything worthwhile (1921), but which was treated by posterity as the first useful publication on the subject since Baudouin de Courtenay (1894). The title of the article (“Najmlađa (treća) promena ...”) may well be the principal source of the unfortunate tradition of referring to the progressive palatalization as the Third.

Brugmann, Karl. Proposed to explain the counterexamples by combining the assumption of a progressive palatalization with the traditional *-j- (Brugmann and Delbrück 1897).

Channon, Robert. The first to publish a defence of Martinet’s chronology in the United States (1972, originally 1965). His overview of the literature, though useful, is misleadingly patchy, leaving aside as it does not only everything by Meillet (in accordance with tradition), but also such fundamental contributions as Baudouin de Courtenay (1893), Pedersen (1905), Milewski (1932, 1937), Vaillant (1950), and Martinet (1952, 1955).

Ekblom, Richard. Wrote two brief monographs in defense of the Brugmann view (1935, 1951).

Jagić, Vatroslav. The first to argue explicitly that the progressive palatalization is indeed a progressive palatalization (1887). Also the first to argue that the progressive and the second regressive palatalization are chronologically distinct (1901).

Jakobson, Roman. Wrote the *Remarques sur l'évolution phonologique du russe* (1929), which contains important theoretical advances, but is fatally flawed in its treatment of the progressive palatalization.

Jeżowa, Maria. Discussed the Brugmann view so thoroughly that it has not been heard of since (1968). Her overview of the literature is the best since Knutsson (1926) and van Wijk (1931), but misleadingly ignores everything written in French apart from Meillet (1910), Trubetzkoy (1922), and Nitsch (1926).

Knutsson, Knut. Author of the first monograph on the progressive palatalization, containing an indispensable overview of the literature and a reasoned defence of the Brugmann view (1926).

Kortlandt, Frederik. Has published what is so far the only large-scale chronology of Slavic phonological history to incorporate the classical conception of the progressive palatalization instead of some version of Pedersen's chronology (1979, 1982).

Kretschmer, Paul. Outsider who revolutionized ideas about the prehistory of the Slavic vowel system (1905).

Lehr-Splawiński, Tadeusz. Wrote an article (1911) containing a version of the blocking rule that was to become influential under Belić's name and an ingenious interpretation of Pedersen's chronology which became very influential after it had been pilfered by Jakobson. Has been treated shabbily by posterity.

Leskien, August. Neglected (despite Lorentz 1904) to let paragraph 30a of his *Handbuch* evolve with the times and went on explaining that the paradigm of *vbsb* consists of a chaotic jumble of distinct stems years after that had ceased making sense. Cannot however be held responsible for the continued posthumous use of the *Handbuch* in teaching, which was not at all in his spirit.

Lunt, Horace G. Has attempted to reconcile Martinet's chronology with the facts (1981).

Martinet, André. Used the Jakobsonian fallacy to argue that the progressive palatalization precedes the first regressive palatalization (1952).

Meillet, Antoine. Argued conclusively that the reflex of *-oi after velars modified by the progressive palatalization is -ě ("Meillet's -ě-"); refuted Pedersen's chronology before the event by arguing that umlaut may have been a synchronic phonotactic rule (1900). Showed that the progressive palatalization must have been earlier than the merger of umlauted *y* with *i* (1905). Showed how difficult it is to

reconcile the alternation in *кнѣдзь* vs. **кнѣгыи* with Baudouin de Courtenay's stress rule (1910). Published Trubetzkoy's "Essai" (1922) despite its obvious defects and prevailed on its author to enter into a discussion with Belić. Was the first to discuss factual material relevant to Trubetzkoy's conjecture that *i* does not trigger palatalization if it reflects **ei* (1934, perhaps together with Vaillant). Contributed more than anybody else, but spoiled everything in the end by committing the older literature (including his own work) to the dustbin in *Le slave commun* (1924, 1934), making it appear that serious study of the subject starts only with Belić (1921) and Trubetzkoy (1922).

Nitsch, Kazimierz. Tried to engage Belić and Trubetzkoy in a rational discussion, but might just as well not have bothered. Explained why it is rational to start from the assumption that the progressive and the second regressive palatalization are two sides of the same innovation (1926).

Oblak, Vatroslav. Used an old observation by Potebnja to argue that the progressive and the second regressive palatalization yield phonetically different outcomes (1895).

Pedersen, Holger. Explained the fact that paradigms such as those of *отць* and *онца* have soft endings by devising a chronology that has remained influential despite its failure to account for Meillet's *-ě-* (1905).

Potebnja, Aleksandr Afanas'evič. Appears to have been the first to publish the insight that the second regressive palatalization is triggered by front vowels only if they reflect earlier **oi* (1879).

Šaxmatov, Aleksej Aleksandrovič. Discovered the blocking principle (1896), but did not follow it up until it was too late. Exasperated Trubetzkoy by the methodological quirks of his *Očerki* (1915), thereby playing midwife to the latter's *Opyt*, from which the ill-fated "Essai" was scavenged.

Sobolevskij, Aleksej Ivanovič. The first to publish a collection of examples displaying the effects of the progressive palatalization (1889, 1891).

Steenland, Lars. Pointed out the fallaciousness of the Jakobsonian fallacy (1975) and was ignored by all concerned.

Thümmel, Wolf. May well have been the last to defend the Brugmann view (1967).

Trubetzkoy, Nikolaj Sergeevič. Published an article on the progressive palatalization in which he displayed near-total ignorance of the subject (1922), and got away with it.

Vaillant, André. Assisted Meillet in preparing the second edition of *Le slave commun* (1934), serving in that way as the vessel through which a number of otherwise forgotten insights reached the post-war period (1950).

Zaliznjak, Andrej Anatol'evič. Has argued that the North Russian dialect of Common Slavic never carried through the second regressive palatalization at all and underwent the progressive palatalization only in the case of **k* (1982).

Zubaty, Josef. Made Meillet's *-ě-* incontrovertible by the most complex piece of reasoning in the entire debate; rediscovered and updated the blocking rule (1910). Has been treated shabbily by posterity.