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STUDYING THE GALICIAN-VOLHYNIAN CHRONICLE. RESPONSE TO DARIUSZ DĄBROWSKI*

The *Galician-Volhynian Chronicle* covering the events of the thirteenth century in southern Rus' is a notoriously difficult text to understand and interpret, especially in its first, 'Galician' part. Attempts to translate its text into modern languages, particularly those that strive to stay faithful to the original, demonstrate this most evidently. Their authors must insert numerous explanatory glosses (usually marked by square brackets) to make the translation legible. In places, the amount of text taken in square brackets almost exceeds that found in the original. For anyone trying to make sense of the *Galician-Volhynian Chronicle*, it is clear that we are dealing with a text of complex makeup that must be understood and decoded before it can be safely used as a historical source. The most obvious hypothesis that may account for these traits is that the chronicle's text underwent a complex history and may have evolved through a series of consecutive editorial episodes, which, having piled one on top of the other, produced the observable effect.

That was the premise for the project of text-critical inquiry into the *Galician-Volhynian Chronicle* launched in 2014 under the auspices of the Centre for Kyivan Rus' Studies at the Ukrainian Academy of Sciences. For five years, the group of dedicated scholars employed the technique of a close reading of the chronicle's text in its entirety to understand the makeup of individual accounts and, hopefully, based on the knowledge

* Dariusz Dąbrowski, 'Badając Kronikę halicko-wołyńską (Kronikę Romanowiczów). Rozważania na marginesie opracowania Oleksija Tołoczki', *KH*, 130, 2023, 2, pp. 279–97.

gained, to develop a plausible scenario of the text's evolution. The results of the study were published in 2021 as *The Galician-Volhynian Chronicle: A Text-Critical Inquiry*, edited by Oleksiy Tolochko.¹ The volume includes the most up-to-date bibliography of studies on the chronicle, a complete concordance to its text, and passage-by-passage commentary, with particular attention to the fragments deemed problematic from a textual perspective. These components of the volume aimed to introduce those who would like to approach the chronicle as a historical source to the complex textual problems involved and to provide them with the tools to interpret the text. The volume also contains an introductory chapter summarising the authors' findings and suggesting a version of the text's history that can be supported by hard textual evidence. In effect, *the Galician-Volhynian Chronicle* is now the most exhaustively read medieval history text, East or West.

Et tu, Brute?

In the Spring of 2023, a review article by Professor Dariusz Dąbrowski of the University of Casimir the Great in Bydgoszcz appeared in *Kwartalnik Historyczny*. The title is intentionally misleading in ascribing to me the work of many scholars. However flattered I might be, I have to decline the honour. In fact, Dariusz Dąbrowski refutes his claim by noting through his article that the book is indeed the result of a collective effort of scholars whom he, with annoying repetitiveness, refers to as 'the Kyiv team' or 'the Ukrainian team'.

The professor from Bydgoszcz declares that the book under his review is fundamentally flawed: it was written 'pod tezę' (p. 381), that is, to prove some preconceived notion the authors had had before embarking on their study and also to exercise in proving some a priori known theories. What sort of 'thesis' this might be, he does not specify. Why would the authors need to prove this particular 'thesis' and not some other, he does not reveal either. However, his assessment presents the book as one large hoax, a mystification, if not an outright academic fraud. This is the most extraordinary claim for which the Polish historian shows no evidence. Coming from a colleague of many years, it is inexplicable. Surely, there might have been a more economical way of cheating than writing a nine-hundred-page book. And surely, there must

¹ *Halytsko-Volynsky litopys: Tekstologia*, ed. Oleksiy Tolochko, Kyiv, 2020. The book has appeared in the Spring of 2021.

have been a more devious way to deceive than to reveal all the primary textual observations for all to see and to judge. Deception should be made of sterner stuff.

Helpfully, Dąbrowski explains how he arrived at his verdict. He discovered that the study's findings are summarised in the research chapter placed at the beginning of the book. If so, they must have existed before the study commenced. This disarmingly naive statement betrays his belief that the investigation and written report on it emerge simultaneously and that the text must mirror every step of research as it develops.

Had the chapter been placed as an afterword to the book, the Polish professor would have had no quarrels with its findings. His statement, moreover, admits to a refreshingly innocent positivistic notion about the nature of the investigation: the results must reveal themselves to an unsuspecting scholar at the very end of the inquiry, once the necessary observations have been made. I do not allow Dąbrowski to be unaware that the vast majority of disciplines, and not only in the sciences, operate by formulating hypotheses and testing them against evidence. Thus, no crime would have been committed even if we had offered some preliminary expectations at the start. But it gets worse. Dąbrowski chose not to report to his readers that the chapter in question contains numerous references to the subsequent commentary section. For anyone, this would be proof that it was the last one written, thus offering the interpretations of the data generated by the inquiry rather than anticipating it.

Dąbrowski finds our results alarmingly unusual (in a positive review, the word would be 'original') and yet also already known, which defies logic. Disappointedly, he does not indicate what sort of *a priori* he has in mind and how we came to possess them. So let me do it in his stead.

A priori

By definition, *a priori* is knowledge that a person obtains and holds independently of experience. Applied to academia, these are beliefs instilled by education, the social environment, or authority. In history, prior assumptions are most often acquired from previous literature on the subject, absorbed without sufficient critical evaluation. So, what are the *a priori*s in the field of chronicle studies that we could have shared with the previous scholarship?

In a vast body of literature on chronicles, one would be hard-pressed to find theoretical discussions on the epistemological foundations of the discipline or the methods employed. True, people often invoke

a specific 'method' (usually dubbed 'Shakhmatov's method'), but they are very vague as to what it entails. The great Russian philologist Aleksei Shakhmatov (1864–1920), credited with inventing the 'method', also remained remarkably indifferent to theoretical reflection, never defining, at least as a set of postulates, the principles guiding his work. What people really mean by referring to 'Shakhmatov's method' is whatever they were able to glimpse over his shoulder and imitate in their own work.

Thus, the chronicle studies operate not as a theoretical discipline but rather as a practical craft achieved by apprenticeship and passed down from generation to generation. It operates not by 'method' but rather stems from certain assumptions about medieval Ruthenian culture that are accepted as self-evident. The most important of these is the overstated value of history and history-writing, which is believed to be at the core of literary activity. Hence, chronicles must have been numerous, produced in quick sequence, and broadly read. Within this image of culture, chronicles migrate easily throughout the political space. They are widely available, and their arrival sparks a surge in chronicle-writing activity in the provinces. Importance within the culture makes chronicles inherently political: they would be commissioned for propaganda purposes, and the authorities would control their production and content. Hence, the idea that if a change in power occurred, the chronicle would be revised or a new one would be composed to conform to the new political realities. Since Shakhmatov, any chronicle is believed to be an intricate compilation (Russian 'svod') achieved by carefully weaving together texts of two or more preceding chronicles, now extinct. If so, theoretically, their texts could be retrieved by untangling individual threads, which is accomplished mainly through detecting conflicting political and ideological biases supposedly embedded in the text. Mining for the lost chronicles became the most important pursuit in Russian and Russian-inspired chronicle studies, delivering yet another 'lost' source — the only meaningful result of text-critical probing worth attempting.

These were ours, as anyone else's, including Dąbrowskiis, *a priori* that we could have followed but were compelled to abandon by the logic of our inquiry.

Here, I will not offer a systematic critique of the views and assumptions outlined above. I will curb myself to a few observations of a relatively uncontroversial nature. The production of historical texts was not the most pressing task on the medieval mind. Before the so-called 'paper revolution' arrived in the fifteenth century, chronicles were few and far between, as evidenced by the meagre number of manuscripts

that survived from the period (in fact, two²). Manuscripts are perishable, but the same can be inferred from a limited repertoire of the chronicle texts that had made it in the later copies (in fact, in our case, one from the early twelfth century, two from the early thirteenth, and three or four (depending on how we count) from the later thirteenth century)³. As elsewhere in medieval Christendom, historical tradition was maintained mainly through two principal strategies. One was a continuation: the received chronicle text was left unchanged but updated by filling in the gap between its last accounts and the new author's times (this technique is most prominent in the chronicle tradition of medieval Novgorod but also evidenced by the continuations of the *Primary Chronicle*). The other strategy involved editing the existing text by either expanding or abbreviating it (the two most obvious examples here are the Kyivan (expansion) and the Suzdal (abbreviation) chronicles for the twelfth century, both of which rework the same text).⁴ In any case, the new chronicle was not viewed as a stand-alone work and did not circulate as a separate manuscript but was invariably attached as a continuation to the already existing corpus (that is how our most famous chronicle collections, the codices known as the *Laurentian* and the *Hypatian*, evolved).⁵ In all the instances we can document, chronicles emerged in the clerical environment, most often in monasteries, while the input of secular powers (beyond the possible patronage) is not at all obvious.

If these observations are valid (and I do not see how they can be objected to), then the whole toolkit for studying chronicles, developed for a parallel universe where chronicles behave according to entirely different rules, must be seriously questioned, if not abandoned.

Since its discovery by modern scholarship, notably by Nikolai Karamzin in the early nineteenth century, the *Galician-Volhynian Chronicle* has been intensively studied. However, the results turned out to be disappointing. Studies would produce results that were quite incompatible,

² These are the so-called Synod copy of the *First Novgorod Chronicle* (first half of the fourteenth century) and the *Laurentian Chronicle* (1377).

³ For the early twelfth century: the *Primary Chronicle* (1116); for the early thirteenth century: the *Kyivan* and the *Suzdal Chronicles*; and for the late thirteenth century: the continuation of the *Suzdal Chronicle*, the *Nogrodod Chronicle*, and the *Galician-Volhynian Chronicle*.

⁴ Irina Yurieva, *Obshchii tekst Kievskoi i Suzdalskoi letopisei*, Moscow, 2022.

⁵ Thus, in the *Laurentian Chronicle*, the text of the *Primary Chronicle* is followed by a series of Suzdal continuations for the twelfth and the thirteenth centuries, in the *Hypatian Chronicle*, the *Primary Chronicle* is continued by the *Kyivan Chronicle* for the twelfth century and the *Galician-Volhynian Chronicle* for the thirteenth century.

and suspiciously, with each new one, the number of hypothetical extinct chronicles within the text would multiply. Thus, writing in the 1920s, Aleksei Orlov and Mykhailo Hrushevsky would claim that there is only one textual seam, albeit differently identified; in the 1930s, Lev Cherepin discovered two; in 1950, Vladimir Pashuto was able to uncover five; and also five were claimed to be identified by Antin Hensiorsky in the late 1950s, while most recently Adrian Jusupović was obliged to come up with no less than four. All these hypotheses are in disagreement, and no common ground has been found yet. There is no way all of them could possibly be right. Chances are, none is. There must be something wrong with the methodology employed that prevents consistent results.

The *Galician-Volhynian Chronicle* has suffered enough in the hands of scholars practising traditional Russian-inspired ‘textology’. The story of its study has been sketched in our book and must not be reiterated here. It is, however, important to stress that the propensity to look for the ‘lost chronicles’ made a surgical scalpel the scholars’ tool of choice. Chunks of varying volume would be carved out of the extant text and proclaimed the remnants of some preceding chronicle or a ‘tale’ incorporated into the body text. The criteria for identifying these ‘sources’ varied, but the unity of ‘method’ always produced the desired result.

Historians approach a chronicle on the level of its content. *What* the chronicle says is the first and most important question in ‘textology’, followed by ‘why’ and ‘what for’. By answering these questions (or faking the answers), a historian obtains his bearings in a text, which he then confidently distributes between the hypothetical extinct chronicles.

Yet medieval culture is alien to the modern mind, and Slavonic texts are not immediately transparent. With luck, an appreciation of ‘what’, ‘why’ and ‘what for’ may come as a reward for the inquiry. However, the investigation should not be premised on false certainties. The starting question, therefore, must be not ‘what’ a text says but ‘how’. This is the most crucial question in the text-critical approach, whose principal technique, ‘close reading’, is too well-established and described in any manual to be reiterated here in detail. In the process of ‘close reading’, irregularities in the text are registered: inconsistencies in syntax, logical contradictions, lacunae and ruptures in the text, and also ‘anomalies’: words and expressions, statements, and realities that stand in sharp contrast with the surrounding text and look ‘foreign’ against the background. In this way, it is revealed whether the text has been tampered with, and if so, what the tell-tale signs of tampering are and where they can be identified within the text.

‘Close reading’ belongs to the descriptive stage of inquiry and starts with no preconceived notions about a text’s purpose or composition.

That is how we began, slowly making our way through the ‘fog of war’, suggesting and rejecting partial hypotheses until those that had survived critical scrutiny and also explained most of the evidence were still standing. Only then did we begin to formulate more general ideas about the text’s history, ultimately arriving at the scenario presented in the book’s first chapter.

Predictably, Dariusz Dąbrowski does not concern himself with the issues of method. Neither does he reveal what ideas about the textual history of the *Galician-Volhynian Chronicle* he had developed, against which our findings will be demonstrably wrong or problematic, which makes the argument *ad hominem*, regrettably, unavoidable.

Ad hominem

Until recently, Dariusz Dąbrowski was known as a diligent student of the Rurikids’ genealogy and matrimonial ties. Research skills and habits developed in the course of genealogical studies are not necessarily those that equip one for textual criticism. Recently, Dąbrowski has become involved in two projects concerning the Galician-Volhynian Chronicle: in 2017, he co-authored its translation into Polish with Adrian Jusupović, and the same year, the two scholars edited the Galician-Volhynian Chronicle for the *Monumenta Poloniae Historica* series.⁶

Both Dąbrowski and his collaborator, Adrian Jusupović, had to muster the new field and develop relevant skills as they proceeded. Unlike the ‘Polish team’, all contributors to the volume under discussion had had a book-length study on the chronicle-writing under their belts before embarking on the project.⁷ Most of them studied the text of the

⁶ *Kronika halicko-wołyńska. Kronika Romanowiczów*, trans. Dariusz Dąbrowski and Adrian Jusupović, Kraków, 2017; ‘Kronika halicko-wołyńska. Kronika Romanowiczów’, in *Monumenta Poloniae Historica*, SN, vol. 16, ed. Dariusz Dąbrowski and Adrian Jusupović, Kraków and Warsaw, 2017.

⁷ Here is selected bibliography: Vadym Stavysky, *Kyiv i kyivske litopysannia v 13 stolitti*, Kyiv, 2005; Petro Tolochko, *Davnioruski litopysy i litopystsi 10–13 st.*, Kyiv, 2005; Oleksiy Tolochko, *Vasiliy Tatishchev: istochniki i izvestia*, Moscow, Kyiv, 2005; *The Hustynja Chronicle*, compiled with the preface by Oleksiy Tolochko, Harvard Ukrainian Research Institute Publications, Texts 11, Cambridge, MA, 2013; Iaroslav Zatyliuk, *Mynule Rusi u kyivskykh tvorakh 17 stolittia: teksty, avtory, chytachi*, Kyiv, 2013; Tetiana Vilkul, *Litopys i khronohraf. Studii z tekstolohii domonholskoho kyivskoho litopysannia*, Kyiv, 2015; *Kievskaiia letopis*, ed. Irina Yurieva, *Pamiatniki slaviano-russkoi pismennosti*. Novaia seria, Moscow, 2017; Vadim Aristov, *Aleksei Shakhmatov i rannee letopisanie: metod, skhema, traditsia*, Kyiv, 2018.

Galician-Volhynian Chronicle and published on it for years before 2014. Professor Dąbrowski's record, on the contrary, is rather unimpressive. Two prefaces published under the dual authorship notwithstanding, he has yet to demonstrate his ability to do independent work in chronicle studies. With such a noticeable difference in experience and achievement, one would expect the professor from Bydgoszcz to offer his opinions with greater humility. Yet he announces them with the aplomb of a teaching assistant marking a sophomore term paper.

Entia non sunt multiplicanda praeter necessitatem

How does Dąbrowski substantiate his claim that our study was driven by 'the desire to prove the a priori thesis about the time of creation and structure of the *Galician-Volhynian Chronicle*'? Having briefly summarised our findings, he moves to polemicise with several randomly chosen pieces of commentary on particular fragments of the chronicle's text. We will have the opportunity to explore the quality of his arguments in due course. Here, it is important to stress the apparent *non sequitur* in his reasoning. That our critic finds some of the comments on the text wanting or even wrong does not prove malicious intent. Dąbrowski completely missed that our findings were conditioned by the method employed and stemmed from the consistency of its application.

Methodological principles have been described in a relevant chapter of our book. We chose to operate not with hypothetical propositions but to stay within tangible textual evidence that can be presented and examined. In other words, we decided to say not everything that can be said about the chronicle but only what can be said with confidence, even at the risk of losing some plausible but unprovable propositions. In the words of August Ludwig von Schlözer, a founder of text-critical studies over the chronicles, it is better not to know than to err. Since Dąbrowski remained utterly immune to this aspect of our work, it will be helpful, for the sake of discussion, to recall the principal reasoning that informed our decisions.

The *Galician-Volhynian Chronicle* is unique among the Ruthenian history texts. Despite its traditional and somewhat misleading designation as a chronicle, it lacks the annalistic structure characteristic of the genre. That alone would suggest that it emerged within a distinct cultural setting where different models for historical writing were appreciated and that conventional chronicle 'textology' may not be applicable here. Indeed, it has been firmly established by Aleksei Orlov that the

Galician-Volhynian Chronicle emerged within the context of the Byzantine chronographic literature available in Eastern Europe in Slavonic translations. Moreover, Orlov's study revealed that Byzantine chronographs profoundly influenced not only the idea of history but also the very fabric of the text. Orlov demonstrated that 'borrowings' from chronographic literature (lexical and phraseological but also direct quotations, paraphrases, and allusions, even some plot developments) are more or less regularly distributed along the chronicle's text. The series extends uninterruptedly to the account (*Hypat.* 1265⁸) that can be firmly dated to 1264. Orlov's observations assume the status of textual argument: all these 'borrowings' were made in a single authorial effort, which means that the relevant portion of the text, its first, 'Galician', part, could not have emerged earlier than 1264.

In contrast to the 'Galician' part, the 'Volhynian' continuation, which lasted until 1289, employed different models. Its most important source of inspiration was the so-called *Kyivan Chronicle* for the twelfth century (incidentally, preserved within the same type of codices as the *Galician-Volhynian Chronicle* was), whose stylistics the 'Volhynian' part closely followed. The 'Volhynian' part mirrors the 'Galician' series of chronographic 'borrowings' with its own series of chronicle 'borrowings'. As Igor Eremin demonstrated years ago, the 'Volhynian' part shows remarkable coherence and unity, and no perceptible signs of different 'hands' can be established in its text.

When the linguists (Antin Gensiorsky, Dean S. Worth, Daniela Hristova, and recently Irina Yurieva) turned their attention to the language of the *Galician-Volhynian Chronicle*, they discovered the same division of the text into two large and contrasting parts: the 'Galician' heavily dominated by the high register of Church-Slavonic typical for chronographic translations and the 'Volhynian' that demonstrates linguistic habits very close to that of the Ruthenian chronicles. The two parts speak distinctly different idioms. Linguistic probing also revealed the 'transition zone' where chronograph-like language terminates and another, chronicle-like one, picks up: between *Hypat.* 1260 and *Hypat.* 1261.⁹

⁸ For the sake of convenience, here and elsewhere, the 'addresses' of the passages discussed are given according to year numbers found in the *Hypatian* copy (hereafter: *Hypat.*). The chronological grid of *Hypat.* is late and artificial and evoked here only to point to the fragments' 'topography' within the text of the chronicle.

⁹ Dean S. Worth, 'Linguistics and historiography. A problem of dating in the Galician-Volhynian Chronicle', *Indiana Slavic Studies*, 3, 1963, pp. 173–85; Daniela Hristova, 'Major Textual Boundary of Linguistic Usage in the Galician-Volhynian Chronicle', *Russian History*, 33, 2006, pp. 313–31; Irina Yurieva, 'Nekotorye osobennosti sintaksisa,

So, this is how the principal segmentation of the text was accomplished. It is important to note that this was achieved through philological critique and linguistic testing of the text, techniques that are neutral to the content ('information') and hence largely immune to arbitrary interpretations or admixture of external biases.

However, this picture is only 'the first approximation'. A more complex one emerged in the course of our inquiry.

Scholars, sometimes against their better judgment, proceeded on the assumption that we deal with two relatively sterile texts: no 'chronographic' inclusions would be found in the 'Volhynian' part, while the 'chronicle' language would not contaminate the 'Galician' part. The two idioms are so dissimilar, were shaped by sets of models so diverse, and emerged within a time gap so substantial that they can serve as reliable markers of different authorships. Contrary to expectations, however, a significant number of 'chronicle' textual borrowings were identified in the 'Galician', supposedly purely 'chronographic' part. How can the presence of a later idiom in a text created much earlier be accounted for? The most obvious explanation is that these are traits left by the native speaker of the 'chronicle idiom', which means that at some point, the initial 'Galician' text was revised by the 'Volhynian' author. This hypothesis neatly agrees with the evidence generated but not sufficiently appreciated by the linguists who spotted the 'chronicle' idiom's penetration deep into the 'Galician' text.

The results of yet another formal test proved consistent with the above hypothesis. Since the *Galician-Volhynian Chronicle* lacks a chronological grid, it employs what might be called 'temporal connectors' to indicate the flow of time. And since different models influenced the two parts, they developed their own distinctive sets of connectors, specifically 'Galician' and specifically 'Volhynian'. Generally, the distribution of connectors follows the pattern established for the language. However, some of the connectors typical for the 'Volhynian' part's final chapters (as in *Hypat.* 1273, 1277, 1282 1283, 1285, 1284, 1287, 1288, and 1292) are to be found deep in the 'Galician' part (*Hypat.* 1209, 1225, 1235, and 1249). And that, in turn, means that the person responsible for revising the 'Galician' part was also responsible for composing the final passages in the 'Volhynian' part.

morfologii i leksiki tak nazyvaemoi Galitsko-Volynskoi letopisi', *Lingvisticheskoe istochnikovedenie i istoria russkogo iazyka. 2012–2013*, 2013, pp. 135–51; ead., 'Lingvisticheskie parametry stilisticheskikh razlichii mezhdru Galitskoi i Volynskoi letopisiami', *Pismennost Galitsko-Volynskogo kniazhestva. Istoriko-filologicheskie issledovania*, Olomouc, 2016, pp. 65–79.

Once the hypothesis about the ‘editorial episode’ was formulated, it informed our reading and interpretation of fragments that previously were marked as ‘problematic’ or difficult’ but eluded explanation. The unifying source of them being challenging suddenly came to be highlighted. The presence of the ‘chronicle idiom’ (textual borrowings, lexical material, peculiar linguistic constructions, connectors, etc.) indicates that they were subjected to alterations and revisions. Exactly how and whether it is possible to restore the original state of text or at least retrieve its message is a matter for conjectural critique, the results of which are offered in the commentary section of the book.

In effect, we arrived at the scenario of the text’s history that best conforms to the evidence at hand.

The ‘Galician’ chronograph emerged as a single work sometime in the mid- or late 1260s, and its text covered events to at least 1264. After 1289 (but in all probability in the early 1290s), its ‘Volhynian’ continuation was composed. While merging the two parts, the editor severed the final passages of the ‘Galician’ part and substantially reworked those he was willing to leave, which resulted in what might be called the ‘transition zone’ (between *Hypat.* 1252 and *Hypat.* 1260). This ‘transition zone’ can be visualised by mapping the distribution of the two variants of the Lithuanian Prince Mindaugas’ name (the ‘Galician’ *Миндогъ* and the ‘Volhynian’ *Миндовъ*). The Volhynian editor also took to himself the task of systematically revising the ‘Galician’ part. After 1292, the ‘Volhynian’ part was edited, either by its author or by someone working in a very similar manner, after which the *Galician-Volhynian Chronicle* assumed its current state.

The suggested scenario describes the evolution of a single text through a series of continuations and revisions, which is consistent with medieval practice and does not require unwarranted hypotheses. Entities must not be multiplied beyond necessity.

Which part of our procedure does the Polish researcher find objectionable? He is unwilling or, rather, unable to indicate. Yet, he completely rejects the scenario of the text’s history that follows from it. On what grounds? Some of his remarks suggest that he subscribes to the old (1950) hypothesis by Vladimir Pashuto regarding the ‘Kyivan Chronicle of 1238’ and takes partial credit for its further development in a book published in 2019 by his collaborator, Adrian Jusupović.¹⁰ This hypothetical chronicle, supposedly incorporated into the ‘Galician’ part, ‘proves quite convincingly’, Dąbrowski assures us, the existence of the

¹⁰ Adrian Jusupović, *Kronika halicko-wołyńska (Kronika Romanowiczów) w latopisarskiej kolekcji historycznej*, Kraków and Warsaw, 2019.

history-writing activity in Daniel of Galicia's milieu already in the late 1240s. Again, this is one of Dąbrowski's *non sequitur*. That people might have composed chronicles in Kyiv does not 'prove' that they were able to do so in Galicia, and in the 1240s at that (the imaginary chronicle could have been cannibalised at any point after 1238). Moreover, a student of chronicles that he now claims to be, Dąbrowski should have known that Pashuto's idea of the 'Kyivan Chronicle' was first developed by Vadym Stavysky, Pashuto's pupil and one of the volume's contributors, in a series of articles published in the 1980s–1990s and later in a book.¹¹ That in the present study we decided against unnecessarily multiplying entities, speaks of great intellectual integrity rather than dishonest omission, as Dąbrowski implies. The Polish professor also suggests we contemplate other mirages like the 'South-Ruthenian compilation of 1225' also known as the 'Peremyshl Chronicle', or the 'Kyivan Chronicle of the Rostislavichi' of 1228.

What other sins does Dąbrowski discern in our book? He ponderously admonishes us for sticking to the 'old' name of the chronicle ('Galician-Volhynian') 'without taking into account the discussion on its genealogical affiliation presented in the critical edition and translation into Polish published in 2017' (p. 380, n. 1). However, as he rightly admits, the name became 'old' only very recently, in 2017, when Dąbrowski and Jusupović decided to rebaptise the text as the 'Chronicle of the Romanovychy' ('Kronika Romanowiczów'). Of course, we were fully aware that the 'Galician-Volhynian Chronicle' is neither 'Galician' nor a 'chronicle'. However, we decided to stick to the traditional name to avoid introducing unnecessary terminological confusion (is it the same chronicle known by a different title? Does the new name refer to the existing text or a newly proposed reconstruction?¹²) The suggested title is quite ambiguous at that: in what sense is it 'of the Romanovychy'? Does it refer to the Romanovychy proper, Daniel and Vasilko, or to the next generations of the dynasty too? Had they commissioned the text or simply happened to be its main characters? If the former is the case, it has not been proven and is likely incorrect. If the latter, it is commonplace. We saw no 'added value' for understanding the text in renaming.

¹¹ Stavysky, *Kyiv i kyivske litopysannia*.

¹² Interestingly, Dąbrowski seems unperturbed that the co-author of the new title also shies away from this invention and clings to the 'old' name in the English translation of his book (Adrian Jusupović, *The Chronicle of Halych-Volhynia and Historical Collections in Medieval Rus*, trans. Miłka Stępień, Leiden and Boston, 2022). Moreover, Dąbrowski himself prefers to refer to the chronicle by its 'old' name, as the title of his latest book (as in n. 19) testifies.

Dąbrowski managed to make himself acquainted with the relevant literature but did not proceed to the next stage, at which the received wisdom is critically reevaluated. Hence, his dependence on the views expressed and his constant appeal to authority. Dąbrowski is appalled that some outdated opinions are not given the respectful treatment they deserve, and seems visibly offended when some are dismissed as lacking merit or irrelevant. Apparently, he expected the same from us and reprimanded us for 'insufficiently' entertaining historiography in our research article. What may or may not be 'sufficient' is a matter of taste and proportion. However, Dąbrowski failed to notice that the relevant chapter is introduced with a note advising the reader that 'We do not intend to discuss exhaustively the minute details of opinions and views on the *Galician-Volhynian Chronicle* expressed in the literature. Instead, we will focus on the recurring motifs of textual studies, as well as the problems of the method'.

Dąbrowski also failed to notice that the deficit he thinks he detected in the introductory article is more than compensated by the comprehensive bibliography of the works on the *Galician-Volhynian Chronicle* placed at the end of the book.

Non sequitur

In fairness, one should admit that in addition to liberally dispersing labels ('baseless', 'unwarranted', 'bizarre', 'insufficient' and the like) Dąbrowski also treated us to his thoughts about the substantiative aspects of the book by offering polemical remarks on a selected number of commentaries on the *Galician-Volhynian Chronicle's* text, which he finds 'characteristic in many respects'. Although this kind of criticism, as noted above, could not possibly prove his principal claim of the book being an ill-conceived methodologically, let us have a look at the argumentation 'characteristic in many respects' for his understanding of how to deal with the chronicle text.

Dąbrowski starts with the commentary (authored by me) on one of the opening accounts of the *Galician-Volhynian Chronicle*, the story (*Hypat.* 1207) of the failed project to marry Daniel of Galicia to Elisabeth, the daughter of King Andrew II of Hungary, ruined due to the machinations of her mother, the Queen. Inside this story, another one about the murder of the King of Romans, Philip of Swabia, was inserted. Since the nineteenth century, this fragment has been identified as problematic. Everything seems to be confused here: names, circumstances, dates.

Those scholars who happened to comment on it invariably concluded that it had been damaged beyond repair. Exactly how it was destroyed, however, remained unclear. That, of course, was reason enough to have a deeper look into the text, hoping to suggest a plausible explanation for its current state. I offered the following: the account belongs to those in the 'Galician' part that suffered from the editorial intrusion of the 'Volhynian' author/editor. Among other things, he 'improved' the account by adducing a biographical note on Daniel's bride-to-be but erred by confusing two Hungarian saint princesses: St Elisabeth (died in 1231), daughter of King Andrew II and Queen Gertrude, and St Kunegunde (died in 1292), daughter of King Bela IV, wife of Prince Bolesław of Kraków.

Dąbrowski finds my argument 'an intricate, multi-level structure' (p. 383) (which is another way of saying 'sophisticated', thank you), quite unnecessary so, for he finds nothing wrong with the account. He concludes his discussion with a list of rhetorical questions called to show the absurdity of my suggestion:

Why did the Volhynian author, writing in the early 1290s, mention Ludwig, Landgrave's son, as Ludwig as the husband of Elizabeth, known as Kinga? Why should he note the connection between the family of Queen Gertrude of Hungary and the murder of King Philip of Germany [--]? [--] What did these ancient events and their participants mean for a historian working in Volhynia? (p. 387)

I wish I knew, with Dąbrowski's confidence, why a medieval chronicler did or did not make his choices. I do not. But this is not even the point. Dąbrowski attributes claims to me that I never made, effectively mocking his own suggestions. I never claimed that the whole set of records on Queen Gertrude and her family scattered along the 'Galician' chronicle was invented and inserted by the Volhynian author in the 1290s.

To demonstrate that my argument lacks merit, Dąbrowski shifts the discussion to the familiar territory of genealogy, where he feels more at home. He completely missed that the argument was based on textual, not factual, grounds. So, let me briefly summarise it. In its current state, the passage in question consists of two stories: the first one about the projected marriage of Prince Daniel to the daughter of King Andrew; the second — about the murder of King Philip of Swabia, which (the murder) was somehow linked to the brother of Queen Gertrude (King Andrew's wife) and hence to the resulting marriage of her daughter to Ludwig, son of Landgrave of Thuringia. The connection between the two

stories is not explicitly stated in the text; instead, it follows from the 'topography' of the adjoining fragments. However, the attachment of the two stories, as they are currently placed in the text, turns out to be rather awkward. The name of Daniel's bride-to-be, Elizabeth, and a brief biographical note about her were separated from the main body of the message by the story of her mother, Gertrude's, family. This caused distortion: from the literal reading of the text, as it now stands, it would follow that it is Queen Gertrude who was called Elizabeth, and it was she who subsequently was made a saint.

It would appear that either the second story (the murder of King Philip) was inserted inside the first one (about the King Andrew's plan to arrange his daughter marriage with Daniel), or else the final 'biographical note' on Elisabeth was added as an afterthought and caused disturbances in the whole fragment. The passage is framed by identical phrases, which in the *Galician-Volhynian Chronicle* is a tell-tale marker of an interpolation. Moreover, the medieval editor signals his interpolation by the standard 'formula of return' to the interrupted story ('But let us return to our former narration which we had already begun').

Only as a final step, the suggestion was made that this interpolation erroneously refers to Kunegunde (Kineka, Kinga), wife of Bolesław, prince of Kraków, and hence belongs to the Volhynian editor who must be responsible for other intrusions into the text. It is supported by the fact that another misplaced phrase in *Hypat.* 1207 'for he had no son' has its source in the Volhynian part (*Hypat.* 1280), where it refers to Bolesław, Kunegunde's husband.

Is this suggestion impossible? Not at all; mistakes like this happen all the time in chronicles. For example, the author of the *Chronicle of Greater Poland* made a similar mistake, stating that Kinga's sister Yolanda (Helena), wife of Bolesław the Pious, was the daughter of King Bela of Hungary and his wife, the 'sister of Saint Jadwiga' (Hedwig).¹³ In fact, St Hedwig's sister was no other than Queen Gertrude of our fragment, wife of King Andrew and mother of Bela. Similarly, the *Annals of Sedziwoj* claim Bela IV to be the father of St Elizabeth, while in fact, he was her elder brother and father of Kunegunde.¹⁴

A man of many trades, Dąbrowski tried his hand in the field of art history, and in 2023, as it happened, published a book on medieval

¹³ 'Kronika Boguchwała i Godysława Paska', ed. Waclaw Alexander Maciejowski, in *Monumenta Poloniae Historica*, vol. 2, Lwów, 1872, p. 582.

¹⁴ 'Rocznik Sedziwoja', ed. August Bielowski, in *Monumenta Poloniae Historica*, vol. 2, Lwów, 1872, p. 877.

architecture as reflected in the *Galician-Volhynian Chronicle*.¹⁵ Thus, he could not have ignored the commentary (authored by Vadym Stavysky) on the chronicle's passage describing the building activity by Prince Daniel in the town of Kholm (Chełm). Dąbrowski's authorial verdict is decisive:

Stavysky baselessly questioned the reliability of the source account, which straightforwardly reports the construction of four churches commissioned by Daniel Romanovich. Instead, he presented a bizarre reconstruction of the supposedly original text of the chronicle, containing descriptions of three of them. (p. 389)

In fact, Stavysky claimed exactly the opposite: the original text described a single church that was later divided into three. Having completely misunderstood the argument, Dąbrowski nevertheless argues against it. How does Dąbrowski know, and with such certainty, that the account accurately reports on past realities? Because it says so! Since this is the extent of the argument Dąbrowski is willing to offer, the reader is owed some further explanation. Stavysky's hypothesis is not at all 'baseless'. It has two sets of arguments behind it: textual and factual.

In our initial discussions, we approached the account of Prince Daniel's building activity in a rather conventional way: as a story, even if somewhat exaggerated and embellished, of actual events and real buildings. Soon, however, the problematic nature of the text became apparent. It speaks of thirteenth-century architecture in a language borrowed from Byzantine chronographs, where it is applied to descriptions of Old Testament and classical buildings. That alone makes the fragment not so straightforward. Additionally, it depicts structures that were all destroyed and no longer standing by the time the writer undertook his task, which means that we are dealing with imaginary descriptions. Most importantly, however, the fragment is full of lacunae, text breaks, interpolations, and inconsistencies that render it obscure and, in places, illegible, which means that it might have been distorted.

In the 2000s and 2010s, the medieval hillfort of Chełm became the subject of intensive archaeological investigations, driven, to no small extent, by the desire to confirm textual evidence. In the article summarising the results of excavations, Andrzej Buko, who led the project, had to admit to a puzzling outcome: none of the structures listed in the

¹⁵ Dariusz Dąbrowski, *Kronika halicko-wołyńska (Kronika Romanowiczów) o sztuce. Architektura*, Warsaw, 2023. The book remained unavailable to me.

Galician-Volhynian Chronicle were discovered, while the structures that the archaeologists uncovered had no correspondence in the chronicle's text.¹⁶ The only structure that figures in both is the church of the Mother of God, whose remnants were found under the current cathedral's foundations. However, it turned out to be a modest affair far removed from the magnificent and imposing edifice of which we read in the chronicle.

The striking feature of the chronicle account is that the author's promise to tell the story of the construction (and destruction) of a single church results in the description of three churches: in addition to the historically attested church of Mother of God, two more, otherwise unattested, are described, St John Chrysostom's and St Cosmas's and Damian's. No less puzzling is that each church is awarded only an incomplete set of details of its exterior, interior, decoration, or church vessels. If combined, however, the scattered characteristics form a whole that makes sense. Thus, a hypothesis was formulated that the current state of the text is a result of a later editor's effort to distribute a complete description between three ecclesiastical structures.

Dąbrowski finds this suggestion 'baseless' (p. 389). He should have known better, for it is consistent with the medieval practice, and similar cases are attested in chronicles. As the outstanding Ukrainian philologist Leonid Makhnovets pointed out years ago, an almost identical editorial technique is found in the *Kyivan Chronicle*, where, in the entry for 1175, a single description was distributed between two churches: the Dormition Cathedral in Vladimir-on-Kliazma and the Church of the Nativity of Mary in Bogoliubovo. The church in Bogoliubovo received the interior, while the cathedral in Vladimir received the exterior.¹⁷

Dąbrowski's attention was also drawn to the commentary by Tetiana Vilkul and Vadym Aristov on the mention of Polovtsian chieftain Konchak in the 'Encomium' to Roman Mstyslavych. In particular, the reviewer declared the authors' interpretation of the cauldron that Konchak carried to be unsophisticated and erroneous. The scholar admonishes Vilkul and Aristov that they should have been aware that the cauldron here is a symbol of the authority of the Polovtsian elite, which the 'Encomium' supposedly drew from the 'Polovtsian epic'. The remark, as usual, misses the argument.¹⁸ Had Dąbrowski carefully read the text

¹⁶ Andrzej Buko, 'Źródła pisane i archeologia: przykład Góry Katedralnej w Chełmie', *KH*, 123, 2016, 2, pp. 221–46. This article cited in our book seems to escape Dąbrowski's attention.

¹⁷ *Litopys ruskyj*, transl. Leonid Makhnovets, Kyiv, 1989, p. 313.

¹⁸ Whatever the merits of the suggestion that the cauldron in the story should be understood as a symbol of authority and an item pertaining to shamanism, the

of the commentary, he would have noticed that the authors refrained from interpreting the meaning of the cauldron and passing judgment on the 'Encomium's' 'connection to the 'Polovtsian epic' (which, after all, is entirely in the domain of the fanciful). The focus of the commentary was on what the expression 'снесе Соулоу' could mean. The authors remained within the confines of real texts and showed the closest literary and semantic context of the mention of Konchak in the 'Encomium'. Dąbrowski asserts that the authors made 'various gross mistakes' (p. 392). In particular, he wonders why the authors derive the verb 'снесе' from the infinitive 'нести' (to carry) rather than 'сѣсти' (to destroy or kill). Dąbrowski refers to the third volume of the famous Sreznevsky's dictionary, col. 782. Indeed, the verb 'сѣсти' is recorded there. However, as easily verified, its primary meaning is 'to eat'. The third of the meanings given — 'to destroy' — is metaphorical (for example, in the expression 'сѣсти завистию', to be consumed by envy). A speaker of Polish grasps this better with another form of the verb, 'сѣдати', with the same set of meanings, including a figurative one ('сѣдати огньмъ', to be consumed by fire). Both have a different root than 'нести'.

Among the fragments of the book that the reviewer dismisses but for which 'there was no room' for his in-depth critique, is Vadym Aristov's commentary on King Daniel's obituary, where a note about the necessity of dating his death to 1266, contrary to the long-standing tradition, is included. Dąbrowski disagrees with such dating, instead preferring the traditional 1264. It should be noted that 1264 is the date provided by the Hypatian copy of the Galician-Volhynian Chronicle, whose artificial chronology is late (dating to the 1420s) and unreliable. This means the date can be safely excluded from the discussion as a late construct.

Starting with Hrushevskiy, scholars have tried to 'save' the year 1264 based on the inner critique of the *Galician-Volhynian Chronicle*. In particular, a strong argument was considered to be the mention of a comet (visible in July-September 1264) placed after the note of the king's death. However, as shown in detail in several commentaries of the book, the sequence of events in this section of the *Galician-Volhynian Chronicle* is erratic, as the accounts are found in the 'transition zone' between the 'Galician' and 'Volhynian' parts where the text is seriously disturbed as the result of editorial incursions. The dating of these, mostly 'Lithuanian',

objection is not made in good faith. Dąbrowski, as a note on page 37 asserts, learned about this discovery from the unpublished PhD dissertation defended in 2021, yet he scolds the authors of the book, which was sent to the printers in 2019, for not considering it.

events cannot be verified by external evidence. In other words, the *Galician-Volhynian Chronicle* does not provide grounds for dating Daniel's death to the exact year. Meanwhile, several Polish annals (the *Traska Annals* and the *Franciscan Kraków Annals*), as well as the *Chronicle of Greater Poland*, unanimously noted Daniel's death in 1266. No serious objections have been raised to date, except for attempts to reconcile it with the fictitious date from the *Hypatian* copy. The text-critical approach suggests that we have no choice but to accept that Daniel passed away in 1266.

Wie es eigentlich gewesen

Dąbrowski's principal problem with our study and, judging from remarks scattered along his article, a source of his constant irritation, is that we approach a medieval chronicle as a text and a literary composition while he sees it as a container of 'information' or 'facts'. He is utterly insensitive to the textual aspects and hence unwilling to entertain text-critical arguments. Our emphasis on the literary nature of the chronicle and the need for a deeper understanding of its textual makeup challenges the traditional view of chronicles as straightforward 'sources' shared by Dąbrowski.

Chronicles are the most unreliable of sources. Historians have been acutely aware of this since the time of Leopold von Ranke. Chronicles do not report in a straightforward and factual way, 'wie es eigentlich gewesen'. They belong to a vast and diverse body of what might be termed 'history-based literature' stretching from terse and factual annalistic records to historical fantasies like the *Alexander Romance* or the *Romance of Troy*, with the likes of the *Galician-Volhynian Chronicle* found closer to the centre of the spectrum yet not all that far from the latter. The most popular works are not necessarily the most informative or accurate from a modern point of view, which means that contemporaries might have found reading (or listening to) history attractive for reasons other than factual accuracy: entertainment, aesthetic pleasure, and amusement play a no small part. If so, then invention (and omission) must be seriously considered as intrinsic elements of a chronicler's craft, and modern scholars must be alert to the literary devices of their medieval colleagues. There is also the problem of authorial intentions and the genre conventions to be considered. The *Galician-Volhynian Chronicle* might be likened to the Western genre of *gesta* with its special focus on the martial deeds of the princes and their achievements as rulers. Embellishment and fabrication are only expected when the author champions

the dynasty and strives to portray it in the best light, turning flaws into virtues, crimes into errors of judgment, battles lost into gallant feats, and so on. Appearance or even truth claims no longer deceive us.

We know little about why and for what they wrote chronicles in Rus'. However, we can confidently state that they were not intended to be 'sources' for future historians. Before becoming a source, they must be appreciated for what they are: 'words, words, words'. Only by paying close attention to the word can a proper method for understanding emerge.

The above discussion of the problems associated with studying Old Ruthenian texts may seem unnecessarily lengthy for a critical reply. I thought it useful nonetheless because, understandably, not all readers of *Kwartalnik Historyczny* may be intimately familiar with the research issues involved and, hence, not equipped to arbitrate on their own. I have tried to discuss complex topics without resorting to the kind of 'labelling' language that our opponent employs. That is why the reply to Dąbrowski's haphazard criticism of what he accidentally (mis)understood in our book turned out to be somewhat protracted. For the most part, Dąbrowski's objections stem from the fact that the solutions proposed in the book he chose to review are outside the conventions he would accept. However, his shortcomings should not deprive the readers of a serious journal of a meaningful conversation about a complex subject.