

*The Igor Tale (IT) and Zadonshchina (Z): some remarks on papers by Donald Ostrowski,
Olga Strakhov, and Harvey Goldblatt*

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I may have learned more about the IT from the papers presented today than in many years of reading (and reading about) the text, and I offer my sincere thanks to the panelists. In the remarks that follow, I will attempt to contextualize, both historically and by analogy (that is, following Origen, at the literal and spiritual levels) some of the points they have made; to suggest a few alternative readings of their data at places where it may leave room for further interpretation; and to play devil's advocate — not only in regard to the individual papers, but vis-à-vis the question posed by the title of the panel itself. As Don and Olga have shown in their fine papers, recent research in the two fields of the humanities that most closely approximate scientific procedure — textual criticism and linguistics — lead us to radically different conclusions; these discourses are apparently incommensurable, and place us in the sort of impasse that Derrida called an *aporia*. And of course, an *aporia* can never be endured as such. Harvey's paper is a methodologically rigorous attempt to overcome it; my comments will make no similar attempt, but will probably leave us *en aporia*.

First, my analogy. The Getty Kouros is a statue of a nude youth in archaic style, acquired by the Getty in 1984 and exhibited in 1985. From the time of its acquisition and exhibition, heated debates have taken place about its authenticity, that is, its archaic (6th c. BCE) origin. Its "well-documented" provenance was soon shown to be a mystification. Art historians remarked its stylistic peculiarities: its oddly floury surface, the unusual

juxtaposition of narrow shoulders and torso with thick thighs. It mixed traits of various regional styles. Most problematically, various parts of the Getty Kouros suggest different datings according to the typology of Gisela Richter: the head shows traits of the late 7th/early 6th c., the hands resemble those of *kouroi* from the second quarter of the 6th c., while the feet display innovative features of the third quarter of the 6th c. No other kouros shows such a wide range of features by date. On the other hand, the Getty Kouros conforms well to archaic canons of proportion and movement: its formal proportions are similar to those of Egyptian statuary of the late 7th to late 6th centuries, as determined by the statistical analysis of Elanor Guralnick, while the position of its feet match archaic canons of movement studied, again statistically, by Ilse Kleemann.¹

That is to say, leaving aside the connoisseur's reaction to the kouros (an analogy to those literary and other historians "amazed and confused" by the IT) and the problem of its "mixed stylistic vocabulary" (cf. those scholars who focus on the IT's lexicon), there is a disagreement concerning the statue's age between those who study the chronological relations among extant kouroi (an analogy to textual criticism) and those who analyze the "flexions and syntax," as it were, of a body of sculpture (an analogy to linguistics). (I am not considering the findings of "real" scientists: at first it was claimed that the surface dedolomitization of the statue proved that it had aged for centuries, but more recently these processes were recreated in laboratory conditions. In this instance, "real" science is least helpful of all.) A colloquium held in Athens in 1992 failed to resolve the impasse, and John Walsh, Getty director, wrote afterwards: "If genuine, [the

¹ For the arguments for and against the antiquity of the Getty Kouros, see Jeffrey Spier, "Blinded by Science: the Abuse of Science in the Detection of False Antiquities," *The Burlington Magazine* 132, 1050 (Sept. 1990): 623-631; and articles in *The Getty Kouros Colloquium* (Athens: The J. Paul Getty Museum, 1993).

kouros] adds an impressive and beautifully preserved example (albeit with some curious anomalies) to the small group of authentic kouroi. If false, it is an astonishing recent creation, made with a rare combination of empathy, technical skill, and scientific knowledge by a faker whose other works must be very good indeed."

I.

Turning to Don's paper, which offers a thorough overview of the stemmatics of the IT, I would like to begin by putting some of the stemmata under scrutiny. First, any scholar who writes off the textual agreements between the Kirillo-Belozerskii monk Efrosin's MS of Z (KB) and the IT as "coincidental" is being both dishonest and more honest than s/he means to be. The texts indeed *coincide* in many places: despite its brevity, KB shows the most individual parallels with the IT of all the Z MSS, as Oleg Tvorogov noted. A priori, then, we ought to reject Jakobson's, Dmitrieva's, and Zimin's versions of the relationship between the IT and Z (their stemmata may yet illustrate the relationships of the various copies of Z among themselves). I should also point out that Jakobson vitiated his own stemma with his careless hypothesis of a "diptych" joining the IT to Z (which could conceivably account for individual parallels between IT and any copy of Z): his supposed evidence, the phrase "let us set word next to word" (*s"stavim slovo k slovu*), is in fact a hagiographical commonplace found already in Nestor's 11th-c. *Life of Feodosii* (*semu podob'no slovo k slovu prilozhim*). It is probably of Byzantine origin. Unfortunately, Jakobson's thesis continues to be cited to this day.²

² See Roman Jakobson and Dean S. Worth, eds., *Sofonija's Tale of The Russian-Tatar Battle on the Kulikovo Field* (The Hague: Mouton & Co., 1963): 17. The "diptych" thesis was Jakobson's alone: see Worth's comments in Victor Terras, *Handbook of Russian*

Second, the question of the Z archetype's composition at the Kirillo-Belozerskii monastery has now superseded Fennell's "great question" of the order of the Z redactions: a study of all the Z MSS (but especially the closely related KB and Sin) against Kirillov's well-preserved library, and the identification of interpolations from this library's books into those MSS, will likely answer both questions at once. Shibaev and Bobrov's articles contribute to this effort, as does my short piece in Norman Ingham's *Festschrift*.³ Third, and in relation to the problem of medieval editing: if we accept, as do Dmitrieva and Shibaev, that the 17th-c. Belarusian MS Sin represents a collation of two MSS or traditions, we must try to find the scholarly milieu where this MS collecting and editing took place. This would be worthwhile work for scholars of Ruthenian (Polish-Lithuanian) texts and their scribal contexts.

To treat briefly Don's overview of the modern candidates for authorship of the IT, in particular Edward Keenan's hypothesis concerning Dobrovský, it may be recalled that Simon Franklin (in his review of Keenan's book) drew attention to Elagin's description of the IT's presence in an "old MS belonging to Musin-Pushkin." As Franklin notes, this fact forces us to rethink Keenan's entire argument — that Dobrovský did not write the IT in order to deceive, that it was an innocent "prose comp" exercise that fell into Elagin's hands and was thence transferred to Musin-Pushkin and Co. Franklin concludes that we must either imagine a conspiracy of Elagin (and Karamzin) with the publishers of the

Literature (New Haven: Yale UP, 1985), s.v. "Zadonshchina." The passage in Nestor's *Life* of Feodosii is on f. 526 of the *Uspenskii sbornik*.

³ See M. A. Shibaev, "'Zadonshchina', 'Slovo o polku Igoreve' i Kirillo-Belozerskii monastyr'," *Ocherki feodal'noi Rossii* 7 (2003): 29-57; Alexander Bobrov, "Problema podlinnosti 'Slova o polku Igoreve' i Efrosin Belozerskii," *Acta Slavica Iaponica* 22 (2005): 238-298; Robert Romanchuk, "Efrosin of Kirillov and an Interpolated Princely Genealogy in the *Zadonshchina*," *Russian History/Histoire Russe* 33 (2006), 353-363.

editio princeps, or accept that the "old" Musin-Pushkin MS did exist.⁴ Although Franklin apparently wishes to recommend the latter point of view, I would propose that what we need now is in fact a sharply paranoiac reframing of Keenan's evidence. At the very least, there is something unusual about Karamzin's excerpts from the IT's putative codex in his *Istoriia Rossiiskogo Gosudarstva*: they betray the interest of the lexicographer, not the historian. As distinct from the many worthless paranoid readings of the IT (devoted to cryptograms, numerological codes, etc.) that are now proliferating, a conspiratorial interpretation of Keenan's findings concerning the *editio princeps* — say, rereading them through Trost — would move the field forward: within such a consistent frame, Keenan's observations, alternately penetrating and self-contradictory, could be rationally disputed (or supported).

II.

Turning now to Olga's spirited overview of the linguistic evidence for the IT's 12th-c. provenance — as a non-linguist, my comments will be of a different order than my comments on Don's and Harvey's papers — I would first like to correct the record in a small way: apparently, not all linguists have uniformly considered the IT to be authentic and old. In his contribution to Dean Worth's 1995 Festschrift, Henrik Birnbaum recalled a 1958 encounter with V. V. Vinogradov at the 4th International Congress of Slavists, in which the "doyen of Russists" professed his belief that the IT was contemporaneous with Z. How he reached this conclusion, unfortunately, Henrik does not tell us. But in a 1964 article surveying Shakhmatov's work on the formation of literary Russian, Vinogradov

⁴ See Simon Franklin, "The Igor' Tale: A Bohemian Rhapsody?," *Kritika: Explorations in Russian and Eurasian History* 6, 4 (Fall 2005): 833–44.

endorses his predecessor's view that the IT, in the form in which it has reached us, is "a literary work of the 15th-beginning of the 16th c." It is not clear that Vinogradov (and/or his chosen mouthpiece, Shakhmatov) is speaking only of orthographic features.⁵

Olga remarks that the first editors' use of the *grazhdanka* may have corrupted the original paleographical and spelling traits of their presumed MS model. This fascinating problem was treated — in good faith! — in a 1957 article by D. S. Likhachev, who asked "why the edition of 1800, as distinct from the 1793 edition (by the same publishers) of the *Pouchenie* of Vladimir Monomakh, with which it has so much in common, used the *grazhdanskii shrift* and not Church Slavic type." He makes two key observations. First is that ChSl type was not used in the 18th c. to preserve the original orthography of a MS. On the contrary, Musin-Pushkin's edition of the *Pouchenie* does not reflect the layout of the original (Laurentian) MS — rather, it is printed according to the Synodal orthographic norms of the time, distorting the original as badly as would the *grazhdanka*. Second, a great amount of editing was done to prepare the IT for printing, so that the MS ultimately used by the editors (as distinct from that of the *Pouchenie*) was in a modern cursive hand. The *editio princeps* simply reflects this fact. Likhachev's observations accord with what we know of humanist editions of classical texts: their typeface was meant to convey the sense of the underlying MS to the reader, in a handsome hand. The editors of the 1800 edition clearly intended to transmit to the reader of the IT the "feel" of a modern cursive MS — for the text is not only in the *grazhdanka*, it is set in italics — just as they intended

⁵ See Henrik Birnbaum, "Toward an Unprejudiced Assessment of the Igor' Tale," in *The Language and Verse of Russia* (Moscow: Vostochnaia literatura, 1995): 52-58; V. V. Vinogradov, "Izobrazhenie russkogo literaturnogo iazyka v izlozhenii akademika A. A. Shakhmatova," *Filološki pregled* 3-4 (1964). I thank Harvey Goldblatt for drawing my attention to this article, cited in Bobrov 297.

the reader of the 1793 edition of the *Pouchenie* to "feel" an old MS beneath its ChSl type. Of course, there may be various reasons for this (including the well-known fact that the editors perceived the IT as an Ossianic song, rather than as edifying "scripture").⁶

In order to underscore her point that it is the relations between lexemes, not the lexemes themselves, that constitute a language, Olga cites Shcherba's witty nonsense-line. This structuralist insight brings to mind the ending of the film "Blow Up" and Slavoj Žižek's interpretation of it.⁷ For the structuralist, as for the tennis-playing mimes at the end of Antonioni's film, the game can be played without an object: only the rules matter. But with the entry of a subject and object of language, the rules become overdetermined. As a supplement to Shcherba, I would cite Maiakovskii's famous line (from *Bania*), "*Ai Ivan v dver' revel, a zveri obedali.*" Surely Shcherba's students would have recognized this too as Russian, which it is, but it is also the Dog-English ("I want very well, is very badly") of a monoglot Russian. Maiakovskii's line shows the misparsing of the signifying chain proper to the linguistic subject, who seeks meaning in a text because s/he perceives it to be disturbed by an object. This is why the editors of the *editio princeps* read *k"meti* as Ukr. "toward a goal," on the one hand, while Keenan reads *saltani* as Cz. "from an altana," on the other. We are trapped in a vicious circle: only the relations between words can inform us about the provenance of the text (i.e., that the IT is 12th-c.), but only its provenance can tell us about the relations between its words (i.e., that *k"meti* and *saltani* are, with some fudging, the correct readings). The illusion of meaning persists: we cannot

⁶ "Istoriia podgotovka teksta 'Slova o polku Igoreve' k pečati v kontse XVIII v.," first published in vol. 13 of TODRL, has been reprinted in Likhachev's collection "*Slovo o polku Igoreve*" i kul'tura ego vremeni, in two editions (Leningrad: Khudozhestvennaia literatura, 1978: 237-277; 1985: 293-332)

⁷ See Slavoj Žižek, *Looking Awry* (Cambridge: MIT, 1991): 143.

exorcize it as easily as does Shcherba — whose purely structural approach, brought to its logical extreme, leads to the anagrammatical or numerological readings mentioned above.

III.

At first sight, Harvey's paper might seem to fall outside of the "science-like" discursive space outlined in our panel title. However, according to John Damascene (in c. 3 of his *Dialectica*), "art (Gr. *technê*, Harvey's Sl. *khitrost'*) is that done with the hands, whereas science (Gr. *epistêmê*, Sl. *khudozhestvo*) is any art that is practiced by the reason, such as grammar, rhetoric, and the like." The philology practiced and championed by Harvey is founded upon authoritative late antique and medieval theories of interpretation: indeed, he and Riccardo Picchio have argued that this medieval study is the ancestor of, or shares an ancestor with, modern philology. There is thus a coincidence of method and object in Harvey's work that I find very appealing.

First, a glance at problems of the composition (or compilation) of the IT. Harvey acknowledges that we cannot exclude the possibility that the IT belongs to the "open tradition": indeed, as a link in the chain of what Paul Zumthor calls the *mouvance* of medieval vernacular texts, extending from the epic commonplaces of the Pskov *Apostol* colophon to Z, it would seem to partake of this tradition. Others who have treated the IT as a compilation made by different hands at different times are Franko and Hrushevs'kyi, who still located its completed form in the 12th c., and V. V. Ivanov and V. N. Toporov — who, again according to Henrik Birnbaum, argued (in the 1980s?) that the IT had an early core that was expanded at "later times." Might not a vernacular "open tradition" — textual *mouvance* — provide a framework for the apparent mutual influence of IT and Z

that has thus far eluded textual criticism? Furthermore, would editorial interventions on the part of Musin-Pushkin and Co. fall within this "open tradition"? It is possible that at least some Ossianic requisites, such as the infamous "blue wine" and "silvery streams," were introduced by the editors of the *editio princeps*, convinced as they were of the IT's "bardic" nature.⁸

The antique and medieval hermeneutic traditions invoked by Harvey treat the text as an object of study, not of aesthetic pleasure. Karlheinz Stierle has defined medieval study as "a mode of reading which assumes that the text raises difficulties ... Study is a slowing down of reading, where understanding gets engaged in never-ending processes and detours."⁹ We know that at least one of the medieval reader-editors of the texts we are treating at this panel studied them: Efrosin's KB copy of Z includes a genealogical gloss and a several-page chronographical continuation, both copied from a short Rus' chronicle copied elsewhere by the scribe. It is possible that Efrosin, a teacher at Kirillov, discussed Z with his students at the monastery school: this could be ascertained through a comparison of features of his text of Z with those of his texts that were almost certainly used in the classroom.¹⁰ This brings me to my last point. Harvey refers to the "rules of the game" that conditioned the production and transmission of Old Rus' texts. Textual study was not static in *Slavia orthodoxa*: insofar as these rules are implicit in study, they were

⁸ In this connection I should remark that Nabokov's argument that the apparent Ossianic material in IT proves its authenticity, insofar as the Russians received a denatured French Ossian via Letourneur (see *The Song of Igor's Campaign* [New York: Vintage, 1960]: 12-13), is too clever by half: surely Nabokov was aware of Karamzin's translations of Ossian directly from the English. Here, as elsewhere (see esp. his commentary to lines 472-474), he is more than likely hedging his bets.

⁹ "Studium: Perspectives on Institutionalized Modes of Reading," *New Literary History* 22, 1 (1991): 115-127.

¹⁰ See c. 5 of my *Byzantine Hermeneutics and Pedagogy in the Russian North* (Toronto: University of Toronto, 2007).

not static either. Thus, as Harvey discusses, from the earliest period Origen's spiritual sense was sought by readers of a text. But from about the middle of the 14th c., as part of the renewed *translatio studii* to the Slavs known in East Slavic contexts as the "Second South Slavic Influence," hesychast modes of study, influenced by Christian Platonism (and complementary academic forms, influenced by Christian Aristotelianism), became available: instead of types, the scholar sought noetic unity in visible multiplicity, making use of the philosopher's procedures of collection (or definition) and division. And in the late 16th and 17th centuries, elements of the Jesuit curriculum migrated from Poland to Ukraine and then Muscovy, where neo-Byzantine scholarship appeared as well. Might not the framework of study help outline the quiddity, or "what-ness," of the IT and Z in their changing contexts? For example — David Goldfrank's example, which he kindly brought to my attention before the panel — in the 15th c., the IT's "thematic clue" from Deuteronomy could have resonated against the hesychast theology of the intellect (*um*) and the heart.

Thank you all again, for a terrific and thought-provoking panel.