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Ostrowski, Donald. *Muscovy and the Mongols: Cross-Cultural Influences on the Steppe Frontier, 1304-1589*. Cambridge, UK: Cambridge University Press, 1998. Pp. xvi, 329. \$59.95. ISBN: 0-521-59085-X.

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This is a provocative and maddening book. Its focus "is the Mongol impact on the Muscovite polity and the different ways that the ecclesiastical and secular establishments of that polity reacted to it" (p. xiv). The author's hypothesis is that the secular administration was heavily Mongol influenced and the ecclesiastical administration was heavily Byzantine influenced. The two influences clashed...[which] helps explain why our sources tend to contradict one another, and why historians come to such diametrically opposed positions on the nature of Muscovite political culture. (246) In fact, the book's title is misleading, since a significant portion of the material has no demonstrable connection with the theme of "Muscovy and the Mongols." Perhaps surprisingly, since in his previous publications Dr. Ostrowski has shown a strong interest in the methodology of historical inquiry, the most serious problems are methodological. The provocative ideas are the least well supported. Much of the detail is to no particular purpose, especially since the author devotes unwarranted attention to issues than have largely been resolved.

Ostrowski first presented his argument about secular administration in a carefully-worded article in *Slavic Review* in 1990. Here he elaborates on what he terms the dual (i.e., parallel military and civil) administrative system of the Mongols, and its apparent Chinese origins. It is plausible that the Mongol models did indeed have a profound impact on the formation of the Muscovite administrative system in the critical period of its evolution in the fourteenth century, but unfortunately he seems to have found no additional evidence in Muscovite sources which could flesh out that argument. His method of demonstrating cultural influence involves three criteria: (1) that the institution or practice existed in the source culture; (2) that its existence in the source culture coincided in real time with its appearance in the target culture; and (3) that a mechanism for its transference from the source culture to the target culture was operative.(34) The success of such a scheme would rest in the first instance on his ability to demonstrate that institutions or practices are identical, but too often he merely declares that to be the case.

Even if one accepts the validity of his method, there is the obvious danger that "proof" will consist in accumulating parallels that have nothing to do with the "real time" being studied; this is precisely what we find here. He ranges freely over anything that comes to mind across Eurasia from China to the Mediterranean; his chronology begins with Ur. His evidence and analogy include Hollywood films and curiously out-of-date news articles regarding the National Basketball Association and IBM. We are treated to dark innuendo about an encounter the author had with some secretive bureaucracy, and worse, from the standpoint of scholarly method, mis-citation by way of "proof" (Ernst Kantorowicz said nothing about pronouia deriving from *iqta*, p. 50; Nancy Kollmann's nuanced arguments are twisted by citation out of context, pp. 79-80).

At times Ostrowski even seems to go out of his way to avoid serious discussion of either the Muscovite institutions or the Muscovite sources. A good example is his off-the-cuff statement that the Muscovite scribes performed the same function as their Uighur equivalents (p. 45). So what, one might ask, for indeed, do not scribes tend to do scribal functions in various cultures which may co-exist and interact? In fact some of the best concrete evidence we have regarding Mongol influence on Muscovite administrative practice is precisely in terminology and practice of scribal matters; yet there is no mention of this, even though we know from Ostrowski's earlier work he is aware of this evidence. His inattention to the sources and literature on the evolution of Muscovite administration leads him to facile generalizations implying that the "loose administrative structures" of the Qipchaq Khanate were sufficient and largely unchanged throughout the Muscovite period. By way of proof (p. 91), he cites Borivoj Plavsic, whose point is just the opposite -- that by the sixteenth and seventeenth centuries Muscovy had developed a quite sophisticated formal bureaucracy. Equally problematic is Ostrowski's bold assertion of Mongol influence on "the creation of the mestnichestvo system" of precedence among the Muscovite elite (pp. 47, 104-106). The point is interesting and important. Yet he does not provide a systematic explication of the Muscovite institution with reference to the literature; the matter seems to boil down to the fact that the Mongols and Muscovites had societies in which clan relationships were important.

Another of his significant assertions is that Muscovite *pomest'e* (land grants to military men) derives from Muslim *iqta*, via the Mongols. This point has been made by others but never elaborated as here. Part of his proof seems to be the supposed ubiquitous influence of the Muslim institution, among other things on Byzantine pronoia. The strongest case Ostrowski can make is to assert that Muscovite borrowing of military tactics from the Mongols (accepted by most historians) means that "it is not unreasonable to conclude that their respective method of treating their cavalymen would be similar" (p. 52). He argues that the creation of *pomest'e* was "in large part to accommodate the influx of Tatar princes and nobility from the steppe khanates" (p. 54). True, there were many Muscovite elite families of Tatar origin, but it is beside any point for him to speculate that in fact there were many more who cannot be documented, when at the same time he pays no attention to evidence about their non-Tatar equivalents who also received *pomest'e*. We are invited to believe that percentages of different ethnic groups among the elite or other statistics which supposedly illustrate "non-compliance" of *pomeshchiki* with their service obligations are meaningful of themselves and bear some relationship to the question of why *pomest'e* was created. He wraps up this incoherent treatment of the evolution of *pomest'e* with the misleading statement that it "replaced" *votchina* in Peter the Great's law of 1714.

Part of the agenda here seems to be to make the case that borrowing from the Mongols had a lasting impact. Hence his conclusion that "pomest'e most likely derived from Islamic practice, and it was the basis for the ruling elite's power in Russia until the nineteenth century" (p. 61). Hence too his comment out of the blue that "the possible influence of the Mughal administrative structure on . . . [Peter the Great's] Table of Ranks requires further exploration" (p. 106). He bases this last on the coincidence of Peter's having met two merchants who had been to India shortly before serious work began on the Table but apparently is not acquainted with the very detailed work that has been done on the drafting of that legislation.

After concluding that the Mongols had a major impact on Muscovite political institutions, Ostrowski devotes the next three chapters to proving that they likely did not influence Muscovite seclusion of women, lead the Russians to rule as "oriental despots," or devastate the Muscovite economy. These are reasonable conclusions, but one wonders whether he has contributed much to what is already known about the topics. Particularly problematic is the chapter on seclusion of women, where his straw person for the sake of argument is Dorothy Atkinson, a "prominent scholar" indeed, but not because of her writing about Muscovite seclusion of women. The chapter does contain some fragmented comments on a subject that is directly relevant to the title of the book, the possible influence of the Mongols on Russian law. However, too much of the discussion involves perhaps well-intentioned but pointless generalizations about women in patriarchal, warrior societies, interspersed with cryptic comments such as "one cannot forget the impact that reading the misogynist book *lamentationum Matheoulouli* had on Christine de Pizan" (p. 82). The conclusion to all this is, "we must admit that not only do we not know when seclusion was introduced into Muscovy but we also do not have definite answers as to why and how" (p. 83). Not content to leave it at that, Ostrowski adds some comments on marriage politics in Muscovy (indeed an important topic) and the bride-show of 1571, with analogies in Byzantium possibly derived from steppe practice centuries earlier. This suggests that "the direct antecedent for the bride-show of 1571 could have been either Byzantium or the steppe. All this, however, is complete speculation" (p.83).

The second part of the book, on the development of an anti-Tatar ideology in the Muscovite Church, serves some important functions. He attempts to make sense out of the conundrum that Charles Halperin, among others, was unable to resolve regarding the "silence" of the early Russian sources about Mongol rule. Ostrowski's arguments dating certain chronicles and texts to the period after 1448 have a veneer of serious textual study. Apparently his publisher would not allow him to indulge in more extended textual criticism; the truncated remains that he offers are likely to seem arcane and unsatisfying to most readers, in part because to a considerable degree they merely reaffirm conclusions others have reached.

Among his most important statements, but one he does not attempt to prove, is to take issue with the idea that there was "princely" tradition of chronicle writing somehow independent from the Church. This is an essential element of his argument that the Church, once freed of the constraints supposedly imposed by its "pro-Tatar" Byzantine superiors, could enunciate anti-Tatar polemics and ultimately stood up to the secular authority's infatuation with Mongol models. This confrontation culminates in the 1560s in Tsar Ivan IV's Oprichnina, which Ostrowski explains as representing a 'Tatar' principle . . . as opposed to the 'Byzantine' principle of the Church hierarchy . . . . Ivan tried to set up the equivalent of a steppe khanate within Muscovy in which the Church had no power to speak of . . . .Indeed, the Oprichnina terror was carried out on the basis of steppe principles of collective guilt.(192, 195) He concludes that the Oprichnina "had no significant impact" (p. 198), and in the following chapter, devoted to Byzantine political thought in Muscovy, broadens his purview to assert that "Muscovy was closer to being a constitutional monarchy than an absolutist monarchy," precisely because there were institutional and theoretical restraints on the ruler.

Unfortunately, the same problems of evidence that plague the first part of the book undermine much of this scheme. In particular, the supposedly central "conflict" over Tatar institutions seems to be an artificial construct. In one case, he indicates critical textual evidence is a church response to the Oprichnina written a decade after its end, even though recent reference authorities cite the standard opinion that the text was composed before it began (p. 193). At very least here, we require some explanation for this discrepancy.

When he moves on in his last chapter to texts enunciating the idea of the "Third Rome," the connection of the material with his theme is tenuous indeed. The earliest statement of the idea is that of "a Pskovian ecclesiastic with Byzantine literary roots

expositing on the Byzantine heritage of Rus' to a government official of Tatar descent, who symbolically and actionably represented the Mongol heritage in Rus' "(p. 229). If we juxtapose the rich new book by N. V. Sinitsyna on the "Third Rome" to what Ostrowski does, we can see how impoverished is his selective treatment of the context for the texts. We might note here that her book, which appeared simultaneously with his and thus could not have been used by him, is much better informed than his regarding the literature of the last decade on the topic. Ostrowski is right about the fact that the "Third Rome" idea seems to have been little used officially in Muscovy, but as he himself admits, serious scholars have already successfully argued that. The only significant official use of the idea was at the establishment of the Patriarchate in 1589, an event that he chooses for the terminus of his study, since it is the "high watermark of Church influence on the Muscovite government in the sixteenth century" (p. 238). Yet one cannot quite figure out what to make of his idea that somehow secular and church officials had to "conspire" in achieving their goal, that advocates of the Third Rome theory were an "extremist element," and least of all that Tsar Fedor "was not a strong proponent of a pro-Tatar focus of opposition to the Church" (p. 239).

Ostrowski hoped that his admittedly "speculative" arguments would stimulate further research on Muscovite political culture. Indeed they may, although he makes it difficult for anyone to take them seriously. He further hoped that his "world history" approach would bring Muscovite history out of its isolation, countering the risk of "keeping the Muscovite field arcane and obsolete" (p.7). Unfortunately, the methodological flaws, rambling and often torturous prose, and inability to distinguish the important from the trivial are likely to have just the opposite effect. For those who wish a balanced reassessment of the old clichés concerning Mongol influence in Russia, Charles Halperin's *Russia and the Golden Horde* (1985) remains the best introduction. Moreover, it is clear that Russian specialists can place their topic into a meaningful world history framework, witness David Christian's ambitious new *History of Russia, Central Asia and Mongolia*. Dr. Ostrowski's book should never have been published in its present form, and it raises serious questions about the standards and editorial judgment of Cambridge University Press.