

Muscovite Adaptation of Steppe Political Institutions: A Reply to Halperin's Objections

Donald Ostrowski

The joke goes: how many scholars does it take to screw in a light bulb? The answer is two – one to get up on the ladder, the other to pull it out from under him or her. Charles Halperin wrote a book that has become a classic on the influence of the Golden Horde on Russia. He may well have felt, then, that I was trying to pull the ladder out from under him and his well-researched work on the subject. If he did, he could have merely dismissed my attempt to look at some of the issues anew. Instead, he opted for taking me up on my challenge to discuss the issues on the basis of the evidence, logical argument, and elegance (i.e., parsimonious comprehensiveness) of interpretation. In the process, however, Halperin has also adopted the role of a prosecuting attorney in seeking to demonstrate that I do not understand the evidence, that my arguments are faulty, and my interpretations weak. Halperin presents his case with verve and passion, and I must say that I am fortunate that I do not have to face him in a courtroom before a judge and jury, since my reply may appear rather pedestrian and didactic in comparison.

Lawyers, it needs to be pointed out, are not required to bring in evidence that counters the case they are trying to build. It is not their job to do the work of the opposition. I feel a little uncomfortable, though, taking up the cudgels as an opposition lawyer against someone with whom I am in basic agreement concerning most of what is Mongol/Tatar influence on Muscovy. These points Halperin presents well in his *Russia and the Golden Horde* and succinctly summarizes them in the article here. They include a number of political and administrative principles and institutions, certain record-keeping practices and diplomatic conventions, as well as, to quote Halperin, “military tactics and strategy . . . weapons, armaments, horse equipage, and formations.”¹ On these fundamental borrowings, we are in accord. Our disagreement concerns whether this influence extended to other Muscovite political institutions. Halperin thinks I go too far in seeing pervasive institutional borrowing, while I think he doesn't go far enough. In what follows, I will limit my reply to offering some other

¹ Charles Halperin, “Muscovite Political Institutions in the 14th Century,” 239. See also idem, *Russia and the Golden Horde: The Mongol Impact on Medieval Russian History* (Bloomington: Indiana University Press, 1985), 90–95.

relevant evidence not mentioned in Halperin's article, to delineating the conceptual differences between us, and, in the interest of scholarly inquiry, to discussing a few methodological issues.

Role of the Boyars

Halperin suggests that there might be a "linguistic sleight of hand" in "transform[ing] a social description ['all boyars'] into a decision-making political institution ['Boyars Council' or *Boiarskaia дума*]" (242). The idea that the *boiarskaia дума* was a political institution that had a prominent governmental role is a fairly standard one.² Zagoskin and Vladimirskii-Budanov were among the first to articulate this view to any extent, and Kliuchevskii provided the most detailed exposition of this position.³ So where does the idea come from that it is merely "a social description"? Halperin apparently adopts it from a series of articles published recently by S. N. Bogatyrev. But, contrary to Halperin's characterization, Bogatyrev does not criticize the "notion of a 'Duma' including all boyars" (241, n. 12). Instead, Bogatyrev writes that "it is better to look at the concept '*Boiarskaia дума*' primarily as an instrument of sociological analysis of the four highest ranks of the sovereign's court (*boiary, okol'nichie, dumnoe dvorianstvo, and dumnye d'iaki*). . . ."⁴ For Bogatyrev, the *Blizhniaia дума* (which included a

² See, inter alia, *Sovetskaia istoricheskaia entsiklopediia*, 16 vols., ed. E. M. Zhukov (Moscow: Sovetskaia entsiklopediia, 1961–76), 2: cols. 660–62; repeated in *Modern Encyclopedia of Russian and Soviet History*, 58 vols., ed. Joseph L. Wiczyński (Gulf Breeze, FL: Academic International Press, 1976–94), 5: 51–53; *Dictionary of Russian Historical Terms from the Eleventh Century to 1917*, comp. Sergei G. Pushkarev, ed. George Vernadsky and Ralph T. Fisher (New Haven: Yale University Press, 1970), 4; Michael T. Florinsky, *Russia: A History and an Interpretation*, 2 vols. (New York: Macmillan, 1947, 1953), 1: 36; *Ocherki istorii SSSR. Period feodalizma konets XV v.–nachalo XVII v.*, ed. A. N. Nasonov, L. V. Cherepnin, and A. A. Zimin (Moscow: Akademiia nauk SSSR, 1955), 117; Maxime Kovalevsky, *Modern Customs and Ancient Laws of Russia* (London: David Nutt, 1891), 154–56; Chester S. L. Dunning, "Notes," in Jacques Margeret, *The Russian Empire and Grand Duchy of Muscovy: A 17th-Century French Account*, trans. and ed. Chester S. L. Dunning (Pittsburgh: University of Pittsburgh Press, 1983), 125; Robert O. Crummey, *The Formation of Muscovy 1304–1613* (London: Longman, 1987), 12; Janet Martin, *Medieval Russia 980–1584* (Cambridge: Cambridge University Press, 1995), 286.

³ N. P. Zagoskin, *Istoriia prava Moskovskogo gosudarstva*, 2 vols. (Kazan': V Universitetskoi tipografii, 1877–79), 2: 40–76; M. F. Vladimirskii-Budanov, *Obzor istorii russkogo prava*, 2nd ed. (St. Petersburg: N. Ia. Oglobin, 1888), 155–63; cf. 6th ed. (St. Petersburg: N. Ia. Oglobin, 1909), 158–74; V. [O.] Kliuchevskii, *Boiarskaia дума drevnei Rusi*, 3rd ed. (Moscow: Sinodal'naia tipografiia, 1902 [first published 1882]); see also his *Kurs russkoi istorii* in V. O. Kliuchevskii, *Sochineniia*, 8 vols. (Moscow: Gosudarstvennoe izdatel'stvo politicheskoi literatury, 1957), 2: 343–45.

⁴ S. N. Bogatyrev, "Blizhniaia дума v tretei chetverti XVI v. Chast' pervaiia (1550-e gody)," *Arkhograficheskii ezhegodnik za 1992 god* (Moscow, 1994), 125.

subset of boyars, etc.) was the political council of advisers, whereas the social group known as “the boyars,” a.k.a. *boiarskaia дума*, had no political function. So, there is no “linguistic sleight of hand” here, merely a difference of interpretation between those who hold the standard view and a scholar who is challenging it.

The question, though, that Halperin raises concerns the exact relationship of the grand prince to the boyars and whether the boyars in council (either through a *boiarskaia* or *Blizhniaia дума*) had merely a consultative role or “possess[ed] legal, constitutional, decision-making power” (241). Our opposite answers to this question reflect a fundamentally different conception of how the Muscovite government worked at its highest levels. Although Muscovy had no formal written constitution, its internal operation was similar to the way we would expect a constitutional monarchy to operate, that is in terms of reaching decisions through institutional consultation and consensus building. In its external implementation of these decisions, in contrast, there were no such limitations on the Muscovite government. There were no civil liberties or protection of the rights of the individual, as have become associated with constitutional governments since the end of the 18th century. In other words, the Muscovite regime operated under a rule of law, but it was not a government in the common interest. Instead, it was a government of, by, and for the ruling class.⁵ The single-mindedness in implementing state decisions was what so impressed foreign travelers with the apparent absolute authority of the grand prince/tsar.⁶ Of course, these travelers were not allowed access to the inner workings of governmental decision-making, so it appeared very much a monolith to them. In order to make clear why I see the Boyar Council as a direct participant in important decision-making matters, I need to present evidence from later periods in Muscovite history, which in turn will help me to explain how I understand the 14th-century sources.

First, though, we can identify four historiographical positions concerning the Boyar Council. One is that the Boyar Council did not exist at all (it was merely “a figment of Kliuchevskii’s imagination” in Hellie’s phrase⁷) and the

⁵ In this sense the ruling class model that LeDonne has applied to Imperial Russia can be advantageously applied to Muscovy as well. See John P. LeDonne, *Absolutism and the Ruling Class: The Formation of the Russian Political Order, 1700–1825* (Oxford: Oxford University Press, 1991), 3–9, 311–12; and idem, “The Ruling Class: Tsarist Russia as the Perfect Model,” *International Social Science Journal*, no. 136 (May 1993), 285–300.

⁶ See Marshall Poe, “‘Russian Despotism’: The Origins and Dissemination of an Early Modern Commonplace” (Ph.D. Dissertation, University of California, Berkeley, 1993).

⁷ Richard Hellie, “Thoughts on the Absence of Elite Resistance in Muscovy,” *Kritika: Explorations in Russian and Eurasian History* 1: 1 (2000), 15.

boyars had no political role other than as abject slaves of the ruler. A second position is that, although the Boyar Council did not exist as a political institution, the political role of the boyars was expressed through another institution such as the *Raspravnaia palata* (Sergeevich) or *Blizhniaia дума* (Bogatyrev). A third position is that the Boyar Council existed but that it had a purely consultative role, which did not involve decision-making. The fourth position is that the Boyar Council existed and that it had not only a consultative role in providing advice but also a decision-making role in supplying consent for, or withholding consent from, the ruler's activities, especially those that involved the most important matters of state policy. I will show in the following paragraphs why I subscribe to this last view.

One of the most remarkable aspects of government that our sources testify to in 16th- and 17th-century Muscovy are formal consultative institutions that, in certain cases, also acted in a decision-making (or at least decision-sanctioning) role. One of these was the *zemskii sobor* (or Assembly of the Land); another was the Boyar Council. The fact that Muscovite grand princes and tsars could not make laws unilaterally without the approval of the "boyars" was established by tradition. Although the term *boiarskaia дума* does not appear in our sources,⁸ we do find references to a *duma* made up of boyars as well as the concept of "all the boyars" or just "the boyars" with whom the ruler is supposed to consult and from whom he is supposed to obtain approval before he can act. For example, the three law codes from 1497 to 1589 include the boyars along with the grand prince/tsar as compiling or issuing the code. The *Sudebnik* of 1497 begins: "In the year 7006, in the month of September, the Grand Prince of all Rus' Ivan Vasil'evich, with his sons and boyars, compiled a code of law. . . ."⁹ The *Sudebnik* of 1550 begins similarly: "In the year 7058, in the month of June, Tsar and Grand Prince of All Rus' Ivan Vasil'evich, with his kinsmen and boyars, issued this Code of Law."¹⁰ The *Sudebnik* of 1589 (long redaction) includes top Church prelates along with "all the princes and boyars" as deciding and issuing the code together with the tsar.¹¹ The *Ulozhenie* of 1649 merely states that Tsar Aleksei "discussed (*govoril*) [the articles] with his state boyars, *okol'nichie*, and *duma* people" before issuing the code, but it goes on to say that both former sovereigns' decrees (*ukazy*) and "boyars' decisions" (*boiarskie prigovory*) were

⁸ The closest is Fletcher's *Boarstva dumna*. Giles Fletcher, *Of the Russe Common Wealth, or Maner of Governement by the Russe Emperour, (Commonly Called the Emperour of Moskovia) with the Manners, and Fashions of the People of That Country* (London: T. D. for Thomas Charde, 1591), 35^f.

⁹ *Sudebniki XV–XVI vekov*, ed. B. D. Grekov (Moscow and Leningrad: Akademiia nauk SSSR, 1952), 19.

¹⁰ *Sudebniki XV–XVI vekov*, 141.

¹¹ *Sudebniki XV–XVI vekov*, 366.

collected.¹² And a subcommittee headed by boyars drafted the law code in the first place. Numerous decrees contain the formula “the Grand Prince decreed with the boyars. . .” or similar formulas indicating that the boyars and the grand prince on certain important matters decreed together.¹³ If the boyars were just giving their advice, it is unlikely they would be included in the formulation as also issuing these documents. In addition, we have a decree from February 1606 that the boyars issued themselves.¹⁴ At times, the formula changes somewhat. In a decree of November 24, 1597, we find the phrase: “the Tsar and Grand Prince Fedor Ivanovich of all Rus’ decreed and the boyars resolved (*prigovorili*).”¹⁵ Although one might argue that this represents some kind of diminution of the boyars’ role, the fact that their resolve is considered important enough to be included is significant. This is not to say that the ruler would not on occasion defy the Council and act on a significant matter on his own. But that could be cause for problems, as we read in the Statute of March 9, 1607, in which Tsar Vasiliu Shuiskii criticizes a previous tsar, Fedor (and Boris Godunov), for “not listening to the Council of the senior boyars” in regard to the issue of whether peasants should have the right of free departure.¹⁶

What we also find is that those documents (in which the boyars participate in decreeing) are the most significant acts of the government, namely law codes,

¹² *Sobornoe ulozhenie 1649 goda. Tekst. Kommentarii*, ed. L. I. Ivana (Leningrad: Nauka, Leningradskoe otdelenie, 1987), 17.

¹³ See, e.g., *Sbornik Imperatorskogo Russkogo istoricheskogo obschestva* (SRIO), 148 vols. (St. Petersburg, 1867–1916), 35: 503, no. 85; 35: 630, no. 93; *Pamiatniki russkogo prava* (PRP), 8 vols. (Moscow: Gosudarstvennoe izdatel'stvo iuridicheskoi literatury, 1952–61), vol. 4: *Pamiatniki prava perioda ukrepleniia russkogo tsentralizovannogo gosudarstva XV–XVII vv.*, ed. L. V. Cherepnin, 486, 487, 495, 514, 515, 516, 517–18, 524, 526, 529; PRP, 5: 237; *Tysiachmaia kniga 1550 g. i Dvorovaia tetrad' piatidesiatykh godov XVI veka*, ed. A. A. Zimin (Moscow and Leningrad: Akademiia nauk SSSR, 1950), 53. The Nikon Chronicle describes the issuance of the decree of 1556 this way: “In the year 7064, Tsar and Grand Prince Ivan Vasil'evich of All Rus' with his kinsmen and with the boyars issued a decree on *kormleniia* and service.” *Polnoe sobranie russkikh letopisei* (PSRL), 40 vols. (St. Petersburg/Petrograd/Leningrad and Moscow: Arkheograficheskaiia komissiiia and Nauka, 1843–1995), 13.1: 267.

¹⁴ PRP, 4: 540.

¹⁵ *Akty istoricheskie, sobrannye i izdannye Arkheograficheskoi komissiei*, 5 vols. (St. Petersburg, 1841–42), 1: 420, no. 221.III. One finds this same formula in later decrees. See, e.g., *Polnoe sobranie zakonov Rossiiskoi Imperii, s 1649* (PSZ), 1st series, 46 vols. (St. Petersburg: Tipografia II Otdeleniia Sobstvennoi Ego Imperatorskogo Velichestva kantseliarii, 1830), 2: 15–16, no. 632 (1676); 2: 16–26, no. 633 (1676); 2: 26–31, no. 634 (1676); 2: 35–36, no. 643 (1676); 2: 36–40, no. 644 (1676); 2: 41, no. 645 (1676); 2: 294, no. 857 (1681); 2: 295, no. 859 (1681); 2: 299–300, 302, no. 860 (1681); 2: 304, no. 861 (1681); 2: 538, no. 1018 (1683); 2: 582, 585, no. 1070 (1684).

¹⁶ PRP, 4: 587.

foreign treaties, and precedent-setting measures. Other, less important decrees, such as *kormlenie*, *votchina*, and *pomest'e* grants, judicial immunities, local treaties, etc., are clearly the prerogative of the ruler alone. Of course, as we might expect, there is always an area of ambiguity, as in the case of Tsar Fedor's decree on peasant departures mentioned above, where the ruler (or those acting for him) and the boyars may be in disagreement concerning whether a particular issue fell into the domain of ruler prerogative alone or required the ruler and boyars acting together.

If the boyars had a relationship to the tsar roughly equivalent to that which a legislative institution has toward the chief executive in a modern constitutional government, namely one of advice and consent concerning important matters of state policy, then we might expect to see exactly these types of formulations in the documents.¹⁷ But we do not have to limit ourselves to extrapolating. We have an explicit statement of this relationship written into the *Sudebnik* (Law-code) of 1550. The original text of Article 98 of the *Sudebnik* states: "And whatever new matters there will be, but [such that] are not written in the *Sudebnik*, and however these matters are resolved with the report (*doklad*) of the sovereign and verdict (*prigovor vershaetsia*) of all the boyars, those matters are to be appended to this *Sudebnik*."¹⁸ Sergeevich, among others, has attempted to explain away this inconvenient bit of evidence,¹⁹ but I find none of these arguments convincing, especially in light of the confirmatory evidence in decrees of the time indicating the need for boyar approval.²⁰ In addition, Kotoshikhin provides a description of how the Boyar Council (or boyars in council) worked:

And when the tsar sits in the Duma with those boyars and Duma men [to discuss] foreign affairs and the affairs of his own state, the boyars and *okol'nichie* and *dumnye dvoriane* seat themselves on benches, some distance from the tsar, according to rank. . . . And they deliberate whatever needs to be deliberated, together with the tsar, as is the custom in other states. And when the tsar speaks his mind on some matter, having

¹⁷ Hellie raised the question why there was no boyar opposition to the ruler but offered a different explanation from the one presented here. Hellie, "Thoughts," 11–19.

¹⁸ *Sudebniki XV–XVI vekov*, 176, and "Commentary," 334–37. One 17th-century copy substitutes the word "reports" for "boyars," but that is more likely a scribal error than some kind of attempt to eliminate the role of the boyars.

¹⁹ V. [I.] Sergeevich, *Drevnosti russkogo prava*, 3rd ed., 3 vols. (St. Petersburg: M. M. Stasiulevich, 1903–09), vol. 2: *Veche i kniaz'. Sovetniki kniazia*, 499–500; cf. idem, *Lektsii i issledovanie po drevnei istorii russkogo prava*, 4th ed. (St. Petersburg: M. M. Stasiulevich, 1899), 266–84.

²⁰ Vladimirskii-Budanov, in later editions of his book on the history of Russian law, refuted Sergeevich's argument concerning article 98. See, e.g., Vladimirskii-Budanov, *Obzor istorii russkogo prava*, 6th ed., 165–68, n. 1.

spoken, he orders them, the boyars and Duma men, to deliberate and to give that matter their attention (*sposob*). . . . And whatever they affirm (*prigovoriat*) on any matter, the tsar and boyars order the Duma secretaries to make note of it and write down that affirmation.²¹

Kotoshikhin goes on to recount a regular procedure for deciding matters with the Council. The ruler can, if he so desires, discuss matters with a smaller group made up of certain boyars and *okol'niche*, the rest being excluded. This smaller group, who meet with the ruler in an inner closed session, we can call the *Blizhniaia дума* (or inner council). Accordingly, we can call the larger group, some of whose members are excluded from the smaller meeting, the Boyar Council. In any case, Kotoshikhin makes it clear that matters decided by the ruler and inner council together must be approved by the Council as a whole. He describes four readings of official documents: first, to the members of the inner council alone; then to the inner council with the tsar; third, to the members of the full council; and finally to the full council and tsar together. In addition, the Boyar Council met on a regular basis. Fletcher writes that it met every Monday, Wednesday, and Friday at 7:00 a.m., but Kotoshikhin tells us that in his time (the early to mid-17th century), it met five days a week, both morning and evening.²² And Olearius reports that Duma sessions could last up to 7 or 8 hours.²³ It is doubtful these were merely social gatherings.

We also find confirmation of something more than just a consultative role for the boyars in the advice given by Dmitrii Donskoi to his sons in the *Life of Grand Prince Dmitrii Ivanovich* written in the 15th century: "Love your boyars, render them the honor they deserve according to the deeds of each, and do nothing without their will (*bez volia ikh*)."²⁴ It is unlikely the hagiographer is

²¹ Grigorii Kotoshikhin, *O Rossii v tsarstvovanie Alekseia Mikhailovicha*, 4th ed. (St. Petersburg: Tipografiia Glavnogo upravleniia udelov, 1906) (Slavistic Printings and Reprintings, no. 126, The Hague: Mouton, 1969), 24. Cf. Benjamin Uroff, "Grigorii Karpovich Kotoshikhin, *On Russia in the Reign of Alexis Mikhailovich: An Annotated Translation*" (Ph.D. dissertation, Columbia University, 1970), 67.

²² Fletcher, *Of the Russe Common Wealth*, 36^f; Kotoshikhin, *O Rossii v tsarstvovanie Alekseia*, 29.

²³ *The Travels of Olearius in Seventeenth-Century Russia*, trans. and ed. Samuel H. Baron (Stanford: Stanford University Press, 1967), 221. See also Kliuchevskii, *Boiarskaia дума*, 407–08.

²⁴ "Slovo o zhit'ii i o prestavlenii velikago kniazia Dmitria Ivanovicha, tsaria ruskago," ed. M. A. Salmina, in *Pamiatniki literatury drevnei Rusi. XIV–seredina XV veka*, ed. L. A. Dmitriev and D. S. Likhachev (Moscow: Khudozhestvennaia literatura, 1981), 216. I translate *volia* as "will" (meaning *agreement*) because the word appears here with a similar meaning to its appearance in the *iarlyk* of Mengü-Temir (ca. 1267) where it is used in the translation of the "will of God" or "will of Divine Heaven." PRP, vol. 3: *Pamiatniki prava perioda obrazovaniia russkogo tsentralizovannogo gosudarstva XIV–XV vv.*, ed. L. V. Cherepnin, 467. Presumably, then, a Church writer would not apply such a strong term as *volia* indiscriminately within a political context.

reporting Dmitrii's exact words, but the fact that approval of the boyars occupies such a prominent place in the *Life* as Dmitrii's final deathbed admonition to his sons would seem to indicate a normative view of the relationship of the ruler to the boyars. Grand Prince Semen in his Testament of 1353 tells his brothers to listen to the advice of Metropolitan Aleksei and the "elder boyars."²⁵ Even Ivan IV, in his Testament of ca. 1572, expressed concern the boyars would "deprive (*izgnati*) him of his possessions (*dostoianiia*) because of his willfulness."²⁶ Although one might try to explain away this concern as some kind of paranoia on Ivan's part, the fact that such a statement appears in an official document would seem to indicate that the boyars had sufficient power to do so. And it would fit in with the plan that some boyars had around this time of replacing Ivan with the Crimean khan.²⁷

The principle of boyar consultation and approval appears in the Oath of 1606 that Vasilii Shuiskii took when he became tsar whereby he swore due process of law through "regular trial with my boyars" for all Orthodox Christians.²⁸ It also appears in the stipulations to Władysław to become tsar in 1610. Specifically, Władysław was to promise not to imprison anyone, confiscate anyone's estates, or change anyone's rank without the consent of the boyars.²⁹ Contrary to the notion that the stipulations to Władysław were a possible turning point to constitutionalism where Russia failed to turn, these limitations had already been well established in Muscovite law and practice. The boyars were merely ensuring that their role would continue to be guaranteed under a Polish king.³⁰ And there are certain respects in which the role of the boyars went beyond even that of modern constitutional legislatures. In Muscovy the ruler could meet with foreign envoys only with the boyars present. In 1489, for example, Ivan III told Nicholas Poppel, the ambassador of the Holy Roman Emperor, that he could

²⁵ *Dukhovnye i dogovornye gramoty velikikh i udel'nykh kniazei XIV–XVI vv.* (DDG), ed. L. V. Cherepnin (Moscow and Leningrad: Akademiia nauk SSSR, 1950), 14, no. 3.

²⁶ DDG, 427, no. 104.

²⁷ Daniel Prinz [Printz], *Moscoviae ortus, et progressus* (Gubena: Christopher Gruber, 1681) 76; reprinted in *Scriptores Rerum Livonicarum*, 2 vols. (Riga and Leipzig: Eduard Franken, 1853), 2: 702. Daniil Prints, "Nachalo i vozvyshenie Moskovii," trans. I. A. Tikhomirov, *Chienie v Obshchestve istorii i drevnostei rossiiskikh pri Moskovskom universitete* (1876), no. 3, 22.

²⁸ *Akty, sobrannye v bibliotekakh i arkhivakh Rossiiskoi imperii Arkheograficheskoi ekspeditsiei imperatorskoi Akademii nauk* (AAE), 4 vols. (St. Petersburg, 1836), 2: 102, no. 44; cf. PSRL, 14: 69.

²⁹ *Akty, otmosiashchiesia k istorii Zapadnoi Rossii, sobrannye i izdannye Arkheograficheskoi komissiiu*, 5 vols. (St. Petersburg: Eduard Prats, 1846–53), vol. 4: 1588–1632, 315–16.

³⁰ This point has been made before. See, e.g., Robert O. Crummey, "Constitutional Reform During the Time of Troubles," in *Reform in Russia and the U.S.S.R.: Past and Present*, ed. Robert O. Crummey (Urbana: University of Illinois Press, 1989), 31.

not meet him without the boyars.³¹ The minutes of the Ambassadorial Chancellery (*Posol'skii prikaz*) as well as accounts of foreign ambassadors to Muscovy attest that this practice was rarely violated.³² The ruler was thereby insulated from making deals with foreign powers inimical to the interests of the boyars.

Who Was a Boyar and When?

We now come to the issue of boyars' witnessing grand princely documents in the 14th century. Significantly, they witness only some documents and not others. Halperin has scolded me for "play[ing] fast and loose" (242) with the numbers of boyar witnesses, and he sees no pattern concerning whether a 14th-century document has witnesses or not, or in the numbers of witnesses for those that have such. He asserts that my claim of borrowing is based on there being four boyars in the Boyar Council until 1375. Because there were not four boyars, the Muscovites, he argues, could not have borrowed this institution. He concludes that, "since we know nothing of the selection criteria of *ulusbeys* [i.e., *qarachi beys*], . . . [the] principles of selection of membership" in the Boyar Council did not "match the Council of four *ulusbeys*" (246–47). Thus, Halperin sees two main differences – size and criteria of selection – that exclude the possibility of a connection between the two institutions.

Contrary to Halperin's assertion, my claim that the Muscovite Boyar Council was borrowed and adapted from the Qipchaq Khanate's divan of *qarachi beys* is not dependent on the presence of four boyar witnesses to these 14th-century documents. In fact, I base my argument on correspondence in function rather than correspondence in number. The Boyar Council of Muscovy and the divan of the *qarachi beys* of the Qipchaq Khanate fulfilled the same role vis-à-vis the ruler, namely one of advice and consent. I had come to the conclusion about the modeling of the 14th-century Muscovite governmental structure on that of the Qipchaq Khanate before I noticed what appeared to be a relic of this borrowing in the number of boyar witnesses. The number of witnesses is thus incidental, not essential, to my argument.

Nonetheless, let us take a look at the documents Halperin cites. In Table 1, I arranged them according to the number of boyar and *okol'nichii* "witnesses." Under no witnesses, we see three types of documents – three grants, three testaments, and two local treaties (in this case with the grand prince's cousins). Two of these types – grants and local treaties – also do not have boyar witnesses in

³¹ *Pamiatniki diplomaticheskikh snoshenii drevnei Rossii s derzhavami inostrannymi* (PDS), 10 vols. (St. Petersburg: [V Tip. II Otdeleniia Sobstvennostoi E. I. V. Kantseliarii], 1851–71), 1: col. 1.

³² For the minutes, see PDS cited above. For a list of foreigners' diplomatic reports from 1486 to 1699, see Marshall Poe, *Foreign Descriptions of Muscovy: An Analytic Bibliography of Primary and Secondary Sources* (Columbus, OH: Slavica, 1995), 205–10.

Table 1: Secular “Witnessing” of 14th-Century Documents (to 1375)³³

No Witnesses	1 “Witness”	4 Witnesses
grant of Ivan I (1328–40)	immunity grant (1363–74) (mentions <i>tysiatskii</i> Vasilii)	treaty with brothers (1350/51)
grant of Ivan I (1328–40)	immunity grant (1363–89)	truce with Olgerd (1371)
testament of Ivan I (1339)	(signed by Timofe[i]Vasil’evich”)	testament of Dmitrii (1375)
testament of Semen (1353)		
testament of Ivan II (1358)		
grant of Dmitrii (1363–89)		
treaty with cousins (1367)		
treaty with cousins (1374)		

the 15th, 16th, and 17th centuries. When we look at the types of documents that have boyar witnesses from four different families, we find a rather significant treaty between Grand Prince Semen and his brothers, Ivan and Andrei. It was a precedent-setting measure that established the working relationship between the grand prince and his brothers for the next 200 years.³⁴ In addition, we see a foreign truce and a grand princely testament, both of which also require boyar approval in later centuries. We also observe that a notable change has taken place in relation to the grand princely testament. From 1339 to 1358, grand princely testaments have no secular elite (boyar) witnesses, although they do have clerical witnesses. In 1375, secular elite witnesses are required. We have no direct indication why this change takes place, but one notes the continuation

³³ Although the treaty of 1350/51 has six witnesses, two of them are “subordinate” witnesses, being the son and son-in-law of Vasilii Protas’evich, the main Vel’iaminov witness. In addition, I do not include on this table the treaty of 1375 with Mikhail of Tver’ because our only copy of it is from the 15th century, which means the names of witnesses might not have been carried over.

³⁴ For a discussion of the significance of this treaty, which he calls of “capital importance,” see John Fennell, *The Emergence of Moscow, 1304–1359* (Berkeley: University of California Press, 1968), 288.

of this practice with regard to subsequent grand princely testaments.³⁵ The posthumous distribution of grand princely wealth apparently becomes a matter of state policy at this time.

Under 1 “witness” we have two documents, neither of which is what Halperin claims it to be. The mention of the *tysiatskii* Vasiliï in the immunity grant dated 1363–74 is not relevant to our discussion because he is not witnessing the document. Instead, the document states that the recipient of the grant is to give to the *tysiatskii* Vasiliï the “rent” (*obrok*) owed the grand prince. There is no reason for anyone else of boyar rank to be mentioned in the document. The signing by someone of boyar rank of the other immunity grant (dated 1363–89) may seem unusual, but we do have examples from the first half of the 15th century of individual signings of grand princely immunity grants by boyars,³⁶ not to mention signings by the grand prince himself³⁷ as well as clerks (*d'iaki*) beginning to do the same by the middle of the century.³⁸ Signing immunity grants may be less a function of rank than of literacy or of some other circumstance. In any case, the “Timofei Vasil'evich” who signed this document might not be the *okol'nichii* T. V. Vel'iaminov, as Halperin asserts, but Timofei Vasil'evich Volui, who does not appear to have been of boyar rank when he was

³⁵ DDG, 36–37 no. 12 (10 boyars); 57, no. 20 (7 boyars); 59, no. 21 (5 boyars); 62, no. 22 (6 boyars); 198, no. 61a (4 boyars [in addition, 2 boyars witnessed the attached codicil. Ibid., 199, no. 61b]); and 364, no. 89 (3 boyars and the treasurer).

³⁶ *Akty sotsial'no-ekonomicheskoi istorii severo-vostochnoi Rusi kontsa XIV–nachala XVI v.* (ASEI), 3 vols. (Moscow: Nauka, 1964), 1: 41–42, no. 30 (1415) and no. 31 (1415–1425), 1: 47–48, no. 41 (before 1425), 1: 48–49, nos. 43 (before 1425) and 44 (1425), and 1: 55–56, no. 55 (1428–1432), “Ivan Dmitrievich” (thought to be I. D. Vsevolozh); ASEI, 1: 51–52, nos. 47 and 48 (1425–1427), “Ivan Dmitrievich” and “Ivan Fedorovich” (thought to be I. F. Koshkin); 1: 57, no. 57 (1428–1432), “kniaz' Iurii Patrekeevich”; 1: 84, no. 105 (1432–1455), “Ivan Kostiantinovich” (thought to be I. K. Dobrynskii); 1: 94, no. 117 (1435), “Andrei Kostiantinovich” (thought to be A. K. Dobrynskii); 1: 99, no. 128 (1436), “Petr Kostiantinovich” (thought to be P. K. Dobrynskii); 3: 466, no. 481 (ca. 1405–1415), “Mikhailo Andreevich” (thought to be the same person who witnessed the second and third testaments of Vasiliï I. DDG, 59, no. 21, and 62, no. 22); and 136, no. 99 (1433/34), “Fedor Mikhailovich” (thought to be the same person who was mentioned as a boyar under Vasiliï II. DDG, 180, 184, no. 58 [1451–1456]; 199, no. 61b [1461–1462]).

³⁷ ASEI, 1: 66, no. 74 (1432 or 1433); 1: 67, no. 76 (1432–1455); 1: 71, no. 84 (1432–1455); 1: 76, no. 92 (1432–1455); 1: 77, no. 93 (1432–1455); 1: 79, no. 98 (1432–1455); 1: 81, no. 101 (1432–1445); 1: 84, no. 104 (1432–1445); 1: 86, no. 107 (1432–1462); 1: 88, no. 109 (1433/34); 1: 105, no. 136 (1438+); 1: 107, no. 139 (1439); 1: 122, no. 166 (1440–1445); 1: 124, no. 170 (1441); 1: 130, no. 180 (1446); 1: 134, no. 189 (1447); 1: 142, no. 197 (1447–1455); 1: 142, no. 199 (1447–1455); 1: 144, no. 202 (1447–1455); 1: 145, no. 203 (1447–1455); 1: 150, no. 215 (1447–1455); 1: 161, no. 225 (1449); and 1: 161, no. 226 (1449–1455).

³⁸ ASEI, 1: 144, no. 201 (1447–1455); and 1: 160, no. 224 (1449).

killed at Kulikovo in 1380.³⁹ The document in question does not identify the rank of the signer.

In sum, multiple boyar and *okol'nichii* witnesses occur only with a certain class of documents – the most important ones in regard to state policy. Therefore, I do see a pattern where Halperin sees only randomness and coincidence. But neither of us, I would suggest, is “playing fast and loose” with the evidence.

Potentially more noteworthy is Halperin’s assertion that not all the people who were boyars at the time these documents were issued appear as witnesses. He compiled a table (256–57) to demonstrate this point, and we are indebted to him for attempting to provide a visual representation of the 14th-century *boiarsstvo*. There are then two possible implications of Halperin’s assertion: (1) if all members of the Boyar Council witnessed these documents, then not all boyars were members of the Boyar Council or (2) if not all members of the Boyar Council witnessed these documents, then the Boyar Council was made up of more than four members. In either case, one or another of my conclusions about the Boyar Council becomes unsupportable.

Concerning the truce of 1371 with Olgerd (Algirdas), Halperin writes: “Boyars I. R. Kvashnyn, D. M. Minin, V. V. Vel’iaminov and his brother T. V. Vel’iaminov did not witness the document” (245). This statement is true without, however, being significant. Kvashnin is first mentioned as a boyar in ca. 1375 as a result of his witnessing the testament of Dmitrii, four years *after* the truce with Olgerd. Dmitrii Minin was killed at the Trotna River in a battle against Olgerd’s army on November 21, 1367, four years *before* the truce with Olgerd.⁴⁰ V. V. Vel’iaminov’s presence on Halperin’s list during this time is based upon his having witnessed the immunity grant of Grand Prince Dmitrii dated by the editors of the published version to the period 1363–74.⁴¹ The dates represent a range of years when V. V. Vel’iaminov could have been *tysiatskii*. The earlier date stands for the year Dmitrii Ivanovich was appointed grand prince by Khan Mürid and the later date the year of V. V. Vel’iaminov’s death.⁴² It indicates not that he was *tysiatskii* throughout this period, as Halperin’s table has it, but that he was *tysiatskii* at some point during this

³⁹ PSRL, 1 (1927): col. 536; 4 (1925): 321; 6: 95–96; 8: 40; 11: 54, 64, 65; 18: 130; 25: 204; 28: 82, 245; 39: 122. No rank is assigned him.

⁴⁰ PSRL, 1 (1927): col. 533; 11: 11; 18: 108; 25: 185; 28: 76, 237; 34: 117; 39: 114. See also *Drevniaia rossiiskaia vivilofka* (DRV), ed. Nikolai Novikov, 20 vols. (Moscow: V tipografii Kompanii tipograficheskoi, 1788–91), 6: 450.

⁴¹ ASEI, 3: 259–60, no. 238.

⁴² The chronicle accounts, however, place his death in September 1373. PSRL, 8: 21; 11: 21; 18: 115; 25: 189; and 28: 77, 239. Halperin accurately reflects this difference in his table, but not in his text (247).

period. Thus, we have no other evidence indicating whether he was *tysiatskii* by 1371 or attained that position afterward. I would say that the absence of his name on the truce with Olgerd is evidence that he attained the position after 1371. T. V. Vel'iaminov, on the other hand, is not on Halperin's list until 1389. His presence on the list at this date is based on his having witnessed the testament of Dmitrii in 1389. But the earliest specific evidence that can be used for his having attained boyar rank is the name "Timofei Vasil'evich" on Dmitrii's testament of ca. 1375. This, too, is four years after the truce with Olgerd. So, we have no definitive evidence that any of the four individuals that Halperin mentions were of boyar rank at the time of the 1371 document, as well as clear evidence that one of them was no longer living.

In addition, Halperin's table identifies the "Dmitrii Aleksandrovich" of the 1371 document with D. A. Monastyrev. The only reliable chronicle evidence we have about D. A. Monastyrev tells us that he was killed at the battle on the Vozha River in 1378.⁴³ The Patriarchal genealogy book (*rodoslovnaia kniga*), the Rumiantsev genealogy book, and another genealogy book in a redaction of the second half of the 16th century report that he had four or five daughters who married.⁴⁴ But neither the chronicles nor the genealogy books stipulate that he was a boyar. Indeed, the report of his death in 1378 is mentioned along with that of Nazar Danilov, son of Kusak, who was not a boyar. In honor-conscious Muscovy, it would have been unlikely a boyar's death would have been placed in the same category with a non-boyar without distinguishing the two. So the identification of our witness with D. A. Monastyrev is rather weak.

Veselovskii proposed that the Dmitrii Aleksandrovich of the 1371 truce was either D. A. Vsevolozh⁴⁵ or a boyar of the appanage Prince Vladimir Andreevich.⁴⁶ As in the attempt to identify "Dmitrii Aleksandrovich" with D. A. Monastyrev, this attempt to identify him with D. A. Vsevolozh is weakened by the absence of evidence concerning when he attained boyar rank.

⁴³ PSRL, 4 (1915): 310; 11: 43; 16: col. 106; 18: 127; 25: 200; 27: 71; 28: 81, 244; 39: 118. See also DRV, 6: 450. A number of chroniclers include his name along with the title "prince" in the list of those killed at Kulikovo in 1380. In some cases, the same chronicles also report the information that he was killed at the Vozha two years earlier. PSRL, 11: 65; 27: 75, 253, 332; and 39: 122. Barring the possibility he arose from the dead, we can say that the 1380 report is a faulty interpolation and should not be accepted as reliable evidence.

⁴⁴ "Rodoslovnaia kniga," *Vremennik Imperatorskogo moskovskogo obshchestva istorii i drevnostei rossiiskikh*, vol. 10: *Materialy* (Moscow, 1851), 123, 262; *Novye rodoslovnye knigi XVI v.*, ed. Z. N. Bochkareva and M. E. Bychkova, in *Redkie istochniki po istorii SSSR*, 2 fascicles (Moscow: Institut istorii SSSR AN SSSR, 1977), fasc. 2, p. 170.

⁴⁵ S. B. Veselovskii, *Issledovaniia po istorii klassa sluzhilykh zemlevladel'tsev* (Moscow: Nauka, 1969), 332.

⁴⁶ Veselovskii, *Issledovaniia po istorii klassa*, 489.

Although the Patriarchal genealogy book identifies him as the son of Prince Alexander Vsevolodich of Smolensk,⁴⁷ we do not know when he entered the service of the Muscovite grand prince. The Nikon Chronicle interpolates his name in the list of those who fought at the Kulikovo battle,⁴⁸ but such evidence is not to be trusted. Besides, many of those who fought on the Muscovite side at the battle were not in direct service to Grand Prince Dmitrii. Other chronicles know him only as a *namestnik* in Nizhnii Novgorod in 1392.⁴⁹

In at least two cases, when Veselovskii could not match up a name of a witness to one of our documents with a known Muscovite boyar, he proposed that individual was a boyar of an appanage prince. Here is the origin of the assertion, repeated by Halperin, that the Ivan Mikhailovich who witnessed both the 1350 treaty and the 1371 truce was an “appanage boyar.” This raises the point, however, that if he was not a “grand princely boyar,” then Halperin should not count him among the number of “Muscovite Boyars” on his table. Yet, calling him an appanage boyar merely replaces one unknown with another, for if we have no other evidence of an “Ivan Mikhailovich” at the grand princely court, we also have no other evidence of an “Ivan Mikhailovich” at an appanage court. Given this lack of corroborative evidence either way, the idea that an appanage boyar was witness to a grand princely document seems to be an unnecessary complication. Therefore, it is better to conclude that Ivan Mikhailovich was a grand princely boyar about whom other evidence is lacking rather than an appanage boyar about whom other evidence is lacking. Still and all, the case of “Dmitrii Aleksandrovich” is different, for we do have evidence of a Dmitrii Aleksandrovich who was a boyar at the grand princely court in the 14th century, and that is D. A. Zerno (not on Halperin’s list). The *Sinodik* of the Uspenskii Cathedral lists him among other boyars,⁵⁰ and the *Barkhatnaia kniga* and the Chronicle genealogy book position him as the founder of the Saburov-Godunov clan.⁵¹ Although we do not have exact dates for his birth and death, we can surmise on the basis of his place in the genealogy books that his time of flourishing was mostly during the third quarter of the 14th century.⁵² Thus, he would have

⁴⁷ “Rodoslovnaia kniga,” 164.

⁴⁸ PSRL, 11: 54.

⁴⁹ PSRL, 8: 62; 28: 86, 250; and 34: 143.

⁵⁰ DRV, 6: 453. Identified by Veselovskii in his *Issledovaniia po istorii klassa*, 165, n. 9.

⁵¹ *Rodoslovnaia kniga kniazei i dvorian rossiiskikh i vyezshikh*, 2 vols. (Moscow, 1787), 1: 240. *Novye rodoslovnnye knigi*, 50. But cf. A. A. Titov, *Sinodiki XVII i XVIII vv. Rostovskogo Uspenskogo sobora* (Rostov: A. Kh. Opperl’, 1903), 30.

⁵² “Rodoslovnaia kniga,” 162, 256. Veselovskii suggested the second and third quarters of the 14th century. Veselovskii, *Issledovaniia po istorii klassa*, 163. But, if Dmitrii’s grandfather Chet arrived in Muscovy in 1330, as the genealogy books state, his full maturity would have to have

been advanced in years at the time of the 1371 truce with Olgerd, which would explain why he was not around in 1375 to witness Dmitrii's first testament. In addition, the chronicles provide an explanation for why D. M. Volynskii could not have been a witness to that testament – he was on campaign against the Bulgars in 1375/76.⁵³

There are further problems with Halperin's table, which he acknowledged that he extrapolated from other historians' work (244). Both "Andrei [sic, should be Aleksei – DO] Petrovich Khvost" and "Andrei Ivanovich Kobyla" are listed as boyars in 1346–47, with Khvost continuing on as boyar until 1356. One may well wonder, then, why Khvost did not witness the 1350/51 treaty. The claim that Khvost was a boyar in 1346/47 relies on the application of what Lur'e termed the "consumer approach" to sources – grabbing evidence from here and there without regard to the reliability of the respective sources of information. One piece of evidence that is used for both Khvost's and Kobyla's being boyars in 1346/47 is an interpolation in the Patriarchal genealogy book of a chronicle entry concerning Grand Prince Semen's marriage to Mariia, daughter of Prince Alexander Mikhailovich of Tver', specifically that "Andrei Kobyla and Aleksei Bosovolkov went for her":

Chronicle Account

a ezdili po nee Andrei
Kobyla da Aleksei Bosovolkov⁵⁴

Patriarchal genealogy book

a ezdili po nee *boiare* Andrei
Kobyla da Aleksei Bosovolkov⁵⁵

It is clear from the exact correspondence in wording that the compiler of the Patriarchal genealogy book was using the chronicle account. Here we encounter an unmarked conjecture on the genealogist's part that they must have been boyars to be sent on such an important mission, yet it does not necessarily follow that they had to be boyars. And I would suppose, in any case, that the grand prince's officials, not boyars, would have been sent on this type of mission. Other missions of high importance were undertaken for the Muscovite grand prince by individuals who were not boyars. But even if one concludes that Aleksei and Andrei were boyars, the Aleksei mentioned here is not necessarily Aleksei Petrovich Khvost. In other words, we come across another unmarked conjecture, that Aleksei Bosovolkov is Aleksei Petrovich Khvost, but I know of no reason,

been late in the second quarter, at the earliest, and most of the third quarter.

⁵³ PSRL, 8: 24; 11: 25; 18: 117; 20: 196; 24: 133; and 25: 192.

⁵⁴ M. D. Priselkov, *Troitskaia letopis'. Rekonstruktsiia teksta* (TL) (Moscow and Leningrad: Akademiia nauk SSSR, 1950), 368; PSRL, 7: 210; 10: 218; 18: 95; 25: 176; 28: 71, 232; and 34: 110.

⁵⁵ "Rodoslovnaia kniga," 146.

besides their having the same first name, to claim that they were one and the same person. Here is where another piece of specious evidence comes into play. A genealogy list found in a late 18th-century copy of a *Razriadnaia kniga* (Service book) states that Aleksei Petrovich Khvost is the son of Peter Bosovolkov (Basavolk).⁵⁶ No relationship between A. P. Khvost and Peter Bosovolkov is described in any other source. Even if we were to accept this rather late and dubious testimony, we cannot exclude the possibility that the Aleksei Bosovolkov mentioned in the chronicle account is Peter's brother or some other relation since no patronymic is provided in the original account.⁵⁷ Veselovskii just assumed they were the same person without explanation.⁵⁸ In the end, the claim that Aleksei Petrovich Khvost was a boyar in 1346/47 derives from a chronicle entry in which Aleksei Bosovolkov is not called a boyar.

Additionally, the treaty of 1350/51 states that Aleksei Petrovich had been involved in a conspiracy (*koromola*) against Semen, and he was not to receive aid from Semen's brothers.⁵⁹ If he had been *tysiatskii* before this, it is clear why he was not *tysiatskii* at the time and therefore not a witness to the document.⁶⁰ The treaty itself testifies that Vasilii (presumably Vel'iaminov) was then *tysiatskii*. Also it has been proposed that Khvost became *tysiatskii* only after Semen's death in 1353, when Ivan II was declared grand prince.⁶¹ We thus have two rather weak, unexplained suppositions leading to a faulty conclusion: that Bosovolkov was a boyar in 1347, that Khvost was Bosovolkov, and that, therefore, Khvost must have been a boyar in 1350/51. Yet, even if one accepts this tortuously arrived at equation, when one reads the treaty itself one finds an explanation why Khvost could not have been a witness to it. The end result of these unexplained conjectures, neglect of reliable source testimony, and uncritical acceptance of a late 16th-century interpolation and of a late 18th-century supposition shows up on Halperin's table as definitive evidence to help demonstrate that the number of boyars was not fixed at "four" at any time during the 14th century.

⁵⁶ The genealogy list is published in N. P. Likhachev, *Razriadnye d'iaki XVI veka. Opyt istoricheskogo issledovaniia* (St. Petersburg: V. S. Balashev, 1888), 392–96.

⁵⁷ In the Uvarov copy of the "Compilation (*Svod*) of the End of the 15th Century," someone wrote in the word "Petrovich" above the line, but it is in different ink and we cannot judge when that was done. See PSRL, 25: 176, note b. It may be connected with the conjecture on the part of the copyist of the Service book.

⁵⁸ Veselovskii, *Issledovaniia po istorii klassa*, 141, 213.

⁵⁹ DDG, 13, no. 2.

⁶⁰ Cherepnin proposed that Khvost was *tysiatskii* before 1350, lost that position, then regained it later. L. V. Cherepnin, *Russkie feodal'nye arkhivy XIV–XV vekov*, 2 vols. (Moscow and Leningrad: Akademiia nauk SSSR, 1948), 1: 23.

⁶¹ Fennell, *Emergence*, 293.

This example provides a valuable object lesson for us all in the dangers of extrapolating from other historians' assertions without checking to see whether those assertions accurately represent the evidence. At times, we might succumb to this temptation, especially when the statements of another scholar seem to support our own views. But careful analysis of source evidence trumps any historian's picture of the past. One must conclude then that Halperin proffers no compelling evidence or argument that there were more than four boyars at any time before ca. 1376. And the sparseness of our evidence about boyars in the 14th century,⁶² as well as Halperin's overlooking some pertinent evidence that does exist, hardly lends credence to his claim there were fewer than four boyars at any particular time.

Clans, Tribes, and Chiefdoms

Halperin also claims that, if the Muscovites borrowed the council of *qarachi beys* as an institution, then the increase in the number of boyars in the late 14th century would indicate "a degree of ignorance" (243) of the institution they were supposedly borrowing. He points out that it was a big deal for the Crimean Khanate just to increase the number of *qarachi beys* from four to five. Apparently there was also a subsequent increase under Sahib Girey in the 16th century to six *qarachi beys*, or at least an attempt to do so.⁶³ One notes, then, that in the Crimean Khanate, as in Muscovy, there were forces pushing toward an increase in membership in the council. Besides, the Muscovite secular elite was not restricted by steppe considerations of four compass points or the cosmological significance of four sides. If anything, they tended to see the world in terms of a circle, which is why the number of witnesses to the 14th-century documents stands out as unusual. The state of Muscovy was also expanding rapidly during the course of the 14th century, incorporating new territory and peoples, whose elites were then represented on the council of state. So, instead of "a degree of ignorance," I see an intelligent adaptation on the part of the Muscovite ruling elite to fit the different circumstances it faced from those of a steppe khanate.

Although we do not have hard evidence of how the *qarachi beys* were selected, we can suppose that the heads of the four main kinship groups in the Qipchaq Khanate were members of the council. In Muscovy, this does not

⁶² Halperin finds his own "0" number for boyars between 1357 and 1366 to be "inconceivable" and the result of "a gap in the sources" (245, n. 32), but he seems ready to accept any other number equal to or greater than 1 as accurately representing the actual number of boyars in any given year (i.e., no gap in the sources).

⁶³ Alexandre Bennigsen, Pertev Naili Boratav, Dilek Desai, and Chantal Lemerrier-Quellejey, *Le Khanat de Crimée dans les Archives du Musée du Palais de Topkapı* (The Hague: Mouton, 1978), 11–12.

always seem to be the case, as we encounter outliers promoted to boyar rank who then raise their kinship group to prominent status. They are considered the founders of these clans. The principles of selection may not have exactly matched, but we can see a correlation between membership on the respective councils and prominence of the members' kinship group, either pre- or post-appointment. Nevertheless, the Muscovite state was like the Qipchaq Khanate in that its creation was a top-down phenomenon, for it was imposed on society from above. In contrast, steppe khanates were theoretically created from the bottom up, the result of mutual agreement to confederate under a khan.

Halperin goes on to argue that the Muscovites could not have borrowed the notion of a council of state including the heads of the strongest clans from the Qipchaq Khanate, because in that khanate the *qarachi beys* were the heads of tribes, not clans (*rod*). He suggests that the "only" way for the Muscovites to "have equated their own boyar clans with Mongol clans" would be for them to have been "grossly ignorant of steppe society – which they were not, or else they could not have borrowed any institutions from the Horde" (242). Halperin derives his assertion that the *qarachi beys* in the Qipchaq Khanate headed tribes, not clans, from the dissertation of Uli Schamiloglu completed at Columbia University in 1986. We do not have much evidence about the kinship groups that the *qarachi beys* of the Qipchaq Khanate headed, so Schamiloglu bases his conclusion on the view of Halil Inalcik regarding the Crimean Khanate,⁶⁴ and quite understandably extrapolates that conclusion to the Qipchaq Khanate. In contrast, other scholars who have studied the Crimean Khanate have preferred the term "clan" to describe the kinship group that each *qarachi bey* headed.⁶⁵ Scholars clearly differ on what the better word is to describe these kinship groups. Inalcik, in his article, at times even slips back into using the term "clans" to describe them (as does Halperin). None of these scholars explicitly defines how he or she is using either the term "tribe" or the term "clan," although to Halperin's credit he does provide an implicit definition of steppe clans as possessing a "social scope, totemic charisma, and political voluntarism" (242) that Muscovite clans lacked.

⁶⁴ Halil Inalcik, "The Khan and the Tribal Aristocracy: The Crimean Khanate Under Sahib Giray I," *Harvard Ukrainian Studies* 3/4 (1979–80), 445–66.

⁶⁵ V. E. Syroechkovskii, "Mukhammed-Girai i ego vassaly," *Uchenye zapiski Moskovskogo ordena Lenina gosudarstvennogo universiteta im. M. V. Lomonosova*, vol. 61: *Istoriia*, no. 2 (1940), 28–34; Beatrice Manz, "The Clans of the Crimean Khanate, 1466–1532," *Harvard Ukrainian Studies* 2: 3 (1978), 284, n. 8. Usmanov uses the phrase *rod-klan* to describe them. See his notation in Sigizmund Gerbershtein [Herberstein], *Zapiski o Moskovii* (Moscow: Izdatel'stvo Moskovskogo universiteta, 1988), 344, n. 676.

The avoidance of defining these terms is not unusual in the scholarship because of the difficulty involved in doing so. As long ago as 1942, the anthropologist John R. Swanton wrote: “there is no one universally valid principle identifying a body of people as a tribe...”⁶⁶ In 1975, an anthropologist at Columbia University, Morton H. Fried, challenged the use of the term “tribe” across the board. He argued that the notion of a tribe is an invention of 19th-century scholars to try to find discrete political units in pre-state societies. He dismisses the idea that tribes “are... closely bound populations in either territorial or demographic senses.”⁶⁷

The word “tribe” derives from the Latin *tribus* to represent three kinship groups of ancient Rome. The Latin word was adopted as *tribu* in Middle English in order to have a word to describe the Israelite groups known as *shebat* or *matteh*. Some time later the word “tribe” developed a negative connotation, being used mainly in referring to “primitive” or “uncivilized” people. Thus, Indians, Africans, pastoralists, Bedouin, etc., are grouped in tribes, whereas “advanced” or “civilized” people are grouped in extended families and clans. This is not, however, the way Inalcik and Schamiloglu intend their use of the term. Instead, they see a tribe as being more independent within a khanate than a clan is within a state. That is, for them, a khanate is a confederation of tribes, each of which reserves the right to end its allegiance to the khanate at any time. It would be rather difficult for clans within a state to attempt to do the same. Thus, their distinguishing of the term “tribe” from “clan” seems to be intended as more a matter of different external relationships to the overall polity than as a matter of differing internal structure.

Halperin takes their preference for “tribe” over “clan” to indicate a fundamentally different kinship unit, so different in fact that the Muscovites would not have seen any similarity between them and their own clans. Halperin’s implied understanding of “tribe” is a combination of clans. But Brian Fagen says we should see tribes as clusters of bands (defined as associations of families of not more than 25 to 60 people) that are linked by clans. Clans, in turn, he sees as much more a kin than a political unit.⁶⁸ Yet, the *Oxford English Dictionary*

⁶⁶ John R. Swanton, *The Evolution of Nations* (Washington, DC: Smithsonian Institute, 1942) (= Smithsonian Institution War Background Studies, no. 2), 3.

⁶⁷ Morton H. Fried, *The Notion of Tribe* (Menlo Park, CA: Cummings, 1975), Preface. He does acknowledge that contemporary Indian populations in the United States will call themselves “tribes,” but Fried sees this as a secondary formation, the result of an imposition of nomenclature by the United States government. In earlier centuries, however, these people would not have referred to themselves in this way.

⁶⁸ Brian M. Fagen, *People of the Earth: An Introduction to World Prehistory*, 7th ed. (New York: HarperCollins, 1992), 61–62.

tells us that the word “clan” was introduced into English in the 16th century with the meaning: “a number of persons claiming descent from a common ancestor and associated together; a tribe.”⁶⁹ The reason the OED equates clan with tribe is the origin of the term “clan” among the kinship groups of the Scottish Highlands, which existed outside of, or only loosely connected with, any state formation. Thus, Inalcik’s and Schamiloglu’s objection to “clan” as indicating subservience to a state structure is misplaced in regard to the original meaning of the term.

The Old Rus’ian dictionary of Barkhudarov tells us that the word *plemia* (which is usually translated as “tribe”) can also mean *rod* or *klan*.⁷⁰ In the *Povest’ vremennykh let*, the word *plemia* is normally used in the sense of a genealogical line of descent, as is the term *rod*.⁷¹ In 1820, a translation of the genealogy of the Shirins referred to them as *rod*.⁷² And in 1476, Eminek Bey, himself a Shirin, referred to his own kinship group as one of the *’ašira*, which can mean either “tribes” or “clans.”⁷³ So, if the Muscovites were confused about the difference between a “tribe” and a “clan,” it is understandable.

The terminology issues aside, however, Halperin’s basic question remains. Were the kinship units that the *qarachi beys* of the Qipchaq Khanate headed significantly different from the kinship units that Muscovite boyars headed? In respect to size in the 14th century they probably were. The Qipchaq Khanate was what Geoffrey Parker would call a “core area.”⁷⁴ It controlled a huge amount of territory that included the entire western steppe region plus extensive portions of the forest zone to the north and west. The Muscovite princes at the beginning of the 14th century, in contrast, governed an area little larger than that encompassed by the present-day Moscow city limits. Muscovy did expand during the course of that century but still remained small in comparison with the areas and population under the administration of the Qipchaq Khanate. No

⁶⁹ *The Compact Edition of the Oxford English Dictionary*, 2 vols. (Oxford: Oxford University Press, 1971), 1: 424.

⁷⁰ *Slovar’ russkogo iazyka XI–XVII vv.*, ed. S. G. Barkhudarov, 23 vols. (Moscow: Nauka, 1975–), 15: 83.

⁷¹ *Povest’ vremennykh let: An Interlinear Collation*, ed. Donald Ostrowski (Cambridge, MA: Ukrainian Research Institute, Harvard University, 2000), 4,16; 5,22; 7,20; 12,10; 20,20; 93,8; 93,28; 96,26; 97,12; 183,10; 183,12; 234,16; 235,23; 242,13.

⁷² F. F. Lashkov, “Sbornik dokumentov po istorii krymsko-tatarskogo zemlevladiiia,” *Izvestiia Tavricheskoi uchenoi arkhivnoi kommissii* 23 (1895), 123, no. 55.

⁷³ Bennigsen et al., *Le khanat*, 61–62. For the definition of *’aširet* (pl. *’ašira*), see James W. Redhouse, *A Turkish and English Lexicon: Shewing in English the Significations of the Turkish Terms* (Constantinople: Printed for the American Mission by A. H. Boyajian, 1890), 1302: “A tribe, clan; especially a nomadic tribe.” My thanks to Brian Boeck for this citation.

⁷⁴ Geoffrey Parker, *The Geopolitics of Domination* (London: Routledge, 1988), 66–67.

doubt at the time each *qarachi bey* alone could have fielded an army larger than that of the Muscovite grand prince.⁷⁵ But we do not know how large in number the immediate kinship group of each *qarachi* was. Steppe clans, on the whole though, were usually rather modest in size, rarely exceeding 100 members. It does appear to be the case, in addition, that steppe kinship formations were more flexible in terms of adding and losing members than Muscovite kinship formations. While both could and did use fictive genealogies to incorporate people who previously had not been considered a member, steppe groups could do so with more alacrity.⁷⁶

The evidence we have shows the political activities of the Shirins, Argins, Barins, Qipchaqs, and, later, the Mangits and Sedjeuts in the Crimean Khanate were similar to political activities of Muscovite clans – with only a few members at any one time being prominent.⁷⁷ From the evidence alone, we would have to conclude that the immediate kinship group of each *qarachi* in the Crimean Khanate (and by extrapolation in the Qipchaq Khanate) was no larger than any of the major clans in Middle and Late Muscovy. But *qarachis* had in allegiance large numbers of people who were not related to them, or related only distantly, and whose social status was below that of the dominant clan.

Given the existence of levels of stratification of those in allegiance to each *qarachi bey*, it seems that what we are discussing here is neither clan nor tribe, both of which are socially egalitarian. Instead, we can draw on the idea of the Harvard archaeologist Lamberg-Karlovsky who has proposed that khanates are pre-state formations intermediate between chiefdoms and states, and made up of chiefdoms.⁷⁸ Although I prefer to see khanates, not as pre-state formations but as a type of state formation different from the Eurocentric conception of “state,” I do think we can accept his formulation concerning the composition of khanates. Each of these chiefdoms, then, is headed by a dominant clan. Under that dominant clan are various groups that owe allegiance to it arranged in a

⁷⁵ In the early 16th century, the Shirin *qarachi* Agysh claimed he could raise an army of 20,000. SRIO, 95: 649–50.

⁷⁶ In this sense, steppe kinship groups were similar to those of other kinship groups that have been referred to as “tribes” (with the exception of 20th-century American Indian tribes). Until the 19th century, for example, people of European descent, like Mary Jemison who became a Seneca or James Smith who became a Caughnawaga, were easily accepted as full members of Indian nations. James E. Seaver, *A Narrative of the Life of Mrs. Mary Jemison* (Canandaigua, NY: J. D. Bemis, 1824); James Smith, “Prisoner of the Caughnawagas,” in *Captured by the Indians: 15 First-hand Accounts, 1750–1850*, ed. Frederick Drimmer (New York: Dover, 1985), 25–60. By the 20th century, this could no longer occur.

⁷⁷ Manz, “Clans,” 284–85.

⁷⁸ C. C. Lamberg-Karlovsky, “Bronze Age *Khanates* of Central Asia,” *Antiquity* 68: 259 (1994), 398–405.

descending social hierarchy.⁷⁹ At least four of these chiefdoms are required for there to be a khan and thus for there to be a khanate. Such an arrangement accounts for the semi-independent status of the *qarachi beys*.

There were differences between the leading kinship groups in the khanates, on one side, and the leading kinship groups in Muscovy, on the other. But there were also similarities between the two, such as lineal descent from a common ancestor and political status. Both Muscovite and steppe khanate lineages were made up of what anthropologists call ramages, or conical clans, in which each member is ranked within a pyramid in regard to the directness of their descent from the real or fictive founder.⁸⁰ Are these similarities enough to outweigh the differences in terms of borrowing? Fortunately, I do not have to answer the question here because I am not arguing that the Muscovites borrowed the concept of clans (or tribes or chiefdoms) from the Qipchaq Khanate but only that they borrowed and adapted the idea that the heads of their society's leading kinship formations, or ramages, should form a council that advised and sanctioned certain actions of the ruler taken in behalf of the polity.

The functions of the steppe council of state are what I am saying the Muscovites borrowed and adapted in their council of state, not kinship groups as such. In particular, the approval of the *qarachi beys* was required for all significant enterprises and the signatures of its members were required on all important documents concerning matters of internal policy in the Qipchaq and Crimean khanates.⁸¹ Agreements with foreign powers required witnessing by the *qarachi beys* and often by additional important personages as well, including brothers and sons of the khan, religious leaders, close advisers, and sometimes prestigious clan members other than the *qarachi beys*.⁸² In addition, all meetings

⁷⁹ The anthropological literature on the polity called a "chiefdom" is extensive. One can begin with Elman R. Service, *Primitive Social Organization: An Evolutionary Perspective* (New York: Random House, 1962), 143–77 and idem, *Origins of the State and Civilization* (New York: W. W. Norton, 1975), 15–16, 151–52. But see also Fred O. Gearing, *Priests and Warriors: Social Structures for Cherokee Politics in the 18th Century* ([Menasha, WI]: American Anthropological Society, vol. 93, 1962), and Jared Diamond, *Guns, Germs, and Steel: The Fate of Human Societies* (New York: W. W. Norton, 1997), 273–76.

⁸⁰ For this definition, see Norman Yoffee, "Too Many Chiefs? (or, Safe Texts for the '90s)," in *Archaeological Theory: Who Sets the Agenda?* ed. Norman Yoffee and Andrew Sherratt (Cambridge: Cambridge University Press, 1993), 61.

⁸¹ M. G. Safargaliev, *Raspad Zolotoi Ordy* (Saransk: Mordovskoe knizhnoe izdatel'stvo, 1960) (= *Uchenye zapiski Mordovskogo gosudarstvennogo universiteta*, vol. 11), 68–69; Manz, "Clans," 282.

⁸² SRIO, 95: 20, 211; *Stosunki z Mendli-Girejem, Chanem Tatarów Perekopskich (1469–1515)*, ed. Kazimierz Pułaski (Cracow: G. Gebethner i spółka, 1881) (= *Stosunki Polski z Tatarszyzną od połowy XV wieku*, vol. 1), no. 91, p. 315; Syroechkovskii, "Mukhammed-Girai," 40; and Manz, "Clans," 286.

with foreign ambassadors required the presence of representatives of the major clans.⁸³ These same three functions attended the Muscovite Boyar Council as well, and they go beyond what we find in state councils of other governments of the time. In short, I submit that this correspondence in functions of the Boyar Council and the divan of the *qarachi beys* is more than mere coincidence, and can best be explained by a conscious adaptation by the Muscovite ruling elite of the steppe institution.

Those who want to argue that the grand prince ruled without the official approval of “the boyars” can do so only by continuing to dismiss all the evidence to the contrary. A council made up of members of the secular elite had a decision-making role in important matters concerning foreign policy and domestic legislation, as well as, after 1375, posthumous distribution of the ruler’s wealth. No matter how one wants to look at it, this was a powerful council of state. Then the question is: how did the Muscovites get the idea for such a council? We do not find any comparable institution in pre-Mongol Rus’.⁸⁴ The Muscovites could have thought of it all by themselves, and I certainly do not want to give the impression that I am arguing that they were incapable of such an innovation on their own. But neither were they so insular as not to be able to adopt a good thing when they saw it. When we find an institution with the same functions in the steppe khanates, when we remember that the Muscovite grand princes in the 14th century were vassals of a steppe khan, and when we realize that the grand princes were making frequent trips to Sarai, the capital of that khanate, at the same time the Muscovite institutional structure was being set up, then we have to give serious consideration to the idea that the same kind of borrowing and adaptation was occurring in regard to the Boyar Council as the sources testify to in other areas.

Methodological Issues

In the remainder of his article, Halperin deconstructs the organization chart that I drew up to show the correspondences between the Muscovite governmental structure and that of the Qipchaq Khanate. The basis for each of Halperin’s point-by-point objections can best be summed up in his statement concerning *namestniki* and *volosteli*: “we cannot compensate for the gaps in our understanding of their activities by invoking Tatar analogues” (253). Here is the crux of our disagreement. The East Slavic sources, at best, provide tantalizing tidbits of information about what a particular official does. Sometimes they provide only

⁸³ SRIO, 41: 40; 95: 39–40, 81, 172, 185, 252, 280, 330, 357, 371, 500. See also Syroechkovskii, “Mukhammed-Girai,” 40; and Manz, “Clans,” 296.

⁸⁴ Here I agree with Sergeevich’s rejection of Kliuchevskii’s claim that Boyar Council-like institutions existed in pre-Muscovite Rus’. Sergeevich, *Drevnosti russkogo prava*, 2: 465–75.

the title of a position and nothing more. I attempted to look at these administrative positions within the context of the Mongol Empire to see whether doing so could increase our understanding of them. I was thereby invoking a principle of uniformity – the functioning of these positions in one part of the Mongol Empire may be applicable to explaining how those positions functioned in another part. Since we already have examples of borrowing and adaptation from the Qipchaq Khanate in terms of military matters, administrative practices, and political institutions and concepts (as described by Halperin in *Russia and the Golden Horde*), it seemed a logical next step to find out to what extent the principle of uniformity could help explain other possible examples of borrowing and adaptation. For Halperin, this attempt to supplement the East Slavic sources by looking at non-East Slavic sources is futile, for he seems to be saying that he will not accept anything that is not already explicitly stated in the East Slavic sources.

My attempt to fill in the gaps in the East Slavic sources is thus rejected by Halperin on the basis that the East Slavic sources do not provide positive confirmation of what I am trying to fill in those gaps with. But then we would not expect them to provide confirmation of what is in their own gaps. For example, he argues that “[t]he Muscovite *dvorskii* lacked the status or expertise attached to the vizier” of steppe khanates because the East Slavic sources provide no evidence that he did (249). The *baskaki* did not have the military function they did elsewhere in the Mongol Empire because the East Slavic sources do not tell us they did. And the *volosteli* and *namestniki* do not represent local equivalents of the “military” and “civilian” governors respectively because the East Slavic sources do not distinguish them this way. On the other hand, these same sources do not explicitly deny that the *dvorskii* had sufficient administrative expertise or that the *baskaki* had a military role or that the *volosteli* and *namestniki* had functional differences. In short, the Rus’ sources are frustratingly silent about the particular duties of and skills required for these positions.

When we encounter such a lack of information in our direct sources, then we are justified in looking for indirect sources of evidence. Halperin seems to be arguing that we are not justified in doing so. This seems a little odd since Halperin resorted to this same method of extrapolation from indirect evidence on occasion in his *Russia and the Golden Horde*. For example, in his book, he cites John of Plano Carpini (a decidedly non-East Slavic source) for evidence that “the *baskak* oversaw the collection of tribute, conscripted troops, and maintained order, that is suppressed opposition to Mongol rule.”⁸⁵ In his article, in contrast, Halperin writes: “While a *basqaq* might accompany a military

⁸⁵ Halperin, *Russia and the Golden Horde*, 33.

campaign, . . . there are no unambiguous references to regiments or even major garrisons under *basqaq* authority” (251) and concludes thereby that “[t]here seems to be nothing specifically ‘military’ about the *basqaqi*” (252). One may be allowed to wonder how a *basqaq* could suppress opposition, maintain order, and conscript troops without having at least some troops under his command.

In an attempt to refute my understanding of a *basqaq* as a military governor sent in to pacify an area that had been conquered or was in revolt, Halperin writes: “The Mongols were hardly so inept as to assign military governors to cities and regions without assigning sufficient military resources to sustain Mongol rule” (251). Exactly! Since we know the khans placed *basqaqs* in Rus’ territory, whether or not one accepts they were military governors, the khans would certainly not have been so inept as to send them there without military support. And such support would be even more vital in Rus’ territory, which was one that was not particularly docile during this period of Mongol hegemony. Halperin seems to be suggesting that the *basqaqs* were without military support in potentially hostile terrain and that they would just call in troops from the steppe whenever a disturbance broke out, but such an arrangement hardly seems likely. To be sure, handling major revolts required calling in reinforcements from the steppe, but peacekeeping requires troops on site, as the recent experiences of the United Nations testify. A yet closer analogy might be the British Raj, which depended on a substantial commitment of troops in India, many of whom were homegrown.

There are certain other problems with Halperin’s objections. He argues that the *volost’* was a territorial unit and therefore could not be a military unit, while at the same time accepting the *tumen* as both a territorial and a military unit (252, n. 60). In order to argue that the *basqaq* had no military function, he points out that there were no *basqaqs* in the decimal command system (252). But the decimal command system he cites was for the regular Mongol army, which did not include *basqaqs* precisely because they commanded special occupation forces. He asserts there is no similarity between the *basqaq* and *volostel’*, on one hand, and the *daruga* and *namestnik*, on the other. Yet, he points out that both the *basqaq* and *volostel’* tended to reside in rural areas while the *daruga* and *namestnik* tended to reside in towns (253). These disagreements between us indicate decidedly different conceptions of how to use our sources.

Focusing for the moment just on the East Slavic sources, we can classify their use into four categories depending upon whether something is reported and whether that something is possible. The first category we can define as those things that are reported in the sources but are impossible, what Lur’e calls “‘a definite contradiction’ between the data of a source and the laws of logic or

science. . . .”⁸⁶ Above (279, n. 43), I mentioned that some chronicles report two deaths for D. A. Monastyrev, one at the Vozha River in 1378, the other at Kulikovo in 1380. Clearly two deaths for a single individual are impossible, yet that is what these East Slavic sources tell us. A second category is made up of those things that are not reported in the sources and are not possible. Although the chronicles report the grand princes made many trips to Sarai in the 14th century, they do not tell us by what means they travelled there. We can, nonetheless, say that they did not travel by train or plane because such means of conveyance had not been invented yet. A third category comprises those things that are reported in the sources and are possible. Here we can further subdivide this category into those things that are likely, those that are unlikely, and those with neutral likelihood (where we suspend our judgment, at least temporarily). For example, the chronicles report that Metropolitan Aleksei traveled to Sarai in 1357.⁸⁷ That is both possible and likely. Furthermore the chronicle account states Taydula, the wife of Khan Janibeg, invited him to come visit her because she was ill. This also is possible, but here we can suspend judgment about the likelihood she was seeking a cure from the metropolitan of Rus’, especially since we have no evidence that he was known for effecting cures. Finally, the chronicle account tells us he cured her. This seems unlikely to us since Aleksei was not known to be a doctor of medicine. Taydula may have recovered but not due to any action on his part.⁸⁸ In any case, we can thus rank the information in the reported-and-possible category in this way.

Likewise, within a fourth category, the unreported and possible, we can rank the information similarly. As I mentioned above, the East Slavic sources do not tell us how the grand princes (or metropolitans) traveled to Sarai. The easiest means would be to go down the Volga by boat. So in warm weather, that is the likely means of travel. However, it is still possible and not unlikely they would travel by horse. In the winter, however, when the rivers freeze, horses and sleighs are the likely means of travel. But in any season, it is unlikely they walked. Archaeologists make extensive use of this type of reasoning in interpreting the available evidence in their discipline, where there is rarely direct confirmation in written sources.

⁸⁶ J. Luria [Ia. S. Lur’e], “Problems of Source Criticism (with Reference to Medieval Russian Documents),” *Slavic Review* 27: 1 (1968), 10.

⁸⁷ TL, 375–76; PSRL, 8: 10; 10: 229; and 15, pt. 1 (1922): col. 66; 28: 74, 235.

⁸⁸ Of course, a “Christian historian” might not consider any of this as unlikely. See the brilliant essay of Georges Florovsky, “The Predicament of the Christian Historian,” in *Religion and Culture: Essays in Honor of Paul Tillich*, ed. Walter Leibrecht (New York: Harper & Brothers, 1959), 140–66.

As this little excursus should make clear, at times we have to maintain a distinction between what we can say did or did not happen and what we must limit ourselves to saying only that the sources do not report. A case in point is Halperin's assertion that the Muscovite grand princes did not conduct censuses, as the Qipchaq khans did. He bases this assertion on the absence in the sources of any statement that the grand princes conducted a census or of any extant results from such a census. Yet, it has long been held that the later fiscal surveys, such as *sokha* registers (*soshnye pis'ma*), cadastral books (*pistsovye knigi*), and census books (*perepisnye knigi*) developed out of the khans' censuses and the grand princes' adaptation of them to gather the tribute.⁸⁹ So the question whether the grand princes conducted censuses in the 14th century falls into the category of not reported in the sources but possible. Whether we conclude such an operation was likely or unlikely or we merely suspend judgment, we are justified only in saying the sources do not report it, not that it did not happen. And studying the relevant non-East Slavic sources can provide us a better idea of what is possible in Rus' territory in the 14th century, especially concerning matters about which the East Slavic sources provide no information. Then we can discuss issues of likelihood, such as whether the *dvorskii* possessed comparable "status or expertise," or the *baskaki* had a military function, or the *volosteli* and *namestniki* were assigned different but overlapping duties.

In the conclusion of his article, Halperin raises the issue of the continuance of steppe influence through to the 16th century:

if Muscovy's secular court in the 14th century was not universally modeled on the Tatars, then perhaps it was also not as "Tatar" in the 16th century; the quantity of Mongol borrowing might not have reached the point that it had a qualitative effect upon the self-conception of the Muscovite court and elite. Thus we need to reevaluate Ostrowski's views of the conflict between the Byzantine Church and the "Tatar" Court during the reign of Ivan IV, the likelihood that the *oprichnina* was created as a Tatar state, and the Simeon Bekbulatovich episode. Recognition of the Mongol factor in 16th-century Muscovy should not be confused with exaggeration of Tatar influence (255).

Leaving aside for the moment the unclear distinction Halperin is making between "Mongol" and "Tatar," we look to *Russia and the Golden Horde* for

⁸⁹ See E. N. Kolotinskaia, *Pravovye osnovy zemel'nogo kadastra v Rossii* (Moscow: Izdatel'stvo Moskovskogo universiteta, 1968), 29; H. L. Eaton, "Early Russian Censuses and the Population of Muscovy, 1550–1650" (Ph.D. Dissertation, University of Illinois, 1970), 12, 15. In contrast, Halperin asserts that "Muscovite cadastres and censuses . . . show nothing in common with Mongol census practice." Halperin, *Russia and the Golden Horde*, 94.

Halperin's understanding of the exact substance of this 16th-century influence on Muscovy:

Russian consciousness of the Chingisid principle is of interest because after the overthrow of the Great Horde in 1480, Moscow's grand princes, despite the thoroughly Byzantine cast of Muscovite ideology, played upon their status as the conquerors, and hence successors, of the Horde khans. . . .⁹⁰

Thus, according to Halperin, it was the Mongol Chingizid principle that the grand princes and later tsars used to advance their imperialist goals. Halperin goes on to suggest that "the system of icons and frescos in the Archangel cathedral of the Kremlin was inspired by the concept of the Chingisid clan," as was Muscovite coinage of the 15th century. He points out the practical importance in invoking the Chingizid principle for the Muscovite rulers to collect the *iasak* from peoples formerly under the domination of the Qipchaq Khanate. In this respect, he continues, it was important "for Muscovy to foster its image as the successor state to the Golden Horde and to remain sensitive to steppe traditions of rule. . . ." Then Halperin refers to the abdication of Ivan IV "in favor of a Chingisid, Symeon Bekbulatovich. . . ." as part of this tradition of "assum[ing] the mantle of the khans."⁹¹ Apparently, then, he is arguing that Muscovy's view of itself as successor to the Qipchaq Khanate did not have a "qualitative effect upon the self-conception of the Muscovite court and elite." It is difficult to comprehend, however, why it would not have exactly that effect. Nor does Halperin allow for the possibility that the Church may not have been completely in accord with Muscovy's view of itself as the successor to a "godless" khanate, despite the fact that we have plenty of evidence in the East Slavic sources after 1448 of the Church's anti-Tatar views.

The opposition between "Mongol" and "Tatar" that Halperin alludes to in the conclusion of his article is a false one in regard to the principles, institutions, positions, and practices we are discussing here. That is, he seems to be suggesting that Muscovy borrowed "Mongol" things but not "Tatar" things. The term "Tatar," as we know, has two meanings – first for a people of the eastern steppe who were traditional enemies of the Mongols; and second for the Turkic-speaking western steppe peoples in general. The first sense of the term does not concern us here, but the second sense is crucial for understanding the nature of the cross-cultural influence operating on Muscovy from the steppe.

⁹⁰ Halperin, *Russia and the Golden Horde*, 100.

⁹¹ Halperin, *Russia and the Golden Horde*, 101.

Table 2: Steppe Influence on Muscovy**First Tatar Influence (14th century)**

<i>Borrowing</i>	<i>Origin</i>	<i>Conduit</i>	<i>Means of Transfer to Muscovy</i>
weaponry	steppe pastoralists	QKh	direct contact with Tatars
military strategy, tactics, formations	Mongols	QKh	direct contact with Mongol-led armies
dual system of administration	China	Mongols and QKh	Muscovite grand princes' trips to Sarai
council of state	steppe pastoralists	QKh	Muscovite grand princes' trips to Sarai
tax system (including <i>kormlenie</i>)	<i>Dar al-Islam</i>	QKh	Muscovite grand princes' trips to Sarai
administrative structure	steppe pastoralists	QKh	Muscovite grand princes' trips to Sarai
<i>iam</i> (post system)	China	Mongols and QKh	imposed by the khans
shin beating (punishment)	China	Mongols and QKh	imposed by the khans
<i>chelom bit'e</i> (petition)	China	Mongols and QKh	Rus' princes' trips to Sarai
lateral system of succession	steppe pastoralists	Kiev	ruling class
clan ranks within polity	steppe pastoralists	QKh	Muscovite grand princes' trips to Sarai
commercial and financial terms	Turkic languages	QKh	merchants and Rus' princes' trips to Sarai

Second Tatar Influence (late 15th and 16th centuries)

Chingizid principle	Mongols	KKh	Turkicized Juchids
<i>pomest'e</i>	<i>Dar al-Islam</i>	QKh	refugee Tatars
certain record-keeping methods (such as scrolls)	Uighurs	QKh	refugee Tatars
<i>beshestie</i> (dishonor)	"Courage cultures"	KKh	Turkicized Juchids and refugee Tatars
<i>zemskii sobor</i>	steppe pastoralists	KKh	Turkicized Juchids

What I see is a complex process in which steppe influences on Muscovy had various origins but where most early ones were funneled through the conduit of the Qipchaq Khanate (QKh) and a number of later ones through the Kazan' Khanate (KKh). In Table 2, I present my understanding of those influences, their origins, and means of transfer to Muscovy.

As the table indicates, I see two waves of Tatar influence on Muscovy, one during the 14th century, the other during the second half of the 15th and into the 16th century. The first wave of influence is mainly, but not only, the result of the Muscovite princes' bringing back ideas from Sarai concerning how to set up their own administration. The second wave is mainly the result of Tatar émigrés from the various steppe khanates of the time coming over into the service of the Muscovite grand prince. This table helps put into context what Halperin sees as the "Mongol" influence of the Chingizid principle. And it helps clarify the "influence through delayed action" explanation of Vernadsky as well as the "remote control" and "institutional time bomb" metaphors of Wittfogel that Yanov pilloried.⁹² Halperin accepted the notion, as did Vernadsky and Wittfogel, that the main "Mongol" influence came in the late 15th and 16th centuries.⁹³ But this was at a time when there really were no more Mongols left in the western steppe. The descendants of all the relatively few Mongols who had remained there in the 14th century had for some time been Turkicized. In other words, they were Tatars. The "Mongol" Chingizid principle was by this time a "Tatar" Chingizid principle, not only in terms of who was transmitting it but also in how it was understood. It was not some disembodied concept that floated freely across the thousands of miles of steppe and across centuries directly from the Mongols of the 13th century to the Muscovite grand princes of the late 15th and 16th centuries. It was carried by individuals who modified it to suit their own needs. Once we get down to specifics of how political and administrative influences occurred, then Halperin's attempted distinction between "Mongol" and "Tatar" in the 15th and 16th centuries in the western steppe area falls apart.

At the point we acknowledge that Muscovy was influenced by the steppe in the 14th century, then we are obliged to accept the applicability of the principle of uniformity in discussing Early Muscovy as a part of the Mongol Empire. This means we can extrapolate from non-East Slavic sources in order to analyze Muscovite governmental and administrative positions because that is the same method by which our conclusions about the other borrowings, the ones we already agree upon, were reached. For example, there is no East Slavic source

⁹² Alexander Yanov, *The Origins of Autocracy: Ivan the Terrible in Russian History* (Berkeley: University of California Press, 1981), 100–01.

⁹³ Halperin, *Russia and the Golden Horde*, 95.

that explicitly states the Muscovites wielded lassos or composite bows in the same way the Mongols did, or that the Muscovites derived the practice of record keeping on scrolls from Mongol/Tatar chanceries, or that the Muscovites employed the same strategies and tactics as the Mongol army, or that the Muscovite rulers were adopting the Chingizid principle or assuming the mantle of the khans. We have to extrapolate these conclusions from the available evidence. This means supplementing the East Slavic sources with evidence from non-East Slavic sources, in particular those that tell us about the Mongols and Tatars. For Halperin to question that method is for him to undermine the conclusions he reached in *Russia and the Golden Horde*, in effect pulling the ladder out from under himself.

This is not to say there is no room for disagreement on the conclusions after we agree on the validity of the method. But to deny that method altogether, that is to reject the idea that Arabic, Chinese, Mongol, Persian, and Tatar sources can supplement East Slavic sources, will lead no further than the position of a Solov'ev – that there was no Mongol or Tatar influence on Muscovy. Why? Because, of course, one won't find evidence of such influence when one doesn't know what influence to look for. And to employ that method only partially and inconsistently, that is to use it in order to support conclusions one wants to reach while denying the method's legitimacy so as to reject conclusions one does not want to accept, seems to me a rather strange way to do history. Once we are agreed on the validity of the method and apply it consistently, not just for influences that we already accept but also for those under discussion, then we can have a meaningful dialogue on whether Muscovite borrowing and adaptation of steppe political and administrative institutions was as pervasive as I have argued it was.

Instead of trying to pull the ladder out from under Halperin's work, I see myself as building on the foundation that he and others have labored so hard to lay. Although we may disagree on the details, I think we can agree that an accurate assessment of steppe influence is essential before we can begin to fully comprehend Muscovite historical development and to place it properly within the context of world history.

ADDENDUM

What's Careful Reading and What's Not A Reply to Goldfrank's Objections

In his critique, David Goldfrank writes some very nice things about *Muscovy and the Mongols*. If I had any sense, I would do well to leave it at that and be grateful for the complimentary remarks. But then if I had any sense, I would not have “stuck out [my] neck” and written a book that I knew was sure to raise the ire of many people. So, in the interest of trying to advance discussion of the issues, I offer this addendum. My justification for doing so is twofold: it touches on subjects that did not come up in the longer response and it affirms the importance of close reading.

I appreciate that Goldfrank places his discussion of economic recovery in Rus' in terms of his making a “guess” (262). As a result of the almost complete absence of quantifiable data, all of our generalizations about the economy during this period must remain at the level of guesswork. Some guesses, though, are more informed than others in terms of trying to account for all the available evidence. This is why Miller's counting of reports about masonry constructions is so valuable. It is virtually the only quantified data we have that reaches from the 10th to the 15th centuries. On the other hand, it is only one indicator of relative prosperity. I agree with Goldfrank that we should place “the real recovery in Northeastern Rus' . . . somewhat later than the 1280s” (262), and I said as much in my book: “insofar as our evidence for masonry building correlates with economic prosperity, by the latter half of the fourteenth century, . . . northern Rus' alone may have been more prosperous than the whole of Rus' at any time before the Mongol hegemony was established” (*M&M*, 130). Furthermore: “by the second half of the fourteenth century, northeastern Rus' was more prosperous than it had ever been before” (*M&M*, 131). Far from asserting that “Northeastern Rus' . . . recovered from the Mongol invasions by the 1280s” (as Goldfrank writes on page 260), I stated that we can trace the beginning of the recovery in northern Rus' to the 1280s when “[w]e have evidence . . . of a few masonry constructions,” but that the 25-year period 1313–37 is when we begin to see significant numbers in northeastern Rus' (*M&M*, 130). Nonetheless, I am glad Goldfrank is willing to consider the possibility there might have been economic recovery in Rus' sometime during the period of steppe hegemony.

Goldfrank's other objections can be divided into three types: (1) those where he interprets a source; (2) those where he relies on “reasoning” in analyzing a source; and (3) those where he neither cites a source nor relies on any reasoning (as far as I can tell). The few cases where Goldfrank does cite some evidence, however, does not bode well for his unsupported assertions if he reached

them on a similar basis. Case in point: Goldfrank, in questioning my statement that “secular government sources present actions taken within a Mongol frame of reference,” claims “similarity between . . . two key legal documents, *Ruskaia Pravda* and the 1497 *Sudebnik*, neither of which points to any Mongol frame of reference” (263). As evidence of that “similarity” he cites the following passages from the two texts:

Ruskaia pravda

The Pravda established for *Rus'* when Iziaslav, Vsevolod, Sviatoslav, Kosniachko, Pereneg, Mikyfor the Kievan, and Mikula Chiudin convened.

Sudebnik of 1497

In the year 1497, in the month of December, Grand Prince Ivan Vasilevich of all *Rus'* with his children and boyars, laid down this judicial code for how boyars and okol'niche shall administer justice.

For ease of comparison I have italicized all the substantive words these two passages have in common, of which we find exactly one – the noun “*Rus'*.” Beyond that, there is no similarity at all between these passages in grammar, structure, or meaning. Indeed, the presence of the word “boyars” in the *Sudebnik* and not in the *Pravda* helps support the argument I made in my response to Halperin about the importance of the role of the boyars in Muscovy in contrast to Kievan *Rus'*. As for the absence of “a Mongol frame of reference” in the heading to the *Sudebnik* of 1497, I believe I covered the matter in *Muscovy and the Mongols* while discussing the Byzantine-based origin of Muscovite document formulas:

What is significant from the viewpoint of diplomatics is that none of these documents makes reference to the Khan in Sarai as the source of the grand prince's authority, nor do they follow the formulas of the Tatar *iarlyki*. This absence of reference would seem to indicate that the individuals who created the formulas for these documents were Byzantine trained, because they followed the Byzantine diplomatic formulas (*M&M*, 95).

Those who established the formulas for government documents were clearly doing so from Byzantine antecedents. In later modifications of those formulas, we can and do find evidence for a different orientation of those in the secular administration.

Another case where Goldfrank refers to evidence is the Church's use of *iarlyki* of the Tatar khans. Goldfrank suggests that this “show[s] how potent one of the central aspects of the Mongol legacy remained within the citadel of Muscovite Byzantinism” (262). But this shows no such thing. The Church used these documents not for any internal Church matters but for defending itself

from state encroachment, in particular in regard to taxation and confiscation of monastic lands. What it does show is that the Church realized “how potent . . . the Mongol legacy remained” within the secular administration. After all, the state continued to send tribute to Tatar khans until the 18th century. If the Church was going to protect itself from having to pay taxes and from having its lands confiscated, then it needed to invoke an authority on these matters that carried some weight with the secular administration.

Where Goldfrank attempts to rely on reasoning in analyzing a source, the results are just as questionable. He states that Gol'dberg and I “may be going too far in arguing that the original Third Rome formulation had *romeiskoe tsarstvo*, when it so obviously is *rossiiskoe* or something like it. Muscovite authors certainly understood what a logical sequence was” (264). Goldfrank adds in a footnote: “Rome, Constantinople, Rome makes no sense whatsoever” (n. 23). Perhaps not, but the progression Rome, Constantinople, Holy Roman Empire does make sense. And it is clear Filofei is referring to the Empire of Charles V by *romeiskoe tsarstvo* because when he refers to ancient Rome, he uses the adjectival form *rimskii*, not *romeiskii*.⁹⁴ Thus, the change by Muscovite copyists of *romeiskoe* to *rossiiskoe* becomes understandable whereas the change from *rossiiskoe* to *romeiskoe* by such copyists would be rather unlikely, by Goldfrank's own admission. In other words, the less likely reading has to be primary in order to explain the change in other copies to the more likely reading. This conclusion follows from the principle of textual criticism that the rougher reading should be accepted as primary over a smoother reading, because the probability is that a copyist is more likely to try to smooth out a rough reading than to roughen up an already smooth reading.⁹⁵ To put it another way, if we accept *rossiiskoe* as the primary reading then we have no explanation for the appearance of *romeiskoe* in the copies that are closer to the archetype. But if we accept *romeiskoe* as the primary reading, then we have a ready explanation for why *rossiiskoe* shows up in the copies that are further away from the archetype. In the end, Goldfrank's proposal of *rossiiskoe tsarstvo* as the primary reading makes even less sense than the “Rome, Constantinople, Rome” sequence he objects to.

⁹⁴ *Pamiatniki literatury drevnei Rusi. Konets XV–pervaia polovina XVI veka*, ed. L. A. Dmitrieva and D. S. Likhachev (Moscow: Khudozhestvennaia literatura, 1984), 442–54. For a more recent publication of the text of this letter with variants, see N. V. Sinitsyna, *Tretii Rim. Istoki i evolutsiia russkoi srednevekovoi kontseptsii (XV–XVI vv.)* (Moscow: Indrik, 1998), 339–46. Sinitsyna also accepts *romeiskoe tsarstvo* as the primary reading but for reasons different from the ones I explain here. *Ibid.*, 235–46. For Paul Bushkovitch's review of Sinitsyna's book in which he discusses her interpretation of this passage, see this issue, 391–99 (esp. 393–95).

⁹⁵ A description of this principle, known as *difficior lectio probior*, can be found in D. S. Likhachev, *Tekstologiya. Na materiale russkoi literatury X–XVII vv.*, 1st ed. (Moscow and Leningrad: Akademiia nauk SSSR, 1962), 170–73; 2nd ed. (Leningrad: Nauka, 1983), 185–88.

Without citation of evidence or resort to logical argument, Goldfrank makes a number of other assertions that seem to be based on a series of misreadings. He states that I “readily conclude[] that we have no way of knowing if defunct Byzantine practices or current Islamic ones influenced Muscovite seclusion of elite women” (264). Yet, what I wrote was: “Even if we wanted to designate the Tatars as the source of veiling in Muscovy on the basis of concomitance of practice, we could not designate them as the source of seclusion according to any of our criteria for establishing influence” (*M&M*, 67). Furthermore, I suggested that “the [Qipchaq] Khanate may have acted as a barrier to the spread of this practice [veiling, but I could also have included seclusion] to the north from Muslim territories to the south” (*M&M*, 67). In other words, I was unequivocal in arguing that veiling and seclusion of women in Islamic lands on the other side of the Tatar khanates had no discernible influence on the Muscovite practice. My uncertainty is only in regard to exactly how the defunct Byzantine practice entered Muscovy in the late 15th or early 16th centuries (see *M&M*, 82–84). The most likely conduit is the Byzantine-Rus’ book culture, but I was unable to find a specific work or works that could have served as the basis.

Goldfrank claims that I “miss the heart of the issue” of despotism. He writes that my definition of despotism “is more akin to arbitrary and centralized tyranny than to what the key theoreticians of despotism, starting with Aristotle, Bodin, and Montesquieu, had in mind” (264). Goldfrank does not tell us what he thinks these “key theoreticians . . . had in mind” when they used the term *despotism*, leaving it to the reader to conclude that whatever it was it must be different from what I described. Then he directs the reader to a book by N. A. Ivanov on “Eastern despotism” rather than to the works of Aristotle, Bodin, and Montesquieu to find out what they wrote. Yet, I started with the definition that derives from Aristotle of despotism as “the ruler’s having unrestricted power within his or her realm.” Then I added that the term *despot* later “came to connote someone who exercised unfair and arbitrary power” (*M&M*, 85). I cited works of Bodin and Montesquieu specifically to indicate where they were using the term in this sense (*M&M*, 86, n. 6). For the purposes of the discussion in that section of the book, I indicated that I would be using the term *despotism* “in the connotative sense of unrestricted power in a society, with the implication of an unfair and arbitrary exercise of that power” (*M&M*, 85). Finally, I added that “any government can potentially act in a despotic way” according to the definition I had just provided (*M&M*, 86). I tried to be as specific as I could in defining this term and how I was using it. If Goldfrank feels that I misrepresented the views of any “key theoretician,” then he should point out where and specifically in what way I did so.

The rest of Goldfrank's discussion of despotism is confusing, in part because he misunderstands my discussion in *Muscovy and the Mongols*. This is clear from his statement that I "need not dismiss despotism as a 'false' issue" (265). Yet, I did not dismiss despotism in Muscovy as a false issue, but "oriental despotism" as writers have used it since Montesquieu – that is, as a mirror to reflect what they see as unfair and tyrannical practices of their own governments:

The entire issue of "oriental despotism" is a false one, concocted initially in the eighteenth century by critics of the French monarchy as a means of criticizing that government. . . . It was then one short step to connect "oriental despotism" with earlier foreigners' claims that Muscovy was despotic (*M&M*, 107).

Thus, as I thought I had gone to some lengths to demonstrate, the historiographical notion of "oriental despotism" is different from primary source evidence relevant to the historical question of whether the Muscovite state was despotic.

Why was this distinction lost on Goldfrank? I think the answer may lie in his claim that "[a]n understanding of early modern Russian 'despotism' (however called) can still help us understand modern Russia's dilemmas, where law and property are so weak; where the state, however inept and inefficient, continues to own most of the land and retains arbitrary powers of confiscation; and where the most prevalent form of social power is gangster tribute-collecting, not productive or commercial capital" (265). Such a statement is a variant of the "oriental despotism" phenomenon, namely picking out those things one does not like in the contemporary Russian (and before that Soviet) state and attributing those bad things to Muscovy's government. Instead, whatever problems or dilemmas present-day Russia faces are due almost entirely to its course of historical development in the 20th century, including two brutal world wars, a devastating civil war, the Cold War, and over 70 years of oppressive communist rule. It has little if anything to do with the structures and functions of government in Muscovy from the 14th through 17th centuries. So, Goldfrank's misreading of my discussion of "oriental despotism" seems to be the result of his falling into the "fallacy of presentism" that Fischer warned us about 30 years ago.⁹⁶

Goldfrank issues a number of other pronouncements that have no correspondence to any evidence that I am aware of. For example, he writes: "In arguing that the period of Chinese influence on the Mongols was not one of despotism, Ostrowski misses Wang An-shih's land-nationalization policies in the

⁹⁶ David Hackett Fischer, *Historians Fallacies: Toward a Logic of Historical Thought* (New York: Harper & Row, 1970), 135–40.

mid-12th century” (265, n. 27). At least four objections can be made to that single statement. First, Wang An-shih (Wang Anshi) (1021–1086) was chief councilor to the Sung Shen Tsung Emperor in the 11th century, not the 12th – that is, over a hundred years before the Mongols began their rise to power. Second, Wang An-shih undertook a number of administrative, economic, and educational reforms, but none of them involved “land-nationalization.” Third, Wang An-shih was dismissed by the Emperor in 1076 because of opposition within the upper levels of the administration to his reforms, and most of his reforms were abandoned by 1086. An attempt was made in the early 12th century to revive some of them, but the attempt met with mixed success, and his Great Reform measures wound up being completely discredited by the 1120s.⁹⁷ His dismissal and the abandonment as well as discrediting of his reforms shortly after they were enacted hardly speaks of any state despotic influence emanating from them. And finally, it may not be entirely accurate to say I “misse[d]” something when that something is largely irrelevant to the topic under discussion. No matter which of Wang An-shih’s reforms Goldfrank imagines may have been “despotic,” it does not negate the evidence Hartwell has gathered to show that the general tendency in China from the 8th to the 16th centuries was toward decentralization and the increased influence of regional and local administrations.⁹⁸ Clearly, Goldfrank has not done his homework here.

⁹⁷ For an in-depth analysis, see James T. C. Liu, *Reform in Sung China: Wang An-shih (1021–1086) and His New Policies* (Cambridge, MA: Harvard University Press, 1959). For brief overviews, see Edwin O. Reischauer and John K. Fairbank, *East Asia: The Great Tradition* (Boston: Houghton Mifflin, 1960), 206–08; C. P. Fitzgerald, *China: A Short Cultural History*, 3rd ed. (New York: Praeger, 1961), 395–406; John T. Maskill, *An Introduction to Chinese Civilization* (Lexington, MA: D. C. Heath, 1973), 130–33; Dun J. Li, *The Ageless Chinese: A History*, 3rd ed. (New York: Charles Scribner’s Sons, 1978), 206–12; and F. W. Mote, *Imperial China, 900–1800* (Cambridge, MA: Harvard University Press, 1999), 138–44. For discussions of aspects of Wang An-shih’s career, see the articles in *Ordering the World: Approaches to State and Society in Sung Dynasty China*, ed. Robert P. Hymes and Conrad Schirokauer (Berkeley: University of California Press, 1993), *passim*. For an exchange of letters between Wang An-shih and his chief critic, Ssu-ma Kuang (Sima Guang), see *Chinese Civilization: A Sourcebook*, ed. Patricia Buckley Ebrey, 2nd ed. (New York: Free Press, 1993), 152–54.

⁹⁸ Robert M. Hartwell, “Demographic, Political, and Social Transformations of China, 750–1550,” *Harvard Journal of Asiatic Studies* 42: 2 (1982), 365–442. Although there is evidence of some centralizing practices under the Sung, in themselves these practices can hardly be described as despotic. The continuation of decentralization policies by the Yuan dynasty indicates how little such centralizing practices influenced the Mongols. See, esp., David M. Farquhar, “Structure and Function in the Yüan Imperial Government,” *China Under Mongol Rule*, ed. John D. Langlois, Jr. (Princeton: Princeton University Press, 1918), 50–55; and Elizabeth Endicott-West, *Mongolian Rule in China: Local Administration in the Yuan Dynasty* (Cambridge, MA: Council on East Asian Studies, Harvard University, and Harvard-Yenching Institute, 1989), 125–27.

Other declarations of his are equally as counter-factual. There is no “problem posed by the assertion that the khan was *primus inter pares* and the fact of several massacres of rivals and their families” after (note: not “during”) the struggle for succession (265, n. 29). The khan was still *primus inter pares* among his princes and the elimination of the failed rival and his supporters would have been as much to protect his princes’ interests as the khan’s. I have no idea what Goldfrank is referring to when he says I “hedge[d]” when I pointed out that I “used Yüan and Ilkhanid sources to reconstruct the Qipchaq Khanate institutions . . .” (261, n. 5). Since we have no documents from and almost no sources directly about the Qipchaq Khanate, what we can say about that khanate is mainly the result of extrapolation from indirect evidence. Nor do I have any idea what he is referring to when he says I “hedge[d] concerning the purported role of the *iqta*, after [I] asserted its seminal role in the development of cavalry service land in Western Europe as well as Russia” (261, n. 5). I still propose that *iqta* was the origin of military land grants in both western Europe and in Muscovy, since I have neither seen any evidence nor read or heard any argument that has led me to modify these hypotheses.

Once again I thank David Goldfrank for his many positive comments about *Muscovy and the Mongols*. Although I don’t wish to appear ungrateful, I must admit to being disappointed with the quality of his objections. When one tries to verify the criticisms, one finds little or no evidentiary support behind them. If Goldfrank had read more carefully he might have had less to object to and even more to praise.

51 Brattle Street
Harvard University
Cambridge, MA 02138 USA
don@wjh.harvard.edu