Lessons of the Kurbskii Controversy—some retrospective notes

Even though Inge Auerbach in this same volume and Serge Zenkovsky in his review of Prof. Keenan’s book sought to cast doubt on my dating of the manuscripts, in fact they did so without any basis. Auerbach’s suggested analogies for the watermarks are simply wrong; Zenkovsky’s suggestions were based merely on the assumption that, as a neophyte, I could not know as much about the matter as my senior Russian colleagues. The edition of the Kurbsky-Groznyi letters edited subsequently by Ia. S. Lur’e and Iu. D. Rykov (Perepiska Ivana Groznogo s Andreem Kurbskim. L.: Nauka, 1979) adduced no evidence to change in any significant way the datings I had proposed both in my appendix to Keenan’s book or in this article from the Banff congress in 1974 (which was known and cited by Lur’e and Rykov). The new edition of the letters did include, inter alia, a lot of references to previously unknown copies of the letters, none of which altered the basic picture presented in Keenan’s book (on the basis of my examination of key manuscripts) about the chronology of the manuscript history. Of course, the interpretation of textual filiations and authorship presented in the new edition reaffirmed the traditional attribution of the letters and rejected Keenan’s hypothesis about their date.

It is important to remember here that we are talking about manuscript dates, not the dates when the texts they contain might have been written. A late manuscript tradition does not necessarily mean there could not have been an earlier date of composition of the texts it contains. In fact, with early Russian materials, it is common not to have manuscripts as old as their texts.

It was only subsequent to the appearance of the 1979 edition of the “Correspondence” that a copy of the first Kurbskii letter was found which could fairly confidently be dated significantly earlier than any of the previously known copies. The publication of the information about the new discovery was by B. N. Morozov, “Pervoe poslanie Kurbskogo Ivanu Groznomu v sbornike kontsa XVI-nachala XVII v.” Arkheograficheskii ezhegodnik za 1986 god (M.: Nauka, 1987), pp. 277-289. Morozov lays out carefully the paleographical and codicological evidence (and includes photographs of the pages with the Kurbskii text). He admits that the particular quire with the Kurbskii text cannot be reliably dated from the watermarks on its paper, but suggests (persuasively, I think) that the adjoining material in the manuscript supports a case for dating the copy of the Kurbskii text to the end of the 16th century. Thus, if he is right, this copy is roughly two decades earlier than previously known ones of any part of the “Correspondence.”

My purpose here is not to undertake a new review of the Kurbskii controversy, but merely to point out that what I wrote in my 1974 paper at that time was supported by all the then available evidence. Neither Auerbach nor Zenkovsky had evidence to the contrary, nor did the Russian scholars who produced the new edition of the “Correspondence” a few years later.

Daniel Waugh

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THE LESSONS OF THE KURBSKII CONTROVERSY
REGARDING THE STUDY AND DATING OF
OLD RUSSIAN MANUSCRIPTS

Daniel Clarke Waugh

The purpose of this article is to underline the necessity of more careful study of Old Russian manuscripts than has hitherto been the case if some of the questions and misunderstandings that have arisen in connection with the Kurbskii controversy are to be resolved. Professor Keenan's book bears witness to the fact that even in the case of works as "well known" as the Kurbskii-Gromyi "Correspondence," their manuscript tradition is in fact incompletely known and erroneous conclusions are drawn from it. Work which has appeared in response to Professor Keenan's questioning of the traditional attribution of the "Correspondence" displays uneven application of modern methods of manuscript study; as a result, if anything, our store of misinformation about the manuscripts may be increasing as rapidly as our fund of reliable information. The evidence that will be presented here, while not pretentiously exhaustive, will focus narrowly on issues pertaining to the manuscript tradition of the "Correspondence": the major issues raised by Professor Keenan's book can best be approached by others once some of the spurious objections to his thesis have been put to rest.

I am dismayed by the quality of scholarly publications regarding the Old Russian manuscripts from which so much of our knowledge of medieval and early modern Russian ultimately comes. It is not entirely clear whether many of my colleagues, especially those who have had little or no opportunity to acquaint themselves firsthand with the manuscripts, are aware that such a problem exists. However, in recent years there has been growing concern in the Soviet Union over the proper study and description of Old Russian manuscripts--see, for example, the very strong statements on such matters by Academician Dmitrii Sergeevich Likhachev in his fundamental Tekstologiya (1962) and most recently in his keynote address delivered at the Tikhonravov Lectures in Moscow in 1972. These
texts to replace those previous editions which have been based on only some of the important manuscript copies.

Yet even work that is now in progress may not meet all the high demands of modern scholarship. Many do not realize—although Likhachev and others have made these facts abundantly clear—that the aids which are available to those working on manuscripts and/or the training of those same scholars, as those who ultimately serve in manuscript repositories as librarians, rarely receive sufficiently thorough grounding in palaeography or filigrologia. Rarely are the individuals who have the kind of acquaintance with what formed the bulk of Old Russian literature—works that are normally termed "religious"—such as one could find among the scholars of the last century, whose descriptions of the contents of manuscripts are often exemplary. But even for well-trained scholars, the pressure of the plan may prevent the arduous job of describing and studying manuscripts from being done with requisite thoroughness. Leading specialists are often assigned to projects that have little to do with their specialties. When given time, these specialists may still be hindered by the simple fact that they are working with guides and techniques that have progressed little beyond what was achieved in the past century. Such is certainly the case for Cyrillic palaeography—no reliable guide for dating is available; the card file of dated hands that is being planned will undoubtedly be long in realization. Likewise this is true in the study of paper, where, despite many recent additions to the literature and reference guides, there are glaring gaps, and the most modern techniques remain largely beyond the ken of most researchers. One can indicate any number of other important features of manuscripts—ornament and bindings, just to name two—where a similar picture is to be found.

My illustrations of such problems will concentrate in two areas—the dating of manuscripts and their codicological analysis, that is, the total study of the physical and textual history of individual books.

In the absence of dated inscriptions, which are the exception rather than the rule in Old Russian manuscripts, one relies on other things on the evidence of handwriting and paper for the purposes of dating. As Likhachev has pointed out, dating by the use of handwriting to a precision of a quarter century is impossible given the available guides. Not only did copyists often have long productive lives, but one finds conscious anachronism in the Muscovite period, if not earlier. The existence of schools of copyists and scriptoria meant that a given style might have a surprisingly long life. Until a whole series of monographic studies has been devoted to such matters and as complete as possible a reference file compiled, using photographs of handwriting, one can expect little improvement in our ability to date by handwriting to an accuracy of less than half a century.

What about paper evidence? Although so to state is an oversimplification, paper evidence means first and foremost the study of watermarks and specifically the attempt to match those in undated manuscripts with those found in dated ones or books and reproduced in albums. As the late Allan Stevenson, the foremost American filigrologist (watermark specialist) once wrote, "It is all too easy to class watermarks with the soldiers and paper dolls." Stevenson went on to suggest that even those who take paper evidence seriously rarely understand its significance, its possibilities for providing vital information, and equally important, its limitations. The result has been an unwillingness on the part of many scholars to trust paper evidence, and perhaps more serious, the tendency to fail to exploit to the fullest the possibilities for using it. While I may be overly optimistic, it seems that there is an immense potential for establishing more precise information about dating and provenance of our Muscovite manuscripts than that which we now have, and for doing so in part on the basis of paper evidence. Yet the problems of essentially nonexistent training and lack of needed finding aids mean that progress will indeed be slow. The mistakes that scholars make show that not only the describers of manuscripts, but also those using the descriptions have only the faintest idea of concepts such as "similarity" or "identity" of watermarks (there is a difference), the significance of provenance of a dated paper to which one refers, the importance of runs, remnants, states, and so on. Even on the most basic level of rough matching, inexcusable errors are made, as the following examples will illustrate.

Generally conceded to contain among the earliest copies of both the first letter attributed to Kurbiski and the first of the replies attributed to Ivan, RS Leningrad Public Library (GPH), Fegodin Collection No. 1567 was
dated by Lur'e 1611-12. However, as I have demonstrated and unfortunately at least one of the reviewers of Apsaphen has chosen to ignore, Lur'e dating is erroneous, something he himself has admitted in private communication. My conclusion about the date of the MS was that it may be from the 1620s or early 1630s (the range of dates when one finds the type of watermarks in it), but given available guides and the fact that those watermarks have not been matched precisely with any yet known in dated books or documents, a more precise dating on the basis of paper evidence is at present impossible.

Nevertheless, in his book-length critique of Apsaphen, Professor Skrynnikov devotes some effort to arriving at a more precise dating of MS 1567. While on the one hand he rightly points out that the present margin of error (depending on whose views one accepts, anywhere from five to fifteen years) makes precise dating by paper evidence risky indeed, on the other hand, he seems confident that given the similarity between the watermarks in MS 1567 and certain ones in T. V. Dianova's unpublished album, one might date MS 1567 to the early 1620s. The similarities to which he refers are those of the decoration of the watermark, but not the initials contained in it—fact which means that the watermark in MS 1567 is not the same as any in Dianova's collection and therefore cannot be grouped with hers of the early 1620s on the basis of decoration any more than it can be fixed at around 1630 or so on the same basis.

One observation that should be added though is the following: while one must always be willing to allow considerable margin for error when using watermarks for dating, in certain circumstances we can be fairly certain of the terminus a quo even though we have not found the precise match for the watermark found in our undated book or manuscript. Such indeed is the case with MS 1567. The type of watermark found in it (a particular variant of a "pot") is apparently not known before ca. 1620, a fact reasonably well documented from the study of paper in printed Muscovite books. If the printing establishment in Moscow, which was one of the largest consumers of paper there in the seventeenth century, did not use any such paper prior to ca. 1620, it is extremely unlikely that such paper would have been used in a manuscript prior to that time. One is much more likely to find a situation where copies would be made on remnants of paper from a larger supply at a date later than that when the bulk of the paper was used, but not earlier than that date.

Unlike MS 1567, MS Lenin State Library (GKI) Underskii Collection No. 720 has remained essentially unstudied, even though it contains a copy of the first Kurbski letter that is textually of some interest. Referring to the unpublished second volume of Kurbski's "Sochinenia kniazia Kurbanskogo," Dr. Inge Auerbach indicates that this copy may be from the sixteenth century. K. A. Uvarov, in a reprint publication of 1972, rather triumphantly concludes on the basis (apparently) of his own visual examination of MS 720 that it is in fact a sixteenth-century MS. Uvarov finds one of the watermarks in it to be of the type of that published by N. P. Likhachev as No. 2718 (second half of the sixteenth century). It is instructive to compare a drawing (freehand, but a reasonably good representation) of the watermark in question with a tracing of the one cited by Uvarov and with yet another mark published in the album of Trombin, referred to in a pencilled note of the same century on the MS (a reference ignored by Uvarov).
Allowing for inaccuracies in scale and minor detail in my freehand drawing, I think it is painfully obvious that Uvarov's identification is erroneous, but that there are striking similarities between the mark in MS 720 and that in Tronson, taken from a manuscript book of 1634. Demonstrating convincing identification would of course demand the use of photographs, beta radiographs or the like and going back to Tronson's source rather than using his tracing. However, my visual examination of the paper and comparison of the watermark with that published by Tronson suggests that the two are "good likenesses" and hence it seems likely that MS 720 may date to the 1630s, a dating which examination of the other watermarks in it would seem to support. Admittedly the hand in the MS is archaic for the 1630s, but there is absolutely no way to push these watermarks back into the sixteenth century, when one finds "poc" watermarks, but none of the type found here.

MS GPB, Collection of the Solovki Monastery No. 962/852 contains what Professor Skrynnikov quite correctly indicates are the oldest and best copies of the first two letters which Kurbski is supposed to have written to the elder Vas'ian and a third work attributed to Kurbski and addressed to Ioan Mavrojenyi. In his critique Professor Keenan incorrectly lumped this manuscript with the other copies of the first two letters to Vas'ian as being from the middle of the seventeenth century. Skrynnikov in his critique provides at least partial watermark evidence suggesting that MS 962/852 dates from around the turn of the seventeenth century; my own findings suggest the first decade of that century. Despite the fact that Professor Skrynnikov has given us valuable additional information about the manuscript, a proper codicological study of MS 962/852 remains to be published.

What is the significance of this MS, containing works identified elsewhere as Kurbski's and in copies dating prior to the time when Shakhovsko is suspected of having initiated the "Correspondence" between Kurbski and Iwan? While certainly not totally irrelevant to the question of the attribution of that "Correspondence," MS 962/852 provides no evidence that helps decide the Kurbski controversy one way or the other. In terms of the relationship of the texts, it is the third "Kurbski Letter to Vas'ian," not the first two, that is directly tied to the first "Kurbski Letter to Ivan." Skrynnikov appropriately undertakes to demonstrate that there is a connection among all three letters to Vas'ian, but even if such a connection exists, as seems to be the case, one must ask whether it proves Kurbski's authorship of all three. I am not persuaded that such is the case. For one thing, it is important to remember that the attribution of the works in MS 962/852 to Kurbski is not found in that MS but only in the later copies of those works—the Solovki MS leaves spaces where the headings were supposed to have been. Secondly, one should keep in mind the fact that the third letter to Vas'ian exists in a totally distinctive manuscript tradition, one which does not appear to have begun until the 1620s or early 1630s. By themselves these facts do not, of course, disprove the assertion of the seventeenth-century headings that Kurbski wrote the letters, but nevertheless, one has reason to suggest that further examination of the attribution of the Vas'ian letters is in order. The early date of the Solovki MS, as one can see from the foregoing, thus really provides no firm evidence pro or con regarding the Keenan thesis.

Not only paper evidence but a variety of data must be taken into account in the description of Muscovite manuscripts, as one can see in the example of MS GPB, Furgudin Collection No. 1494, one of the well-known "Kurbski Collections," which is commonly dated around the end of the seventeenth century. While the following information does not substantially change that dating or the consequences of it, nevertheless it serves to illustrate the importance of studying ornament. MS 1494 contains two printed masovki on pages of its table of contents (fols. 1, 2). Using the album of Zernova, the foremost student of the Slavonic imprints of Muscovy and the early Imperial period, one determines that these masovki are known in printed books in one case for the period from 1678 until the middle of the eighteenth century, and in the other case appearing for the first time in 1706. If one is correct in assuming that Zernova's guide is complete and that the use of such printed ornament on a separate sheet made for a manuscript book would not precede the use of that same ornament in a printed edition, then the obvious conclusion is that MS 1494 cannot antedate 1706. Even though the handwriting and the watermarks (which can be identified with dated ones used over a period of decades, and then only roughly) could be from the seventeenth century, they do not contradict the evidence of ornament.

The question of dating is merely one aspect of the
codicological study of manuscripts. One should recognize
that codicology is a relatively recent discipline. 23
But how important it is, especially when we are dealing
with manuscripts such as those containing the "Correspond-
ence" or related works, manuscripts that are usually mis-
cellaneities including other works as well and which have
passed through the hands of those who mutilated the books,
alternated their original contents and the like. The study
of the so-called "convoy"—the works accompanying in the
manuscript tradition the one of particular interest to
us—can be of considerable importance for dating, attribu-
tion, or determining how a cryptopist or patron understood
the work in question.24 Yet we can say very little about
convoy if we cannot establish when the manuscript mis-
cellany as we now have it was put together, whether that
happened at the time of copying of the individual parts or at
some later date. Even if one takes such thorough descrip-
tions of Moscovite manuscripts as those found in Bychkov's
exemplary volume on parts of the Pogodin Collection,
published nearly a century ago, one finds the best that most
scholars have managed to do then or since: indications
based on the evidence of handwriting that such and such a
miscellany consists of a certain number of independently-
copied shorter manuscripts, or perhaps that the miscellany
is one and in a single hand. 25 Yet a closer examination of
the Pogodin Collection, some of whose manuscripts con-
tain works in the Kurbskii corpus, suggests that the inde-
pendent parts are not always independent and that many
whole manuscripts are in fact parts.

Certainly one cannot suggest that MS ODB Pogodin
Collection No. 1573 is not well known to scholars: its
contents were admirably described by Bychkov, and even before
his time, works contained in this MS were published in the
Abey of the Archaeographic Expedition. 27 Among others,
Kuntevich and Lux'e have used MS 1573 in their work on
the Kurbskii-Koriuny corpus. Certain aspects of MS 1573's
connections with other Moscovite MSS have been indicated
only recently, however, in my Appendix to Apotheosis, and
since the writing of the description there, some addi-
tional striking evidence regarding the history of MS 1573
has come to light. 28 It turns out that this miscellany is
only one portion of a large and immensely interesting
seventeenth-century shemyk, which the noted archae-
ographer E. M. Strovev acquired apparently in the 1830s,
divided into several parts, the order of which he rearranged,
and then bound in at least four separate bindings.

all now found in his collection (part of the Pogodin
Collection). While the bearing of this fact on the Kurbskii
controversy is not yet clear, I can think of no better ex-
ample of the way in which the history of even the "best-
known" Moscovite manuscripts is still relatively little
known and the way in which proper codicological analysis
can extend the horizons of our knowledge of them.

MS ODB Pogodin Collection Nos. 1567 and 1494 exhibit
similar evidence of alteration, evidence whose signifi-
cance for the study of these manuscripts struck me only
after returning to them following the publication of
Apotheosis. Strove commonly added a table of contents to
his manuscripts before he had them bound; the inscriptions
he made on these tables of contents generally indicated
the place and date when the manuscript was prepared by him
for binding. MS 1567 is an exception to the rule that
Strove prepared his manuscripts for binding in Moscow on
its table of contents, one finds the indication of Tver' in
1832, suggesting possible provenance of the manuscript
in the region of Tver'. Moreover, given the existence
of internal breaks in the MS and the knowledge we are ac-
quiring about the methods Strove used, it seems entirely
possible that other portions of this MS exist, possibly
bound separately or with other MS fragments in the Strove
shemyk that are now part of the Pogodin Collection. In
MS 1494 of that collection, also once part of Strove's
library, one notes that the original foliation has been altered
and that there was once another work contained in the current folios 238 and 239, a work that is
now missing. 29 This fact should be taken into account
when comparing MS 1494 with the other "Kurbskii Collec-
tions"; it may be that the missing part will still turn up
in the other manuscripts of the Pogodin Collection.

Sometimes the question of whether or not certain
works were always in convoy can be solved simply by look-
ing at the pencilled notations made on the MS by some
scholar at the turn of the century but not published.
Sad to say, modern scholars have tended to ignore such things
and often read only the one work of which they are prepar-
ing the edition. A case in point is MS Central State
Archive of Ancient Acts (TsGADA) Collection of the Library
of the Archive of the Ministry of Foreign Affairs (fond
181), No. 46. This book is a mother of the well-known
"shemyk Kurbskii," used by Kuntevich as the basis for
his edition of many of the works attributed to Kurbskii
and examined subsequently by Lux'e and others. Among the
many works found in these Kurbiskii Miscellanies are translations of chapters from the widely-known "Chronicles" of Stryjewski and Guzman. Lur'e attempted to deal with the question of when these Kurbiskii Miscellanies first appeared in their present form. While recognizing that none of them has been shown to date earlier than the second half of the seventeenth century, he suggested that the protograph was one compiled by Kurbiskii or his "Kuriski--\_
hiski" and included the Guzman translations. As Lur'e correctly pointed out, the Stryjewski translations can be excluded, since they are indicated in the MS to be the work of Andrei Izlyov in 1662. Yet the Guzman translations might likewise have been excluded, for a penciled note on fol. 322 of MS 60 in a hand of the turn of our century indicates "To peterovu Paszkowskovo 1611 g. v 1., a 110-1 po 559-\_iu stranitsu zdes' perevedeno." As I have already suggested in Appendix Jc to Professor Keenan's book, there is ample evidence for asserting that the Guzman translations are from the first Polish edition of 1611 (edited and expanded by Paszkowski), not from the first Latin edition of 1578 or subsequent sixteenth-century republications of portions of it.

Proper codicological study of the manuscripts would not only have prevented certain wrong assumptions about the original composition of the "Kurbiskii Miscellanies" but also have allowed us to more easily put forth by Nikolay Andreyev the so-called "Khororostin Chro-


Graph" somehow proves knowledge of the "Kurbiskii Correspondence" prior to the time that Shakhnovskii could have been. This manuscript, State Historical Museum (GIM), Uvarov Collection No. 1386 (116) is so called because of its contents and the fact that it belonged to Prince I. F. Khororostin in the middle of the seventeenth century. As a number of scholars have noted, MS 1386(116) is only one portion of the Khororostin Chronograph; the remaining three portions are found in other bindings in the Uvarov Collection.

Does the Khororostin Chronograph in fact prove knowledge of the "Correspondence" prior to Shakhnovskii's time? Ivan Andreyevich Khororostin, the assumed author of one of the works Professor Keenan posits as a source for the first Kurbiskii letter to Ivan, was a cousin of the owner of MS 1386(116), Ivan Fedorovich. While definite proof is lacking, the most reasonable hypothesis, given the appearance of the MS and the owner's inscription, is that the book was compiled for Ivan Fedorovich. While the date of copying cannot be determined precisely, there seems little reason to question a date of ca. 1640; in any event, a date of earlier than ca. 1625 cannot be supported by any evidence. The claims that have been made or implied on the basis of this manuscript regarding early knowledge of the "Correspondence" come from a line following the description of the events of the latter 1540s (a passage that is apparently condensed from the Sypovniaks kyj), where the copyist has noted, "a prostrannoe o sen pipet o posheereh i o smotrenii bolesti v gostode” v ustavovanece v vokolnoj knizhe Ivana Vasili'evicke vsorui gromote chto pisali v litvu ko


Kniaz'iu Ondreiu Kurbiskimu protiv evo otopiski" (fol. 320 verso). If one reads on, one finds reference to the fall of Kazan' and then a similar statement, "a prostrannoe pipet o vseiu kazanskii teatvstvii v knizke Kazanskogo uajda..." (fol. 321). Now if one examines the other portions of the manuscript of which MS 1386(116) is one part, one finds copies of the first two letters in the Kurbiskii-Groznyi Correspondence, the Kazan' History, and a variety of other works. The evidence of the passages cited above is then quite clear: they are simply cross-references to other works in the same manuscript, which the copyist of ca. 1640 knew were there. There is no reason whatsoever to see these cross-references as coming from earlier than the date of the manuscript itself, nor is there any reason to connect the manuscript or its contents with Prince Ivan Andreyevich Khororostin, the cousin of its owner. This example demonstrates very well the need for codicological analysis of Muscovite manuscripts by any scholar using them for whatever purpose; even without the completion of some much needed paleographical and illogographical studies and reference works, much more can be done with codicological analysis.

My conclusions from the foregoing may seem to provide little of positive value for resolution of the Kurbiskii controversy. The less than satisfactory study of the Old Russian manuscripts containing the "Correspondence" and related works must improve if the controversy is to be resolved and it is incumbent upon those who have no access to the manuscripts to be extremely wary of conclusions they cannot check themselves, even where those conclusions are based on the de vies examination of the manuscripts. I tend to agree with Professor Grozev's assertion in his thoughtful review of Apocalypse, that "the careful study of the manuscripts has not—and probably cannot—provide
conclusive grounds for a precise dating of the Kurbskii text" (the first letter to Ivan). But he might have gone one step further and suggested that neither has it provided any grounds whatsoever for a convincing confirmation of the traditional attribution.

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NOTES


3 For the Academy of Sciences Library, several volumes have appeared as part of the Opisanie rukopisnogo odela Biblioteki Akademii nauk SSSR. The Lenin Library has published, inter alia, Materialy sobranii rukopisii.

Opisanie, T. I. No. 1-No. 3005, Moscow, 1961; Sobranie I. I. Sakhnevicha i N. N. Kuznetsova. Opisanie, Moscow, 1959; and a number of thematic descriptions such as I. N. Rykov, "Spiski 'Istorii o velikom kniaze moskovskom' Kurskego u fondakh odela rukopisii," Zapisniki Obshchii rukopisii, vyp. 34 (1973), pp. 101-124. One should note as well that the State Historical Museum is the process of completing (but on a much less ambitious scale) the description of the important sviad Collection of Old Russian and Slavic manuscripts begun in the classic work of Gorskii and Nevozovskii in the last century; see, T. N. Protasova, comp., Opisanie rukopisii Sviadalka sobranii (1930-1975) v opisaniia K. I. Gorskogo i K. I. Nevozovskoi, pt. I, No. 577-819, Moscow, 1976. The technical quality of this edition, which is offset from typescript, is a good indication of the low priority which such enterprises receive in the eyes of the state publishing authorities. I am not aware of any intentions to publish descriptions of other undescribed collections in CPM, but one should note that a card catalogue of individual works in the collections there may be consulted by those working in the Manuscript Division.

4 See Rykov, "Spiski"; also his "Istorii o velikom kniaze moskovskom" A. M. Kurbskii, V russkom rukopisnom traditsii XVII-XIX vv. (Arkhiepiskopskii obsor spiskov, 1974), pp. 61-78; see also V. V. Likhachev, "Istorii o velikom kniaze moskovskom" R. M. Kurbskii, V russkom rukopisnom traditsii XVII-XIX vv. (Arkhiepiskopskii obsor spiskov, 1974), pp. 61-78; see also V. V. Likhachev, "Istorii o velikom kniaze moskovskom," V russkom rukopisnom traditsii XVII-XIX vv. (Arkhiepiskopskii obsor spiskov, 1974), pp. 61-78.

5 One such study that has been completed in recent years is the dissertation of D. M. Vostokhov, which has been only partially published as "Iz istorii rukopisnogo dela Rossi, XVII veka," Arkhiepiskopskii eshegodnik an 1962 g. (1963), pp. 56-76. Evidence of the growing concern over development of paleography is to be found in the proceedings of a conference held in 1971, "Konferentziia, posvящennaya metodike izdeleniya i izucheniiano knigovodstva v rukopisnom dela, Moskovskii gosudarstvennui institut im. 1. I. Lel'ena, Kafedrala rukopisnii, Moskva, 1971, pp. 61-78; see also V. V. Likhachev, "Istorii o velikom kniaze moskovskom," V russkom rukopisnom traditsii XVII-XIX vv. (Arkhiepiskopskii obsor spiskov, 1974), pp. 61-78.

Postavl'nye iskusstvo M. Gromova, Podgotovka tekstov E. S. Lukhacheva, Ia. S. Luk'o, in *Kommentarii* Ia. S. Luk'o, Moscow-Leningrad, 1951, p. 537.


R. G. Skrynnikov, *Izobrazitel'noe iskusstvo M. Gromova i Kurbskogo* *Podgotovka tekstov E. S. Lukhacheva, Ia. S. Luk'o, Moscow-Leningrad, 1951, p. 537.


K. Uvarov, "Kolizhive i novnaiennee spiski sochnennik Kurbskogo," *Testyly dokladov k naruchnot studenteshkoi konferentsii. Istoriya, Filologiya, Novosibirsk, 1972, p. 97:

One of the most convincing arguments refuting the opinions of E. Keenan in the manuscripts of original works of Kurbskii of the sixteenth century preserved in the USSR, which remained unknown to foreign scholars...It is pertinent to give our dating of the copy of this letter of Kurbskii to Ivan Graznyi (AL, Indol'nikov Collection, No. 720, fol. 138v). The copy is written in a single hand in a careful semi-natural of the second half of the sixteenth century: the watermark on fol. 138r, 138v, 139, etc., is a bat with a crescent of the type Likhachev No. 2718. Thus even a random short example plainly shows the complete lack of substance in the hypothesis of E. Keenan concerning the indicated correspondence as a literary phenomenon of the seventeenth century.

I am grateful to a kind colleague in Moscow for providing me with a copy of Uvarov's article.


The pencilled notation is on the page facing fol. 1 of the MS and reads: "From, a. Zdes' 1, 229." (The reference to fol. 229 is erroneous, however, since that particular folio does not contain the indicated watermark.) The Xerox of watermark No. 937 is from K. A. Trononion, *Arkhitektura i izobrazitel'noe iskusstvo dlia bumag dlia tsaria* ..., Moscow, 1944 (republished in facsimile under the editorship of J. S. C. Simons and with supplementary materials provided by S. A. Klepkov in *Theetriss' Watermark Album, Hilkenau: The Paper Publications Society*, 1965). The appropriate comparison is on the following page. The representation of the watermark in the MS is a freehand drawing, smaller in scale than the actual watermark; the Xerox copy from a tracing on the right is from the album of A. A. Gerasimov, *Filigran* XLI, 1944, 191, V. 8. Vruchnye mnogie pismenniki i podgotovki dokumentov russkogo protsostvbeta, Moscow, 1985. Regarding the obvious differences between the two pictures, one should note that while de vino comparison suggests that the size of the two marks is the same, the quality of Gerasimov's drawings is quite suspect. In particular in the given mark, the top letter, which I read as C, could easily be mistaken for S or D. The fact of the reversal of the letters so that...
they face the handle of the pot is important (usually for single-handed pots, the handle is on the left with the letters reading left to right). It would be advisable to check Geraklitov's source, an Oktoskh published in Moscow in 1631, to see whether in fact the watermark there is not closer to that in MS 720 than Geraklitov's drawing would seem to indicate. This comparison should, of course, be made directly with the MS or with photographs made from it. One might note as a desideratum that in the case of manuscripts whose dating may be of particular importance, any proper scholarly edition should include accurate reproductions of the watermarks in the paper, if such exist, since whether or not they can be identified with dated marks, their publication would remove some of the uncertainties that exist given merely verbal descriptions. Some scholars half a century or more ago did publish photographs of watermarks in editions of texts (e.g., A. S. Uritov, Drevnosti po Konshchingomu episku i podobnym, Moscow, 1908), but sadly this felicitous precedent has not been followed.


12 Xenian, Apospysia, p. 204, n. 66.

13 The contents of MS 962/852 have been detailed in I. I. Fortis'ev, et al., Opytvoe vypuskii Solovetskogo monastyr'a, nastolashchekihau v bibliotekh ka Rossiskoi duxnovoi akademi, pt. 2, Kazan', 1885, pp. 553-559; for Professor Skrynnikov's further observations, see his Pervyi ts'is, pp. 26-27. My own observations on the MS include the following: despite a number of changes in hand and numbering of the gatherings, paper evidence suggests that most of the MS is the work of one scribe at one time. The watermarks are numerous; one note in particular in the section containing the works attributed to Kurbskii (fols. 22a-25a) a single-handed pot topped by a rosette and with letters on the side P/BB, a good likeness of which is Likhachev, Paleography, 1952, No. 1952 (1994). This paper is the primary one to be found from fol. 66 to the end of the MS. Among other watermarks is a similar pot with letters TD/1 on fols. 74, 81, etc. (similar to the mark that appears to have been drawn with distortion in Geraklitov, Filigrani, No. 456 (1600); a one-handed pot with PD/1 (fols. 361, 363, etc.), a good likeness of which is Likhachev, No. 1952 (1994); a two-handed pot with a date 1596 (fols. 322, 457, etc.)--Geraklitov Nos. 755 and 756 (1600) are good likenesses; a two-handed pot with somewhat unusual top decorated by a rosette, on the side the letters AG and on the base the date 1555 (fols. 128, 130, etc.). It is pertinent to note that Likhachev Nos. 1952 and 2946 were taken by him from a single dated MS, the Gostunov Plesnat' row in the Solovii Monastery Collection; Geraklitov Nos. 456, 755, and 756 are also from a single book, a hronika otechestva printed in Moscow.

14 The earliest copies are in MS GPH, Pogodin Collection No. 1567, and CM, Museum Collection No. 1551.

15 A. S. Zarzynova, Opytvoe vypuskii Solovetskogo monastyr'a XVII-XXIII uka, 1677-1755, Moscow, 1963, Nos. 13 and 266 respectively. This book is another illustration of how a fundamental reference work receives such low priority that it is poorly published in a minuscule edition.

16 How easily one can be misled by the date in a manuscript can be seen from the comment made by a scholar on the Metropolitans' inscriptions inserted in MS 1404, to the effect that this is a dated copy of 1677. The reference was to a partially completed copy of an inscription on the top of fol. 258v: "185-go genvaria v 23 den' po' (the numbers are written with Cyrillic letters). As this
inscription exists in complete copies in other MSS.
"Kurbiski Miscellanies," it obviously is not here the
original, and in any event, it does not appear at the
end of the book, where it would have been found in the
original.

23 The paper and handwriting would suggest that the
table of contents at the beginning of the MS was compiled at
the time the copy was made and not later, as frequently
in the case in Old Russian manuscripts.

24 For a survey of the subject, see S. F. Zabedev,
"Kubdeckolka—sauka o rukopisnymi knigach," Voskroyil-
An excellent detailed example of codicological analysis
may be found in S. V. Kochnov, Osnovki russkoi diplom-
niki, Moscow, 1970, Chapter 4.

25 Regarding the study of "convoy" see D. L. Likha-
chev, "Izuchenie sostava shnornika dlia vyslania
istorii tekst paizveduemion," Trudy Odolno drevnerusskoi

26 A. F. Bychkov, Osnovki tekhonov-stanovskikh i
mezhkh rukopisn. imp. Biblioteki biblioteki, ed. I.
Osnovki tekhonov-stanovskikh i russkikh rukopisn. do-
brnok imp. Biblioteki biblioteki, St. Petersburg,
1862.

27 Ibid., pp. 139-146.

28 Kocan, Apagyma, pp. 126-130; my "X Izuchenii
istorii rukopisnogo sobranii P. M. Stroeva," Trudy
Odolno drevnerusskoi literatury, Vol. XXX (1976),
pp. 194-203, and forthcoming in Vol. XXXI.

29 The original foliation (in Cyrillic) jumps from
257 (on present fol. 258) to 306 (on present fol. 259),
but beginning with the latter an effort was made to erase
the numbers sufficiently to "correct" them to follow from
257. Similarly the signatures were altered. This
is precisely the kind of alteration of MSS that Stroev
engaged in.

30 Fontanitsa Tronom Gromovo, pp. 549-551.

31 Another example demonstrating how attention to
pencilled marginal notes would have prevented the publi-
cation of erroneous information is that of the published
description of MS library of the Academy of Sciences
(RAN), No. 32, 14, 12 in the usually reliable Osnovki
Rukopisnogo obelno Biblioteki Akademii nauk SSSR,
published description, apparently relying on the label on
the manuscript's binding, indicates that the manuscript
contains karyonki (translated newspapers and pamphlets)
for 1686, whereas in fact, as some scholar determined from
content analysis and noted in pencil on the margins,
the period covered is 1686-1689, with the chronologi-
cal sequence violated by the fact that the folios were bound in
arbitrary order.

32 See Andreyev's letter to the editor of Knyag
nosch, No. 111 (1973), p. 300, written for the benefit of
all these "knyag domoga istoricheskoi istina." The ultimi-
rate source for his information was probably S. O. Shmidt;
see, the latter's Stanislavich vostokova arxarchie, Moscow,
1967, pp. 15-26. While Andreyev has written a
lengthy review of Apagyma (in another time, a spekal-
stitshik E. Camana, Knyag nosch, No. 109 (1972),
p. 258-272), one can only regret that a scholar of his repute
has allowed emotion to cloud his judgment, as one can
see in the letter to the editor cited and in another to
the editor of Knyag nosch of 23 April 1972, in which
Andreyev objected first of all to the fact that "sreda
'ucherxyst sovetrikov' Kynana ne nazvanio ni odnogo itar-
nika, kotopriy byly by mskim po kul'turo."