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On the Kievan Letter from the Genizah of Cairo*

*To my teacher,
professor David Jacoby*

“The Kievan Letter — An Original Document of Khazarian Jews of Kiev” has not ceased to stir debate ever since it was published, under this title, by Norman Golb and Omeljan Pritsak in 1982.¹ Two major statements contained in the title, namely the Letter’s Kievan origin and its Khazar connection, have been hotly challenged; many points of detail in the editors’ commentary have been contested as well. Reviving the debate in all its complexity cannot be the purpose of the present study since its author sides with the editors both in situating the Letter in Kiev and in linking it to Khazars.

This study will rather focus on two issues in the Letter that have not been treated in a satisfactory manner either by the editors or in later scholarship. These issues are neither minor nor negligible. The first issue concerns the diplomatic aspect of the Letter. I will argue that the document that survived through centuries in the Genizah of Cairo was a copy — and not the original — of the letter produced by the Jewish community of Kiev on behalf of a member in distress. Then I will examine the impact of this observation on the historical interpretation of the document. The second issue concerns the monetary unit, *zaquq*, named in the Letter to describe the debt that needed to be repaid by its bearer. The Kievan Letter seems to be the earliest surviving text to employ this rare term. An attempt to situate *zaquq* on Rus’ian ground will prompt us to explore the complex nature of the Rus’ian *grivna*, which presents structurally similar traits. This exploration will touch on increasingly larger issues and lead to some observations on the monetary bases of the tenth-century East-West trade.

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¹ N. Golb, O. Pritsak, *Khazarian Hebrew Documents of the Tenth Century* (Ithaca — London, 1982), 1–71. Further references to this edition are provided in the text.

I. The Genizah document — a certified copy of the original letter

After a short exordium in the style of synagogal hymns, the writers of the Letter announce the business at hand: “Now, our dignitaries and masters, we, community of Kiev, (hereby) inform you of the troublesome affair of this (man) Mar Jacob ben R. Hanukkah . . .,” who stood surety for his brother and proved unable to reimburse his debt. After heaping praise on Mar Jacob, the writers of the Letter warrant for his good faith — “so we have sent him among the holy communities that they might take pity on him” — and close with an eulogy of charity and a reminder of its many rewards (pp. 10–15).

In presenting this smooth translation to the reader, Norman Golb did not hide the difficulty that it posed. If one strictly follows the word order of the Hebrew phrase, it would have to be translated: “Now, our dignitaries and masters, we (hereby) inform you, the community of Kiev . . .”. Golb (p. 6) supported his translation (quoted above) with arguments that have not been accepted by all scholars. First Simon Schwarzfuchs, in his review of the edition, and lately Marcel Erdal have opted for a literal translation of the phrase and for the ensuing view that the Letter was sent to Kiev from elsewhere.² It would seem useful, therefore, to restate the reasons why this view is not tenable and why the literal translation creates more problems than it solves.

The Kievan Letter is a typical letter of endorsement (*hamlatsa*), which attests the support of a Jewish community for one of its members forced by bad luck to seek charity in other communities (a redemption of captives being the most common cause of such a quest). It is addressed, accordingly, to “holy communities scattered to all (the world’s) corners (line 6)”. In the present case the community that issued the letter is itself an interested party since, in order to redeem Mar Jacob from his creditors, it advanced some of the money he owed and thus could potentially profit from the success of his collection. The endorsement loses all its meaning and value, however, if the community that issues it is not identified — as it would be the case here if Kiev were taken to be the destination of the letter rather than the place where it was produced. In the former interpretation, the anonymous community, of which Mar Jacob was a member, very awkwardly inserts a specific appeal to the community of Kiev in a circular letter which contains the announcement: “so we have sent [Mar Jacob] among the holy communities that they might take pity on him (lines 16–17)”. Should one conclude that Mar Jacob carried a separate letter for every single community he planned to visit — with the appeal for help diluted each time by the announcement that he and his sponsors did not count on the generosity

2 S. Schwarzfuchs, review of Golb, Pritsak (as above), in *Revue de l’histoire des religions* 201, 1984, 432–434, see p. 433; M. Erdal, “The Khazar Language,” in P. B. Golden, H. Ben-Shammai and A. Róna-Tas (eds.), *The World of the Khazars: New Perspectives* (Leiden — Boston, 2007), 75–108, see p. 95; cf. M. Эрдаль, “Хазарский язык,” *Хазары* (Jews and Slavs 16) (Moscow — Jerusalem, 2005), 125–139, see p. 131.

of that community alone?³ The interpretation endorsed by Schwarzfuchs and Erdal renders better one phrase but deprives the document as a whole of its logic and sense. I see no alternative, therefore, to admitting with Golb that one pronoun, *lakhem*, was misplaced in the text for stylistic reasons, thus creating an ambiguity that the context helps to resolve.

The main text of the Letter is followed by nine signatures of witnesses certifying its contents and the good faith of the bearer. The first among them, Abraham, carries the title of *parnas*, the community leader (line 25), and I would like to emphasize that such a short title is appropriate and understandable only if the community led by Abraham is previously identified. Most strikingly, Abraham's name, as well as the eight names that follow, are written by one hand, the same that wrote the main text of the Letter. Golb (pp. 5–6) has duly noted this palaeographical feature of the document but, to the best of my knowledge, it has received no commentary. It can have only one explanation, however. The Genizah document was not the original but a copy of the letter of endorsement issued by the Jewish community of Kiev in favor of hapless Jacob. Paradoxically, it was thus unfit for serving its original purpose: only the letter carrying the original signatures could be presented to potential donors as attestation of Jacob's good faith.

Yet the letter preserved is by no means a purely private copy. In the last line it carries a signature of another *parnas*, Isaac, apposed in a different script and ink. In the same line, it carries a runic inscription, inscribed in very peculiar ink, which has much contributed to the Letter's fame (Fig. 1). Like Abraham, *parnas* Isaac did not deem necessary to identify the community he led. This can only indicate, to my mind, that each *parnas* apposed his signature in his own realm of authority: Abraham was the first to sign the original document in Kiev, while Isaac certified a copy executed in a place, necessarily different from Kiev, where he headed the local Jewish community. I will later propose some considerations on where this place could possibly be, but however located, this must have also been where the letter was marked with the runic script. In other words, the diplomatic aspect of the Letter shows it to be a copy of the original *hamlatsa* issued by the Jewish community of Kiev, a copy certified by the head of a different Jewish community, probably for the specific purpose of presentation to the authority, that marked it in the runic script in the left lower corner.

Omelian Pritsak, who provided the document with an historical commentary, has neglected its diplomatic features. After having identified the runic script as Khazarian, Pritsak wrote: «The Kievan letter was written in Kiev in Hebrew and was read and annotated by someone, evidently an official charged with reading

3 I fail to understand the logic of the statement by Erdal, *ibid.* (2007), p. 95, that “[the letter] might then be appealing to the community of Kiev for help by mentioning that other Jewish communities were also being called upon to do so.” His linguistic argument against the Kievan provenance, namely “the letter may have been sent from a place where the language spoken was *not* Khazar (*ibid.*)”, is hardly pertinent since few would claim that Khazar was the language spoken in Kiev.



Fig. 1. The Kievan Letter from the Genizah of Cairo (Cambridge, T-S /Glass/ 12.122).
After: Golb, Pritsak, *Hazarian Hebrew Documents* (cited n. 1).

documents, in Khazarian script and language, whose annotation certified the validity of the document for travel purposes» (pp. 42–43). The assumption that the Kievan Letter was counter-signed in Kiev by a senior Khazar official prompted Pritsak to a long discussion of the chronological limits of Khazar power in Kiev and of the circumstances of its conquest by the Rus'. Pritsak reached the conclusion that Khazar rule in Kiev extended in the early 930's and that the Kievan Letter should be dated towards the end of this period, ca. 930 (pp. 60–71).

Pritsak's emphasis on the role of Khazars in the early history of Kiev provoked a sharp rebuttal epitomized in a series of articles by Petro Tolochko.⁴ Tolochko proposed his own interpretation of the written sources and drew attention to the nearly total absence of archaeological traces of the Khazar presence on the site of Kiev. It would be awkward for the present writer to take a position in this debate. While I believe, not unlike Pritsak, that Kiev emerged in the late ninth century as a trading factory on the outskirts of the Khazar Qaganate and that it was conquered by Oleg 30 to 40 years after the date (AM 6390/AD 882) indicated in the Russian Chronicle,⁵ I would not associate myself with any of Pritsak's arguments. I will argue below that dating the letter as early as ca. 930 is not only implausible, but frankly impossible. My main point, however, is that Pritsak's basic premise — that the runes were inscribed on the letter in Kiev — is blatantly wrong. They were apposed in the place where the letter was copied and where the head of the Jewish community was named not Abraham, as in Kiev, but Isaac.

Pritsak read the runic inscription "in Khazarian" as *hokuriim* and translated it: "I have read [it]" (pp. 41–43). Golb pointed out, for his part, that this formula would be equivalent to the mark *legi* or *legimus* in Byzantine imperial documents (p. 15). Pritsak's reading and translation were accepted with little discussion and only minor misgivings by Erdal.⁶ They were, however, rejected by Igor L. Kyzlasov and systematically deconstructed, rune by rune, by Vladimir V. Napol'skikh who claimed that Pritsak assigned arbitrary phonetic values to most of the runes and created a verbal form that was linguistically impossible.⁷

A non-turcologist cannot take a position in this linguistic debate, but he can appraise the historical conclusions that the linguists draw from their analysis.

4 П.П. Толочко, "Спорные вопросы ранней истории Киевской Руси," *Славяне и Русь в зарубежной историографии* (Kiev, 1990), 99–121, see pp. 104–108; "Миф о хазаро-иудейском основании Киева," *Российская археология*, 2001/2, 38–42; "К вопросу о хазаро-иудейском происхождении Киева," *Хазарский альманах*, 2, 2005, 99–108.

5 К. Цукерман, "Перестройка древнейшей русской истории," *У истоков русской государственности: историко-археологический сборник* (Saint-Petersburg, 2007), 343–351, see pp. 346–349.

6 Erdal, "The Khazar Language" (cited n. 2), 97–99.

7 И.Л. Кызласов, *Рунические письменности евразийских степей* (Moscow, 1994), 34; В.В. Напольских, "К чтению так называемой «хазарской надписи» в Киевском письме," in Н. Голб, О. Прицак, *Хазарско-еврейские документы X века* (Jerusalem — Moscow, 2003), 221–225. The reader should note that the appendix by Napol'skikh only appears in the second edition of the Russian translation of Golb and Pritsak's monograph edited by V. Ja. Petrukhin, where it replaces Pritsak's response, printed only in the first edition (1997), to Petrukhin's criticism of his arguments.

Pritsak's reading concords, for Erdal, with his own hypothesis according to which the Genizah letter was dispatched to Kiev "from the Danube Bolgar realm."⁸ This theory defies imagination in a way few of Pritsak's constructions ever did. There is no trace of use of the runic script by the Danube Bulgars.⁹ Its sudden emergence in the state chancellery of tenth-century slavophone Bulgaria is as incongruous as the endorsement — or, as Erdal would put it, "censor's note" — apposed by a Christian Bulgarian official on a Hebrew letter issued by a Jewish community. It would also make little sense to travel from Bulgaria to Kiev to raise charity when Byzantium with its rich and numerous Jewish communities was near by. For Kyzlasov and for Napol'skikh, by way of contrast, the rejection of Pritsak's reading, of any reading of the runic inscription in fact, corroborates its attribution to the Khazar realm that has produced a number of runic inscriptions which all resist decipherment.

Another issue raised by the linguists concerned the interpretation of names borne by the witnesses who signed the Letter. It would be only a slight exaggeration on my part to say that Pritsak, in interpreting all six non-Jewish names as Turkic, preferably as ethnonyms, transformed the Letter into a miniature catalogue of Turkic tribes. Soon afterwards, however, Abram N. Torpusman and Vladimir Orjol pointed out that the name form GWSTT' does not need to be linked to Altaic languages and to the Petcheneg province of Talmac (p. 39), but rather to the well attested Slavic name Gostjata (Гостята).¹⁰ To my knowledge, this interpretation has not been contested.

The form SWRTH has been endowed by Pritsak with a Mongolian suffix and explained as "belonging to the tribe of Säwär", better known as Sabirs or Sabir Huns (pp. 37–38). Torpusman attempted a similar approach based on a Slavic basis in relating SWRTH to the Slavic Severa (or Severjane), a tribe that dwelled on the left bank of middle Dnieper. Thus, he constructed a form Severjata (Северята), one belonging to Severa, yet admitted that no such form is attested in Slavonic.¹¹ The most recent theory belongs to Erdal, who derives the form from the word *swartä*, which "means 'the black one' i.e. 'the dark one' in Gothic and in early

8 Erdal, "The Khazar Language" (cited n. 2), 97–98. Erdal's suggestion has been taken over and developed by Dan Shapira (Д. Шапира, "Евреи в раннее Средневековье в соседних с Россией странах," in *История еврейского народа в России*, том 1: А. Кулик (ed.), *От древности до раннего Нового времени* [Jerusalem — Moscow, 2010], 44–76, see pp. 61–62). Shapira believes that the Letter was authored, in 968–971, by the Jews of "Preslavac-Kiev" in Bulgaria, who suffered from the violence of their Rus'ian conquerors, led by prince Svjatoslav. It was supposedly destined for the Jewish community of Kiev, the capital of Rus', and through its intermediary to princess Olga, with the view of soliciting her intercession on behalf of the Bulgarian Jews. Yet, I see no way to relate this daring construction to the actual content of the Letter and I regret the perpetuation of the old confusion between the Bulgarian capital Preslav, conquered by Svjatoslav, and the half-mythical "Preslavac-Kiev."

9 Кызласов, *Рунические письменности* (cited n. 7), 38.

10 A.N. Torpusman, "Slavic names in a Kiev manuscript from the first half of the 10th century," in A. Demsky (ed.), *These Are the Names: Studies in Jewish Onomastics*, vol. 2 (Ramat-Gan, 1999) 171–175, see p. 174, cf. A.N. Торпусман, "Антропонимия и межэтнические контакты Восточной Европы в средние века," *Имя — этнос — история* (Moscow, 1989), 48–53 (*non vidi*); В. Орел, "О славянских именах в еврейско-хазарском письме из Киева," *Palaeoslavica* 5, 1997, 335–338.

11 Torpusman, "Slavic Names" (cited n. 10), 175.

Scandinavian.” Erdal draws attention to the fact that SWRTH was not a name but a nickname (*ha-mexuneh*) of a witness whose name he cites as Shimshon Yehuda. The man’s actual name was Yehuda (Shimshon being the patronymic of a previous witness), but otherwise the point is well taken. A nickname must have a meaning for people who apply it to its bearer and Erdal insists, therefore, on the use of Gothic “in this part of Eastern Europe”, namely, in Crimea, were it was spoken as late as the sixteenth century according to the testimony of the Holy Roman Empire’s ambassador to the Sublime Porte Ogier Ghislain de Busbecq.¹²

The idea that Gothic was spoken in tenth-century Kiev — or, according to Erdal’s localization of the Letter, in tenth-century Bulgaria — has the attraction of novelty, but it is hardly plausible. The likely explanation of the nickname has been provided by Vladimir Orjol, who put the accent on the most striking feature of Yehuda’s signature: the lack of a patronymic replaced by a nickname that also explains its absence. According to Orel, the *waw* of SWRTH had been misplaced before rather than after the *resh*, thus dissimulating the word SRWTH, Slavic сирота, orphan.¹³ This solution can be slightly simplified. The nickname can be read either SWRTH or SYRTH, as the scribe barely distinguishes between *waw* and *yod* (cf. the *waw* in *ha-mexuneh*); in any case, the confusion between *waw* and *yod* is most common in Hebrew manuscripts, especially in words that the scribe could not recognize and vocalize. The fact (not acknowledged by Orjol) that the signatures in the Letter belong to the copied part of the document becomes, in this respect, essential. Yehuda’s nickname SYRTH indicated that he had no known father (or parents in case of his being abandoned as a baby); there is nothing to support Orjol’s assertion that it indicated Yehuda’s status as a proselyte (*ger*). This reading has the advantage of explaining both extraordinary onomastic features of the signature: the lack of a patronymic and the use of a nickname. It firmly situates the signatory in a Slavic-speaking milieu.

Four more names were singled out by Pritsak as Turkic: KYBR, MNS, MNR and QWFYN. Erdal is highly critical of Pritsak’s attempts to link KYBR with the Kavar tribes that rebel against the Hungarians in *De administrando imperio*, and QWFYN with the river Kufis/Kuban and with the Kup’i Bulgars of *Armenian Geography*. Only for the name MNR does he consider possible Pritsak’s explanation as *Man är*, “great man,” though he adds that “even in the case that *Man-är* is the correct explanation of the name and is Khazar, that also does not help us much towards deciding what sort of a Turkic language the Khazars spoke”.¹⁴ The latter question is outside the scope

12 Erdal, “The Khazar Language” (cited n. 2), 100–101

13 Орел, “О славянских именах” (cited n. 10); cf., on the term, D. Wörn, “« Sirota » als sozialrechtlicher Terminus im russischen Mittelalter (Anmerkungen zum Verhältnis von rhetorisch-topischem und terminologischem Sprachgebrauch),” in: U. Halbach, H. Hecker, A. Kappeler (Hrsg.), *Geschichte Altrusslands in der Begriffswelt ihrer Quellen. Festschrift zum 70. Geburtstag von Günther Stökl* (Quellen und Studien zur Geschichte des östlichen Europa 26), Stuttgart, 1986, 171–187.

14 Erdal, “The Khazar Language” (cited n. 2), 99–103, see p. 103 for the quote.

of the present paper, but what I would like to retain is that the criticism of Pritsak etymologies, however justified, produced no alternative to the attribution of the names KYBR, MNS, MNR and QWFYN to the Turkic linguistic realm.

Thus, nearly thirty years of debate have enriched rather than destroyed the onomastic panorama drawn by Pritsak. Out of ten Kievan signatories of the Letter, five (Abraham the *parnas* included) display purely Hebrew onomastics, one, Yehuda, has a Hebrew name and a Slavic nickname, two have presumably Turkic names QWFYN and MNR and Hebrew patronymics, one has presumably a Hebrew name (it is partly lost) and the patronymic MNS, and finally Gostjata, whose Slavic name appears uncontested, has the patronymic KYBR, and what is more, bears the dignity of priest (*cohen*). This dignity is also claimed by MNR, son of Shmuel, the only other *cohen* on the list.

In commenting these combined Hebrew and “non-Hebrew” onomastics, Golb sensibly describes the Jewish community of Kiev as a community of converts who “did not, in accepting Judaism, throw off the old tribal associations,” but rather kept them alive through “retention of the old familial names” (pp. 26–27). Golb has more trouble with the priestly dignity of the Jewish Khazars, the priesthood in Judaism being strictly hereditary and inaccessible to converts. Convinced that “the converts came to practice a genuine rabbinical form of Judaism,” as described in the long version of the Letter of the Khazar king Joseph to the Jewish dignitary Hasdai ibn Shaprut of Cordova, Golb allows for a “process of sacerdotal metamorphosis” transforming the old priests of Tengri into Jewish priests at an early stage of the conversion process (*ibid.*), but not for actual sacrifices. I have argued elsewhere that the passages in the long version of king Joseph’s Letter, which obsessively insist on the strictest Jewish orthodoxy of the Khazars and which are absent from the short version, should be considered as interpolations in the text. I have argued, moreover, citing the explicit testimony of an early-twelfth-century *responsum* by R. Yehuda ben Barzilai of Barcelona, that the Khazars continued to perform sacrifices after their conversion.¹⁵ This shocking evidence can be disregarded, but it cannot be disproved.

Disregarding evidence has become an art of a kind in the current debate on the Khazars’ link to Judaism, which itself deserves a short digression. This debate has been launched by Shlomo Sand (Zand), a Tel Aviv University specialist in French intellectual history, in particular the French cinema. In his book with the eloquent title “The Invention of the Jewish People” — aimed, as one would expect, at deconstructing this notion — a large chapter, its central piece for many reviewers, is dedicated to the Khazars.¹⁶ The author admits that the “Zionist reconstructors

15 C. Zuckerman, “On the date of the Khazars’ conversion to Judaism and the chronology of the kings of the Rus Oleg and Igor. A study of the anonymous Khazar Letter from the Genizah of Cairo,” *Revue des études byzantines* 53, 1995, 237–270, see pp. 248–250.

16 זנד, ש. זנד, מתי ואיך הומצא העם היהודי? (Tel Aviv, 2008), translated in English as Sh. Sand, *The Invention of the Jewish People* (New York, 2009). I refer in the text to the English translation.

of the past” (p. 213), otherwise described as “memory-merchants” (p. 236) or “‘authorised’ historians” (p. 18), could not erase the story of the Khazars’ conversion to Judaism despite all their anti-Khazar bias. Nevertheless, he retells this story at length, while recognizing “being a non-specialist” and “exercising a high degree of speculation” (p. 20). Like any anti-Zionist pamphlet of its kind, Sand’s book has earned a huge publicity and a much wider distribution than all recent scholarly publications on Khazars put together. It is worthwhile, therefore, to take a glimpse at his 40-page exposé that has become for tens of thousands of readers the new “authorised” version of the Khazar history.

Surprises await the reader at every page. Thus he learns that attested Khazar history “begins in the fourth century CE” (rather than in the middle of the seventh) and that the Khazars, “a coalition of strong Turkic or Hunnic-Bulgar clans,” “mingled with the Scythians who had inhabited these mountains and steppes between the Black Sea and the Caspian Sea” (p. 214) (the last Scythians, reduced to a small enclave in Northern Pontus, disappear from the historical record nearly half a millennium before the Khazars arrive). Using an antique Hebrew translation of selected passages from the Armenian chronicle of pseudo-Sebeos, Sand declares that “the Great Kagan” mentioned is the ruler of Khazaria (p. 215), even though he could have learned from a recent English translation and commentary that the chronicle speaks of a *qagan* of the Turks. Sand suggests that two Hebrew characters, *shin* and *tzadik*, entered the Cyrillic alphabet “in the course of the Khazars’ early rule over the Russians” (p. 218); it matters little that the Cyrillic alphabet was brought to Rus’ unchanged from Bulgaria, a country that the Khazars never ruled, just as they never ruled, in fact, “over the Russians” either. Scores of similar blunders can be found over the next thirty pages. My aim, however, is not to review the book — consecrated by now as an outstanding historical achievement and required reading by such distinguished French historians as Pierre Assouline and Jacques Julliard — but rather to check how the author’s principled disregard for modern “authorized” scholarship impacts the one original thesis he strives to build.

The Khazars are central for Sand’s argument since he makes his own the thesis developed by Avraham Polak and popularized by Arthur Koestler, according to which the converted Khazars were the real ancestors of the East European Jews. Repackaged for the sake of deconstructing the Jewish people, this idea proved immensely popular. Many Israelis were so excited by the perspective of changing ancestry and turning into descendants of Huns and Scythians that Sand’s book became a huge bestseller in Hebrew. The original thesis, however, had suffered from a structural weakness, which has been often pointed out. The destruction of the Khazar Qaganate is dated in the sources and in traditional historiography to the 960’s; there is some debate concerning the exact year, but it is contained within this decade. A significant increase in the number of East European Jews — too significant, in the revisionists’ view, to be explained by a migration of German-

speaking Jews from Western Europe — can be observed in the late thirteenth century at the earliest (in reality, a century later). Koestler's popular essay did not explain where the descendants of the converted Khazars were hiding for at least three centuries after the destruction of their state, and even less so, how they survived the Mongol invasion that most severely hit the urban centers, in which they could have plausibly dwelled. Sand bravely faces this problem and provides a revolutionary new solution, which can be resumed in three words: Khazaria didn't fall!

Sand asserts (p. 228, with reference to D. M. Dunlop¹⁷), as his first proof of Khazaria's survival after the 960's, that "in 1016 CE, a joint Byzantine-Russian force attacked and defeated the Jewish Kingdom." Yet Sand should have noticed that the ruler of the defeated entity had a distinctively Christian first name, George, and a family name, Tzul or Tsules, well attested among the nobility of the Byzantine city of Cherson in Crimea. In reporting this episode, the Byzantine historian John Skylitzes makes no mention of a "Jewish Kingdom" and it is in no way related to the defunct Khazar Qaganate.¹⁸ The following explanation of Khazaria's survival after its alleged military defeat is frankly bizarre: "Thereafter, the Russian church was headed by the patriarch of Constantinople, but this holy alliance did not last long. In 1071 the Seljuks, rising tribes of Turkic origin, defeated the empire's considerable forces, and eventually the Kievan Russian kingdom, too, fell apart" (p. 228) (Rus' was strong enough until the Mongol invasion and, incidentally, the patriarch of Constantinople continued to appoint the Russian metropolitans until the mid-fifteenth century). Rather than on Khazaria's continuous survival as a state, however, Sand insists on its great resilience: he asserts that in the mid-thirteenth century it re-emerged as "a small kingdom ... under the aegis of the Golden Horde" (p. 229). Within its borders (which Sand does not define), this kingdom kept all the Jewish Khazars intact. Not for long, though, and this was the Mongols' fault: "The Mongols did not understand the needs of land cultivation in the vast territories they captured, and did not sufficiently care for the farming needs of the subjugated populations" (p. 229). The Jewish Khazars, who must have become good farmers in the course of their long conservation period, fled the Mongols. Manifestly inspired by the image of the lost tribes freshly released from behind the river Sambation, Sand depicts the Jewish Khazars as they "advance into the Western Ukraine and hence to Polish and Lithuanian territory" (p. 229), filling Eastern Europe with Jews, who had ethnically nothing in common with the (in any case, inexistent) Jewish people. Yet this majestic vision collapses as soon as it touches very basic historical evidence. Sand, as it is his custom, does not quote the sources, but one can surmise from his references that he builds up his scheme with some confused

17 D. M. Dunlop, *The History of the Jewish Khazars* (Princeton, 1964), 251.

18 C. Zuckerman, "Byzantium's Pontic policy in the *Notitiae episcopatum*," in C. Zuckerman (ed.), *La Crimée entre Byzance et le Khaganat khazar* (Monographies du Centre de recherche d'histoire et civilisation de Byzance 25) (Paris, 2006), 201–230, see p. 224.

data gathered in Avraham Polak's publications. The mysterious Khazar kingdom under the Mongols is *Gazaria*, a geographic name attested in many sources of the period. But it has long been shown that this name — which goes back not to the Qaganate of Khazaria, but, surprisingly enough, to the homonymous region in Eastern Crimea — designated in the sources dealing with the Golden Horde not some resilient Khazar entity, but ... the Golden Horde itself.¹⁹

The revisionist view of the origins of East European Jews is based on the estimation that the demographic potential of German Jewish (Ashkenazi) communities was insufficient to nourish a migration wave to the east. This estimation is as impressionist as any, but it is not my aim to discuss it. My point is that without the safe haven constructed by Sand outside time and space, the Jewish Khazars had no demographic potential whatsoever for nourishing a migration wave, which Sand sends, in the late thirteenth century, in the opposite direction. After their country was destroyed in the 960's, some, we are told, converted to Islam, while those who remained Jews could plausibly contribute demographically to the meager Slavonic-speaking Jewish communities of Eastern Europe, but this scanty Jewish population, badly hit by the Mongol invasion, had mostly vanished before the Ashkenazi migrants arrived.

Sand's fantastic construction has provoked an even more imaginative response. Moshe Gil, a distinguished Arabist from the University of Tel Aviv, claimed in a recent article that the Khazars had never converted to Judaism at all,²⁰ thus cutting the grass under the revisionists' feet and suppressing *in ovo* any further attempt to relate the East European Jews to the Khazars. Gil starts with the very true observation that no ninth-century Arabic source, including the major historiographers, when mentioning the Khazars, mentions their Judaism; in my study of 1995, this was one of the arguments for dating the Khazars' conversion to Judaism later than most scholars have previously believed. Then, surprisingly for an Arabist, Gil affirms (pp. 7–8) that Ibn al-Faqih al-Hamadani (ca. 903) does not mention it either, even though al-Faqih's testimony on the Khazars' conversion, which he describes as recent, is well known and widely quoted.²¹ In Gil's scheme, associating Khazars with Judaism was a pure libel, an ingenious way to discredit the Khazars in the Muslim world devised by none other than Ahmad Ibn Fadlan, the caliph's envoy to the Volga Bulgars in the early 920's. The major testimony on the Khazars' Judaism produced in the next generation by al-Mas'udi is attributed to Ibn Fadlan's influence (p. 10), although Gil brings no textual proof of al-Mas'udi's acquaintance with Ibn-Fadlan's *Report*. As we know, al-Mas'udi traveled in person to Transcaucasia bordering the Khazar

19 Zuckerman, *ibid.*, 221–226, with references.

20 מ. גיל, "הח'זרים לא התגיירו" (M. Gil, "The Khazars did not convert to Judaism"), *Zion* 75, 2010, 5-14. Further references to this article are provided in the text.

21 Dunlop, *The History* (cited n. 17), 109; Zuckerman, "On the Date" (cited n. 15), 246.

Qaganate, where he gathered information for his treatise. In proclaiming that only the Arabic sources — ultimately going back to a single source — really matter (p. 13), Gil takes no heed of Christian of Stavelot mentioning in the 860's, over half a century before Ibn Fadlan, the Khazars' fresh conversion to Judaism.²² He equally disregards the nearly contemporary Slavonic *Life of Constantine*. The Jewish tradition on the Khazars' conversion is briefly mentioned (pp. 13–14) as the figment of imagination of Ashkenazi Jews, distraught by persecutions and vacillating between excessive pride (in a mighty Jewish kingdom) and self-hate: becoming descendants of Mongols would detach them from the disgrace that had befallen the progeny of Abraham, Isaac and Jacob. Gil crudely projects Sand's mental scheme into the past without explaining to the reader, how Ashkenazi Jews could draw inspiration from Arabic texts. In fact, all Hebrew sources on Khazars were produced in the Orient, before any major persecutions, and contain plentiful data that could not be found in any Arabic source.

Though published in a leading journal in the field of Jewish history, *Zion*, Gil's piece, by its disdain for sources and modern scholarship (which the author chooses deliberately to ignore), stands on equal grounds with Sand's. Both contenders' approach is a far cry from the attitude adopted nine centuries earlier by R. Yehuda ben Barzilai from Barcelona. In his well-known *responsum* (contradicting Gil's claim, p. 14, that not a single rabbinic *responsum* mentions Khazars), he does not hide his embarrassment at the question how could it happen that the Khazars, after converting to Judaism, continued to perform sacrifices. He openly admits that his first inclination was to put their conversion in doubt. Then, after much inquiry and after collating the different versions of king Joseph's letter — their divergences were one of the reasons for R. Yehuda's doubts — he came to the conclusion that the Khazars did convert, that their kings were Jews... and that nevertheless they performed sacrifices.

22 See Zuckerman, "On the date" (cited n. 15), 245.

II. The monetary unit *zaquq*. On the date and the destination of the Letter

The bearer of the Letter, Mar Jacob, had stood surety for his brother, and after the brother was murdered by robbers, inherited a debt that he was unable to reimburse. The amount of debt is indicated as 100 *zequqim*.

The term *zaquq* (pl. *zequqim*) has been commented by Golb in a very unsatisfactory manner. Golb translates *zequqim* as “coins” and claims that this is “one of a large number of terms in Hebrew which signify coins” (p. 13, ad l. 16). He further argues that while “usually meaning silver coins,” the term is here “evidently” used for gold coins, designating the Byzantine *triens* (Greek *trimission/tremissis*), a gold coin that corresponds in weight and value to one third of a *solidus/nomisma* (p. 7). The only element I can retain in his analysis is that *zaquq* cannot designate the Arab dirham, the only silver coin that widely circulated in the Kiev region in the tenth century. A debt of 100 silver dirhams would have been far too small to justify the agitation that the Letter describes. However, the term cannot designate the Byzantine *triens* either. The last coins of this denomination were struck about a century before the Letter was issued, but as of the mid-eighth century their share in the monetary circulation became so insignificant that Philip Grierson describes them as “ceremonial issues.”²³ There is no indication that they ever reached Rus’. What is more, the *triens* was never used, in Byzantium, as an accounting unit; whatever coins were used for the actual payment, the accounts were kept in *nomismata*.

My major objection to Golb’s analysis concerns, however, his main assertion that, in the period under consideration, the term *zaquq* could signify simply “a coin.” Golb refers to Eliezer Ben-Yehuda’s classical *Thesaurus*, but both a careful perusal of the *Thesaurus* entry and, on a different scale, a consultation of the computerized Bar Ilan Responsa Project database show that the term *zaquq* is anything but banal allowing a number of specific observations that we will need to explore.

Zaquq starts being perceived as a coin in fourteenth-century commentaries on the Jewish marriage contract (*ketuba*), in which this term is used to quantify the amount owed by the husband to his wife in case of a divorce. For the commentators in question this is a foreign or obsolete notion, which they try to translate into monetary terms of their time. In the earlier usage, however, *zaquq* is never a coin. It designates a certain amount, usually a pound or half a pound, of pure silver, which can be embodied in ingots or, when specially indicated, in coins. The word *zaquq* means purified (metal) and as of the twelfth century it designates the *marca puri argenti*, the German silver mark. Two other observations compete the picture. The uses of the term *zaquq* are rare in the eleventh and barely existent in the tenth century. In the computerized dictionary of the Israeli Academy of the Hebrew

23 Ph. Grierson, *Byzantine Coinage* (London — Berkeley, CA, 1982), 184.

language our Genizah Letter is the earliest attestation. The term is used exclusively in the Western Ashkenazi realm (roughly, the Carolingian Empire). It is unknown to the Eastern Jews — in all Genizah documents, so rich in economic realia, it receives attestation in our Letter alone.

These elements of data are hard to reconcile with the Letter's dating in the early 930's, as proposed by Golb and Pritsak (see above). They also raise the question of Mar Jacob's intended itinerary. If he planned on gathering funds in Egypt, where the Letter was found, or, in general, among Jewish communities of Moslem or Byzantine realms, why would the amount of debt be expressed in units of value that meant nothing to the potential donors? Answers to these questions may change our perception of the Letter's date and of its historical setting. They will depend on a better understanding of the term *zaquq*.

The earliest mention of the term *zaquq* outside the Genizah Letter appears in a ruling by R. Gershom, surnamed the Light of the Exile for his role in forging the Ashkenazi rabbinic tradition. Born in Metz in the middle of the tenth century, he taught in Mainz until the Jews were expelled from this city in 1012 and probably died before 1028. His most famous ruling was the ban on taking a second wife, instituted for one thousand years some time before the expulsion of Jews from Mainz (which is sure finally now to have expired). He also banned divorcing a wife against her wish, both measures explained by the desire to adapt the norms of Jewish family life to those of German catholic society.²⁴ The ruling, which mentions *zaquq*, carries no date and can be situated in the late tenth — early eleventh century. It is transmitted in the collection of rulings (*Sefer ha-Dinim*) composed by R. Yehuda ben Meir ha-Cohen, a pupil of R. Gershom, before the middle of the eleventh century.²⁵

אמר ר' שלמה בשם הרב ר' גרשום זצ"ל, שאסור לנהוג כדרך שנוהגין במלכות זה, שנותן ליריד של קולוניאה זקוק של כסף שהוא י"ב אונקיות ומקבלים למלונם במגנצא או בוורמשא י"ג אונקיות פשיטים.

ושאל ר' יעקוב בר' יקיר את רבנו גרשום ואמר לו אינו יכול, אלא אם כן יקבל המלוה את הסחורה שהלוה נותן בה אותו כסף, יוליכנה המלוה עמו או שלוחו באחריותו של מלוה עד מקום שיקבל שם המעות, ושיתן לו י"ג אונקיות ושר'. ובלאו הכי אסור.

24 A. Grossman, The connection between religion and economy in the status of the woman in early Ashkenaz, in: מנחם בן-ששון (עורך), דת. (A. Grossman, ed., *Religion and Economy: Connection and Interactions. Collected essays*), Jerusalem, 1995, 139-159, voices some reserves regarding this explanation and links both rulings to the life style of Ashkenazi merchants, who stayed away from home for long periods of time and often took a new wife in their different ports of call. One can object to this link that Oriental Jewish sages, confronted with the same problems, produced no comparable rulings.

25 א. גרוסמן, חכמי אשכנז הראשונים: קורותיהם, דרכם בהנהגת הציבור, יצירתם (A. Grossman, *The early sages of Ashkenaz. Their life, leadership and work, 900-1096*), 3rd revised edition, Jerusalem, 2001, 175-210, especially 196-206. It should be pointed out that before the generation of R. Gershom and his pupils, the Ashkenazi *responsa* are sparse and no strong argument can be derived from the fact that they provide no earlier attestations of the term *zaquq*.

“R. Shelomo said in the name of R. Gershom, may the memory of the righteous be for blessing, that it is forbidden to act the way people act in this country in giving, for the fair of Cologne, a *zaquq* of silver, which is of 12 ounces, and in receiving in their house in Mainz or Worms 13 ounces of coins (*peshitim*).” According to R. Gershom, such profit would be only allowed if the lender, in person or through an agent, shares in the responsibility for transporting the merchandise acquired with the money he lent, i.e. becomes an active partner in the transaction that brings him the profit.²⁶

The proper explanation of the term *zaquq*, as used by R. Gershom, depends on understanding the halachic content of his ruling. It describes a practice of Jewish merchants who, while aware of the legal prohibition of usury, believed to have found a way to circumvent it without violating the Jewish law. Why then did they consider legitimate to lend 12 ounces of silver in the expectation of being repaid 13 ounces? The explanation surely resides in the opposition between silver in form of *zaquq* (pure silver) and of *peshitim* (coins). It was more convenient for merchants going to a distant fair to carry silver in ingots rather than in small coins of their city of origin, which would trade at a discount; by way of contrast, they would be repaying the debt after having realized the merchandise acquired at the fair in their city of origin, and thus amply supplied with local coins. Yet, what the merchants considered to be a crucial banking service which deserved remuneration, was dismissed by R. Gershom as usury. For our purposes, it is important to notice that R. Gershom did not conceive of *zaquq* as a fixed ponderal unit and considered it necessary to specify its weight.

In a case encountered by R. Yehuda ben Meir ha-Cohen himself, *zaquq* appears as an equivalent of a “half” (חצי). As for the term “half”, it is amply explained in another case related by the same author, a story of a Jew who sold a certain object to a bishop for 3 *litraoth* (pounds) of *pshitim* (coins). The bishop claimed that he had no coins available and proposed to pay the Jew 5 “halves”, which, as shown by Irving A. Agus, were freshly created *marcae* of 8 ounces, “half” a pound of 16 ounces. The advantage of ingots over coins — which escaped R. Gershom — was the only justification that the bishop could claim for paying 2.5 pounds of silver instead of 3 pounds, as promised.²⁷ Thus, in a text from the first half of the eleventh century, the term *zaquq* is used to designate 8 ounces of cast silver (in one or several ingots).

An explicit reference to *zaquq* as an ingot or a combination of ingots appears in a commentary on the Book of Deuteronomy-*Devarim* (ad 25, 13) by R. Shmuel ben Meir of Troyes (1080’s–1150’s). He describes the fraudulent practice of fabricating

26 Sh. Eidelberg, *The Responsa of Rabbenu Gershom Meor Hagolah*, New York 1955, p. 94 (n° 29).

27 י. א. אגוס, שיעור הכתובה בתור קנה-מידה לעמדתם הכלכלית של היהודים בגרמניה בימי הביניים (I.A. Agus, “The value of the marriage contract as a measure of the economic standing of the German Jews in the Middle Ages”), *Horeb* 5, March 1939, 143-168, see p. 152, n. 47. As pointed out by Agus (who refers to older studies going back to Leopold Zunz), several fragments of R. Yehuda’s *Sefer ha-Dinim* were printed among the *responsa* of R. Meir of Rothenburg (thirteenth century). Prof. Simcha Emanuel kindly points out to me that three such fragments in all employ the term *zaquq*.

a *zaquq* of two pieces, seemingly equal but actually slightly different in weight, and of weighing them together to show that the weight is right. Whoever gets the lighter piece is cheated, and so R. Shmuel advises to produce the *zaquq* in one ingot. Very numerous references to *zaquq* appear in the writings of his younger contemporary, R. Eliezer ben Nathan of Mainz (ca. 1090-ca. 1170). Thus, for instance, he mentions the practice of buying a *zaquq* of silver for future delivery at the price of 10 ounces, the measure (שער) of the *zaquq* being 1 *litra*. This practice is defended by analogy with the advance payment for a harvest, which justifies a preferential price for the buyer, but R. Eliezer judges it to be close to usury (אבק ריבית) and, therefore, forbidden (ad *Baba Metsia*, f° 5). The analogy makes it clear that the *zaquq* of silver, acquired for coins, was an ingot (or ingots) perceived as commodity (a view rejected by R. Eliezer). There can be no doubt that here, as in R. Gershom's ruling about a century and a half earlier, *zaquq* is associated with a *litra* (pound) of 12 ounces.

As of the late twelfth century, the term *zaquq* will designate exclusively the *marca puri argenti* of 8 ounces, which, with some regional variations, becomes the reference in Germany and beyond. Among the later commentators, however, there is much debate regarding the weight and value of *zaquq*, and this for a very practical reason: a tradition emerged to designate in *zequqim* the value of the standardized Ashkenazi marriage contract (*ketuba*). As convincingly argued by Agus, the new *ketuba*, which marks a profound transformation of this contract, goes back to R. Gershom. It deserves a short digression.

The Jewish marriage contract specified the husband's commitments towards his wife as well as the latter's rights in case of widowhood or divorce. It listed and evaluated the dowry, which the wife was entitled to recover, and indicated her fixed indemnity (*'ikar ha-ketuba*) — considered consecrated and non-modifiable, even though recalculated time and again in the monetary terms in use — as well as the voluntary addition by the husband (*thosefeth ketuba*). In a total breach with the Talmudic and Oriental tradition, the new Ashkenazi *ketuba* featured a fixed evaluation of the dowry and of the husband's "voluntary" contribution, each at 50 pounds of silver. These amounts are clearly attested (as the usage of "the last generations") in the middle-late twelfth century, when their sum is also presented as 100 *zequqim*.²⁸

The meaning of this change in the marriage contract provoked a sharp debate between Irving A. Agus and Abraham (Alfred) H. Freimann. Agus, who was probably the first to have truly grasped how enormous the combined amount of the "dowry" and of the husband's addition was, considered it to be an indication of the genuine opulence of the Ashkenazi Jews and of their wish to provide for their daughters' future. Freimann objected that the amount, which invariably appeared in the Ashkenazi *ketuboth*, was within the reach of only a select few; he suggested that

28 א. פריימן, שיעורי הכתובה באשכנז וצרפת בימי הביניים (A. Freimann, "The value of the marriage contract in Ashkenaz and France in the Middle Ages"), in S. Lieberman (ed.), *Alexander Marx Jubilee Volume* (New York, 1950), Hebrew part, 371-385, especially pp. 374-376.

it was made a standard fixture “in order not to shame those who have not” after the Jewish communities on the Rhine were ruined by the First Crusade. In responding to Freimann, Agus argued convincingly that the change in the *ketuba* could not be dated as late as the twelfth century, after the First Crusade, and maintained its attribution to R. Gershom. He emphasized the contractual binding value of the indemnities listed in the *ketuba*, which could not be taken for mere window dressing, but he did not properly address the economic issue raised by Freimann.²⁹

It escaped neither of the two scholars that the new contract formula had made the divorce extremely difficult, but this consideration was not properly developed, since Agus considered the amount of the indemnities as basically affordable and Freimann, as to a certain extent notional. More recently, Israel Yuval spoke against applying this consideration in explaining the revision of the *ketuba* since, as he pointed out, R. Gershom’s ban on divorcing a wife against her wish (cf. above) would have sufficed to prevent frivolous divorces.³⁰ One may object that applying this ban would have been difficult without a binding economic sanction stipulated in the *ketuba*. More generally, the perusal of references to *zaquq* in the *responsa* (which mostly involve cases of only a few *zequqim*) supports Freimann’s contention that hardly any Jewish husband was able to pay a *ketuba* of 100 *zequqim* representing, at the lowest estimate, way over 20 kg of silver. I might add, in respect to the argument advanced by Agus, that if the whole point was to provide for the bride’s future, there was no reason to depart from the ancient tradition and to make the *ketuba* uniform. What emerges from the combined evidence is the view of a conscious effort — and it matters little whether it should be attributed to R. Gershom in person or also to his pupils — to rid Jewish family norms of the traits, bigamy and free divorce, that surrounding German society found most aberrant. The heavy pecuniary penalty for divorce made it for all practical purposes impossible, but this situation reversed in the course of the thirteenth century, after the penalty was reduced to insignificance by the debasement of coinage, in which it was expressed.³¹

This short distraction should not divert us from the aim, which consists in commenting on the term *zaquq* as employed in the Kiev Letter. The evidence produced by R. Yehuda ben Meir ha-Cohen proves that this term was in use by the late tenth — early eleventh century. The early discrepancy between the *zaquq* of 12 and of 8 ounces shows that this accounting unit of pure silver emerged independently of the *marca* of 8 ounces. Most importantly, the exclusive association of the term with the

29 I. A. Agus, "The Standard Ketuba of the German Jews and Its Economic Implications," *Jewish Quarterly Review*, New Series, 42, 1951-1952, 225-232, defends his ideas presented in his study cited. n. 27, against Freimann, cited in the note above.

30 י. יובל, "הסדרים הכספיים של הנישואין באשכנז בימי הביניים" (I. Yuval, "The Financial Arrangements of Marriage in Ashkenaz in the Middle Age"), in Ben Sasson (ed.) *Religion and Economy* (cited n. 24), 191-207, see pp. 195-196.

31 On this process, see I. A. Agus, "The Development of the Money Clause in the Ashkenazic Ketubah," *Jewish Quarterly Review*, New Series, 30, 1939-1940, 221-256.

Ashkenazi realm reveals the links between the Jewish communities in Germany and in Kiev, and leaves little doubt that the Letter was destined for the potential Jewish donors on the Rhine and not on the Nile. These observations create the challenge to set the Letter in a chronological context closer to the first appearance of the term *zaquq* in the *responsa*, which would also account for its German connection.

I believe that the German connection is indeed the key to the Letter's dating. If we leave aside the mysterious *Rugi* of the Raffelstetten Customs Regulations (ca. 904),³² there is no evidence of links between Germany and Rus' before the 950's. By the late 950's, however, the relations were very close, since in 959, an embassy dispatched by princess Olga of Kiev requested from the king (future emperor) Otto I a bishop and priests to spread Christianity in Rus'. I will not review the sources for this episode, amply studied by Aleksander V. Nazarenko, and will only recall its final fiasco. Bishop Adalbert was only dispatched to Kiev in the early spring of 961, but he returned the very next year after having lost some of his followers barely escaping with his life. Most scholars explain this dramatic turn of events by the change of governance in Kiev. The chronicles situate in 964 (AM 6472) the "coming of age" of Olga's son, Svjatoslav, a convinced pagan, who took over the reins of power. Even some of the staunch adepts of the "conventional" chronicle chronology admit that this date is wrong putting Svjatoslav's takeover a couple of years before, in 961 or early in 962, in connection with Adalbert's mission.³³ It would be difficult, in fact, to attribute to Olga Adalbert's unceremonious expulsion, which spoiled the relations between Otto I's court and Kiev for the years to come.

It would hardly be daring to suggest that the diplomatic *rapprochement* between Rus' and Germany was accompanied — and possibly preceded — by the establishment of close ties between Jewish communities in both countries. In this period, it would have been natural for Kievan Jews to seek help from their wealthy coreligionists on the Rhine. After Adalbert's fiasco in 961 or 962, however, no Jew from Kiev would have dared to venture into the Carolingian realm. At that time, before Svjatoslav's Khazar campaigns later in the same decade, the way to the East was still open. This is why, I believe, Mar Jacob — who could not stay in Kiev and face his unsatisfied creditors — undertook this costlier and riskier journey. Someplace on the way, probably in Sarkel or in Atil, the head of the local Jewish

32 The debate on the identity of the *Rugi* has been recently revived by A.V. Nazarenko, who took up a strong position in identifying them as Rus'ian merchants trading along the route from Bavaria to Kiev, and on to Khazaria, throughout the second half of the ninth century: А.В. Назаренко, *Древняя Русь на международных путях: междисциплинарные очерки культурных, торговых, политических связей IX–XII веков* (Moscow, 2001), 71–112. As I have pointed out (Цукерман, "Перестройка" [cited n. 5], 348–349), not only this route and trade have left no archaeological traces, but there is neither any trace of the city of Kiev until the very end of the ninth century. I retain the traditional view, according to which the ninth-century *Rugi*, who seem to be fairly close neighbors of Raffelstetten in Bavaria, perpetuate the name (if not the ethnicity) of the homonymous tribe, which settled in the region, in Noricum, in the fifth century.

33 See Назаренко, *ibid.*, 263–310, with ample bibliography.

community certified a copy of his *hamlatsa*, which was then endorsed by a Khazar official and used as a *laissez-passer*.

My reasoning sets a very narrow date for the Letter: late 961 or early 962. This is the only period of time when Mar Jacob could request from the Jewish community of Kiev a *hamlatsa* intended to be taken to the Carolingian realm and then discover that, for a traveler coming from Kiev, this destination has become too risky. This dating has the advantage of placing the Letter closer to first mention of *zaquq* in the *responsa*, yet before the fall of the Khazar Qaganate. The combination of the Ashkenazi term *zaquq* with Turkic onomastics in a letter found in Cairo is highly symbolic as yet another evidence of the Jewish trade route, described a few years earlier by Hasdai Ibn Shaprut in his letter to the Khazar king Joseph as leading from Spain to the country of Gevalim (usually identified as Czechia) and then to the Caspian.

My reasoning leaves, however, one crucial question open. This question concerns the size of Mar Jacob's debt. As we have discovered, the early *zaquq*, a measure of pure silver, did not have a steady ponderal value. Applying the weight indicated by R. Gershom, a pound of 12 ounces, would saddle Mar Jacob with the spectacular debt of well over 30 kg of silver (I avoid the debate over the precise weight of the ounce). I wonder though if *zaquq* without further specification would mean more to the Letter's readers than a "measure of pure silver." This admission could imply that the main purpose of the Letter, rather than describing the precise amount owed, was to attest the good faith of its bearer, who had to flee his country because of a heavy debt, possibly too heavy to be repaid. It would also seem plausible that the Letter's authors, well aware of the ambiguity of the term, had primarily in mind a measure of silver in usage Kiev, which expressed the debt's value not only for the Jewish debtors, but also for the cruel gentile creditors, who put Mar Jacob in chains of iron for a year — the Rus'ian *grivna*. However common, this notion is by no means unambiguous.

III. The early Rus'ian *grivna*

Grivna is the only Rus'ian value unit that is mentioned in the earliest stratum of the Rus'ian chronicles. *Grivna* is also the only value unit employed in the earliest stratum of the Rus'ian customary law, *Pravda Rus'kaya*, corresponding to the first 18 chapters (in the modern division) of the so-called *Short Pravda*. After a brief survey, this evidence will be confronted with the data on pecuniary fines contained in the tenth-century treaties between Byzantium and Rus', thus providing the key for the appraisal of the *grivna*'s value in silver. Some observations on silver ingots, also known as *grivny*, will sustain the solution proposed. Establishing the precise *grivna* value should suggest a possible evaluation of the debt cited in the Genizah Letter and, incidentally, of the monetary mass in circulation in the early Rus'.

a. Grivna in the most ancient layer of the chronicle
(Narration of 1016/7)

As I have argued elsewhere, both the junior version of the Novgorod First Chronicle (N1Chr. jun.) and the *Povest' vremennyh let* (Tale of Bygone Years) go back to a Chronicle composed in the Kiev Caves monastery in 1076, which was, in its turn, an expanded adaptation of a Narration (*Skazanie*) composed in 1016/7.³⁴ The Chronicle of 1076 adds no independent mentions of *grivna*. In its early layer, the mentions of *grivna* are few and mostly relate to events that are contemporary with the composition of the Narration. Thus, we learn that in 1014, prince Jaroslav, who ruled in Novgorod, repudiated the yearly levy of 2000 *grivny* that he used to expedite to his father Volodimer in Kiev; he distributed 1000 *grivny* each year to his retinue.³⁵ The ancient annual tribute that the city of Novgorod paid to Varangians and that Jaroslav abolished in 1016 is estimated at 300 *grivny* (N1Chr. jun., 107).³⁶ 300 *grivny* was also the amount distributed by Volodimer to the poor at the feast he threw for the consecration of the church of the Transfiguration of Christ (*Preobrazhenie*), built to celebrate his victory over the Petchenegs (N1Chr. jun., 166).

Jaroslav's reward to the Novgorodians who raised him to the throne of Kiev in the winter of 1015/6 is specified for three categories of warriors: 1 *grivna* for a conscribed peasant (*smerd*), 10 *grivny* for a village head (*starosta*), and the same sum of 10 *grivny* for a citizen (*novgorodets*) (N1Chr. jun., 175).³⁷ Jaroslav's army consisted of 3000 Novgorodians, all categories combined, and of 1000 Varangian mercenaries (N1Chr. jun., 175). The citizens of Novgorod proper were organized as a "thousand" (вои славны тысящу: N1Chr. jun., 174). A recent study reminds us that this notional number should not be pushed too hard;³⁸ however, at this early stage in the history of Novgorod, it was probably not very far removed from reality. All potential warriors could not be available for an expedition launched at very short notice, but if we estimate the number of Novgorod citizens in the troop at 750, we cannot miss the mark by more than 20%. Assuming that there were about 50 village heads, Jaroslav's expenditure on his Novgorod allies can be evaluated, with the same margin of error, at $(750+50) \times 10 + 2200 \times 1 = 10\,200$ *grivny*.³⁹

34 К. Цукерман, "Наблюдения над сложением древнейших источников летописи," in К. Цукерман (ed.), *Борисо-Глебский сборник/ Collectanea Borisoglebica*, vol. I (Paris, 2009), 183–305.

35 N1Chr. jun. in *Новгородская первая летопись старшего и младшего извода*, под ред. А.Н. Насонова (Moscow — Leningrad, 1950), 168. Further references to N1Chr. jun. are provided in the text.

36 On this episode, see Цукерман, "Наблюдения" (cited n. 34), 205–206.

37 For this episode, see also the senior version of the Novgorod First Chronicle, ed. Nasonov (cited n. 35), 15.

38 П.В. Лукин, "События 1015 г. в Новгороде. К оценке достоверности летописных сообщений," *Отечественная история*, 2007/ 4, 3–20.

39 For a calculation along the same lines, cf. <Н.П. Бауер>, Б.А. Романов, "Деньги и денежное обращение," *История культуры древней Руси: Домонгольский период*, vol. I: *Материальная культура* (Moscow — Leningrad, 1948), 370–396, on p. 378. In recent publications dedicated to N. P. Bauer, V. V. Guruleva and P. G. Gajdukov indicate that this chapter, initially written by Bauer, was completed after World War II by Romanov and published under his name alone, since Bauer was condemned and executed in 1942.

Jaroslav's Varangian mercenaries must have been duly rewarded as well, especially since Kiev was taken without resistance and the campaign produced little booty. The author of the Narration had no reason to dwell on their reward, but he provided us, I believe, with an indirect indication of its value. Jaroslav's expedition of 1015, in which the author might have participated in person, supplied him with a model for the description of prince Oleg's campaign against Constantinople. Most readers know this description in the revised version of the *Povest' vremennyh let*, which attaches it to the fictional expedition of 907.⁴⁰ Oleg appears in this description at the head of a host of many tribes ("Varangians, and Slovenians, and Chud, and Slovenians (sic!), and Krivichi, and Merja, and Derevljany" and seven more tribes) embarked on 2000 boats of 40 rowers each. This fantasy is a common fixture in standard works on Byzantine and early Rus'ian history.⁴¹ The original description, however, is well preserved in N1Chr. jun. There, the expedition is situated after Igor's wedding to Olga and the birth of Svjatoslav, about four years before Igor's death; this is a reminiscence, tainted by legend, of the Rus'ian raid on Constantinople in 941.⁴² Oleg's victorious army consists only of Varangians and Slovenians, embarked on 100 boats of 40 rowers each (N1Chr. jun., 108, with a preference for the common reading of the *Akademicheskij* and *Troitskij* manuscripts). These are the exact composition and numbers of Jaroslav's expedition of 1015. This is why I find significant the legendary figure of the indemnity that prince Oleg made the defeated Byzantines pay: 12 *grivny* for each Rus'ian warrior (N1Chr. jun., 108). My guess is that in constructing the glorious past, the author imagined a reward in line with Jaroslav's generosity. On this assumption, 12 *grivny* was the amount paid by Jaroslav to each Varangian. His total expenditure after the conquest of Kiev can be evaluated at about 22 000 *grivny*.

The only other mention of *grivna* in the Narration goes back to the onset of Volodimer's reign when, after a rapid victory over his half-brother Jaropolk, he had to commit himself to paying a ransom to his Varangian allies of 2 *grivny* for each person found in Kiev. The author makes it clear that Volodimer was unable to fulfill his promise and had to extricate himself from it by trickery (N1Chr. jun., 127–128). The number of people subjected to ransom is hard to evaluate. We learn that Jaropolk, unable to withstand Volodimer's assault, first locked himself in Kiev with his warriors, then escaped with some of them to the fortress of Rodnja; many of his soldiers must have stayed in Kiev and had to be ransomed together with its inhabitants and refugees from surrounding settlements. It would appear obvious that the amount that Jaroslav discovered in his late father's treasury, accumulated over 37 years of prosperous reign, was not available to Volodimer himself ca. 978.

40 *Повесть временных лет*, I: изд. текста Д.С. Лихачева, перевод Д.С. Лихачева и Б.А. Романова (Moscow-Leningrad, 1950), 23–24. Further references to this edition (PVL) are provided in the text.

41 See, for a recent specimen, П.В. Кузенков, Русь Олега у Константинополя в 904 г., *Причерноморье в Средние века*, VIII, 2011, 7–35, with selective bibliography.

42 Цукерман, "Перестройка" (cited n. 5), 346.

b. Grivna in the Short Pravda and in the treaties between Byzantium and the Rus'

The “Short” (*Kratkaya*) *Pravda* is transmitted in the entry for the year 6524/1016 of the N1Chr. jun. (pp. 176–180). It consists of two sections. Unlike the second section and unlike the “Long” (*Prostrannaya*) *Pravda*, its first section (pp. 176–177), which forms its earliest stratum, names the compensatory fines only in *grivny*. The highest penalty, of 40 *grivny*, is for murder (if the family renounces blood revenge) or for an injury resulting in the loss of a hand. The next level of penalties “for dishonor” (with no lasting injury involved) goes down to 12 *grivny*, and a range of minor offences is punished with a 3 *grivny* fine. When a pecuniary settlement is envisaged as a compensation for material damage, the text stipulates that the payment should be made in money (*skot*), with no further specification.

The status of the *Short Pravda*, as part of the chronicle and as a document, is controversial. According to the view most recently defended by Aleksej Gippius,⁴³ its first section reproduces the legal “charter” (*gramota*) granted by Jaroslav in 1016 to his Novgorod allies. This so-called *Pravda Jaroslava* was first inserted in the so-called “Novgorod chronicle of the eleventh century” (a hypothetical text reconstructed by Shakhmatov, which lacks substance in my view) and then replaced, in the “chronicle of 1167” composed by German Vojata (another Shakhmatov’s *Konstrukt*, which, this time, I find plausible), by an expanded legal compilation, our *Short Pravda*. By way of contrast, Oleksiy Tolochko depicts the *Short Pravda* as a fifteenth-century compilation produced, expressly for inclusion in the N1Chr. jun., through a radical abbreviation of the *Long Pravda*.⁴⁴ In a study in preparation, I admit that the *Short Pravda* was only inserted in the Novgorod chronicle in the late fourteenth or the early fifteenth century, but I also argue that the chronicler, in searching for a legal document apt to impersonate Jaroslav’s “charter,” did not make one up but rather selected the most archaic version of the *Pravda* that he could find. The norms recorded in the first section of this document may, as it is commonly assumed, go back to the reign of Jaroslav (1015/6–1018, ca. 1023–1054⁴⁵) and even further back in the past. In the perspective of this study, the exclusive use of *grivna* in this section becomes an indicator of its early date.

43 А.А. Гиппиус, “К истории сложения текста Новгородской первой летописи,” *Новгородский исторический сборник*, 6 (16), 1997, 3–72, see pp. 61–63.

44 О. Толочко, “The Short Redaction of *Pravda Ruskaia*: A Reconsideration,” *Palaeoslavica*, 15, 2007, 1–56; А. Толочко, *Краткая редакция Правды Руской: Происхождение текста* (Ruthenica. Supplementum 2), Kiev, 2009. For a recent edition of *Pravda Rus'kaya*, see “Русская Правда. Краткая и Пространная редакции,” изд. М.Б. Свердлова, *Библиотека литературы Древней Руси*, IV (Saint-Petersburg, 1997), 490–517, 668–669, 675–676 (also available online).

45 See Цукерман “Наблюдения” (cited n. 34), 200–202, 221–222, on the chronology of Svjatopolk, whose reign preceded and then interrupted the reign of Jaroslav.

The first section of the *Short Pravda* carries another indication of its archaicity. This indication is vastly significant not only for the history of the text but also in a wider context, because, as I will attempt to show, it leads to a solid appraisal of the value of the *grivna*.

At the heart of my argument is the long suspected link between the provision of *Rus'kaya Pravda* and the clause in each of the two treaties between Byzantium and Rus' (911 and 944), preserved in the *Povest' vremennyh let*, that settles the compensation for bad wounds. The legal provisions in both treaties are very close. Not only are they nearly identical in content, but they also show similarity in wording, which is without parallel in the treaties:

Treaty of 911 (PVL, 27)

Аще ли ударить мечем, или бьеть кацѣм любо сосудомъ, за то ударение или бьенье да вдасть литръ 5 сребра по закону (v. l. покону) рускому; аще ли не имовит тако сотворивый, да вдасть елико можетъ, и да соиметь с себе и ты самыя порты, в них же ходитъ, а о процѣ да ротѣ ходитъ своею вѣрою, яко никакоже иному помощи ему, да пребываетъ тяжа отолѣ не взыскаема.

If one strikes with a sword or hits with any implement whatsoever, for this striking or hitting one should give 5 litras of silver according the Rus'ian law (v. l. custom); and if the one who performed the deed is impecunious, he should give as much as he can and he should take off the very clothes he wears, while for the rest he should take an oath according to his faith that there is no one to assist him, and from that time the amount at stake will no longer be collectable.

Treaty of 944 (PVL, 38)

Ци аще ударить мечемъ, или копьемъ, или кацѣмъ любо оружьемъ (v. l. съсудьмъ) русинъ грѣчина, или грѣчинъ русина, да того дѣля грѣха заплатитъ сребра литръ 5 по закону рускому; аще ли есть неимовитъ, да како можетъ, въ только же проданъ будетъ, яко да и порты, въ нихъ ж ходитъ, да и то с него сняти, а о процѣ да роту ходитъ по своей вѣрѣ, яко не имѣя ничтоже, ти тако пушень будетъ.

If a Rus'ian strikes a Greek or a Greek (strikes) a Rus'ian with a sword or a lance or with any weapon (v. l. implement) whatsoever, for this transgression he should pay 5 litras of silver according to the Rus'ian law; and if he is impecunious, he will be fined for as much as he can (pay) so that even the clothes, in which he walks, should be also taken off him, while for the rest he should take an oath according to his faith that he owns nothing, and so one will let him go.

The treaties contain several other references to the Rus'ian law (законъ) or custom (поконъ). This is the only case, however, when a whole clause, strikingly alien to the Byzantine legal practice, seems to have been dictated to the imperial officials by their Rus'ian counterparts in accordance with their unwritten customary law. What is more, Rus'ian law is specifically quoted as the source of the compensatory fine. Expectedly enough, scholars sought to discover the same legal norm and the same amount of fine in the *Pravda Rus'kaya*.

Both the *Short* and the *Long* versions of the *Pravda* contain provisions for compensating people who were struck with a weapon in a brawl. In both texts, however, two distinct cases are considered in a rather confused manner.

3. Аще ли кто кого ударить батогомъ, либо жердью, либо пястью, или чашею, или рогомъ, или тылеснию, то 12 гривнѣ; аще сего не постигнуть, то платити ему, то ту конецъ. 4. Аще утнеть мечемъ, а не вынемъ его, либо рукоятю, то 12 гривнѣ за обиду. 5. Оже ли утнеть руку, и отпадетъ рука либо усохнеть, то 40 гривенъ. 6. Аще будетъ нога цѣла или начьнетъ храмати, тогда чада смирать. 7. Аще ли персть утнеть которыи либо, 3 гривны за обиду.

3. If one strikes another with a stick, or a perch, or a fist, or a cup, or a (drinking) horn, or the reverse of his palm (?), then 12 *grivny*; if he is not caught, he should pay and that's the end. 4. If one stubs with a sword but does not unsheathe it, or with its handle, then 12 *grivny* for the offense. 5. Yet if one stubs a hand and it falls off or shrinks, then 40 *grivny*. 6. If the leg is intact or he starts limping, one should restrict the children (from seeking revenge?). 7. If one stubs any finger, then 3 *grivny* (are due) for the offense.

23. **Оже кто ударить мечемъ.** Аже кто ударить мечемъ, не вынезь его, или рукоятю, то 12 гривенъ продажи за обиду. 24. Аже ли вынезь мечъ, а не утнеть, то гривна кунь. 25. Аже кто кого ударить батогомъ, либо чашею, либо рогомъ, либо тылѣснию, то 12 гривенъ. 26. Не терпя ли противу тому ударить мечемъ, то вины ему в томъ нѣтуть. 27. Аче ли утнеть руку, и отпадетъ рука или усохнеть или нога, или око, или нос утнеть, то полувирье 20 гривенъ, а тому за вѣкъ 10 гривенъ. 28. Аже персть утнеть кии либо, 3 гривны продажѣ, а самому гривна кунь.

23. **If one strikes with a sword.** If one strikes with a sword without unsheathing it, or with its handle, then 12 *grivny* fine for the offense. 24. If one unsheathes a sword but does not stub, then 1 *grivna* of *kuny*. 25. If one strikes another with a stick, or a cup, or a (drinking) horn, or the reverse of his palm (?), then 12 *grivny*. 26. If, unable to support (the offense, the victim) strikes back with a sword, he carries no guilt. 27. If one stubs a hand and the hand falls off or shrinks, or a leg, or an eye, or one stubs the nose, then half the wergeld, 20 *grivny*, and for that person (the victim) 10 *grivny* for the mutilation. 28. If one stubs any finger, 3 *grivny* of fine, and for the person itself (he victim), 1 *grivna* of *kuny*.

The rulings of the *Short Pravda* belong to the most archaic part of the document (cf. above) and come shortly after the ruling on murder (§1 of the modern editions), which is punishable — unless avenged — with a fine (*vira* — wergeld) of 40 *grivny*. As noted by commentators, the *Short Pravda* preserves the original meaning of fine as a compensation for the victim or his family, while in the *Long Pravda* the prince collects the fine and the compensation for the victim is established separately in cases of bodily damage. Many other aspects of the text could call for a commentary, but we need to restrict ourselves to one. Leaving fingers apart, the *Short Pravda* distinguishes between two basic situations: a strike with a hand or any blunt implement that carries an offense but no bodily damage, and a grave mutilation of a major limb, manifestly by an unsheathed sword, involving a total

disability for a warrior. The same distinction is apparent in the better-preserved parallel section of the *Long Pravda*. While a permanently disabled hand entitles in the *Short Pravda* to a compensation equal to the full *vira* of 40 *grivny*, the *Long Pravda* attributes half of this amount as a fine to the prince, and 10 more *grivny* to the victim. The latter text, however, contains an expanded list of limbs, which strongly suggests that the withered §6 of the *Short Pravda* originally treated the loss of a leg at least with the same gravity as the loss of a hand. Thus both versions of the *Pravda* clearly distinguish between dishonor and disability. Now it remains to decide, whether the treaties of 911 and 944 relate to the former or the latter.

In making this decision, we find surprisingly little guidance in the abundant scholarly literature dedicated to the treaties and to the *Pravda Rus'kaya*. The often helpful commentary on the treaties by A. V. Longinov provides a good example of the common disarray.⁴⁶ He notes, as nearly all the commentators, the similarity between the beginning of the penal clause in the treaty of 911, “Аще ли ударить мечьмъ или биеть кацъмъ любо съсудьмъ, etc.” and the opening of §3 of the *Short Pravda*, “Аще ли кто кого ударить батогомъ, etc.” He claims that the treaties do not take into account “the quality of the strike” (which would be absurd), but then adds that the harshness of the fine would rather imply a wound with grave consequences for the victim’s health. The mention of the Rus’ian law or custom in the treaties, as Longinov rightly observes, refers most specifically to the amount of fine, but he can find no equivalent in *Pravda Rus'kaya* to the hefty sum of 5 Byzantine *litrai* of silver. In the numerous commentaries on these texts, I have discovered no alternative reasoning to the commonly drawn parallel between the “Аще ли ударить” clause in the treaties and the “Аще ли кто кого ударить” clause in the *Short Pravda*.

A straightforward translation in metrological terms of the alleged parallelism between the two clauses consisted in dividing the weight of five Byzantine pounds by 12; thus, it produced a *grivna* valued at ca 136 g of silver. Russian historical works from the second half of the nineteenth century onwards often cite this figure. The same simple calculation has been recently retained by Omeljan Pritsak.⁴⁷ Yet, when Valentin L. Yanin, in his renowned monograph of 1956, attempted a first comprehensive reconstruction of “the monetary and ponderal systems of the Russian Middle Ages,” the heavy *grivna* did not fit into any system. Parting from the same alleged parallelism, Yanin found a way to reduce the *grivna*’s weight by half, to 68.22 g, by adducing complex textological and metrological considerations.⁴⁸

46 А.В. Лонгинов, *Мирные договоры русских с греками, заключенные в X веке. Историко-юридическое исследование* (Odessa, 1904), 130–136.

47 O. Pritsak, *The Origins of the Old Rus' Weights and Monetary Systems: Two Studies in Western Eurasian Metrology and Numismatics in the Seventh to Eleventh Centuries* (Cambridge, Mass., 1998), 37–38.

48 В.Л. Янин, *Денежно-весовые системы русского средневековья. Домонгольский период* (Moscow, 1956), 48–52. The incompatibility of the hypothetical heavy *grivna* with the metrological systems attested in the region had been previously pointed out by Bauer, see Н.П. Бауер, *Денежный счет Русской Правды, Вспомогательные исторические дисциплины* (Moscow — Leningrad, 1937), 183–244, on p. 212.

This *grivna*, supposedly a relic of an ancient Slavic ponderal system, was one of the cornerstones in Yanin's reconstruction, but it also became its most contested element.⁴⁹ The recent critical analysis of Yanin's reasoning by Nazarenko makes any further discussion superfluous.⁵⁰ In Yanin's scheme, this archaic *grivna* was replaced in the second half of the tenth century, after the conclusion of the treaties, by a *grivna* of 51.19 g. This scheme was revised by Nazarenko, who placed the emergence of the 51 g *grivna* in the ninth century.⁵¹ Since Nazarenko maintained the afore-cited parallelism, he had to justify anew the old equation of 5 pounds of silver with 12 *grivny*, his *grivny* being lighter by one quarter than those devised by his predecessor. Yet, the *grivna* of 51.19 g is a sheer phantom, just as the previous one. Before arguing this point, however, and for a better orientation of the reader, I should suggest an alternative solution.

If we break through the bibliographical maze and read the texts as they stand, we discover no ambiguity. A clause in a treaty, which provides for the case when one person strikes another with a sword or, more explicitly, with a sword or a spear, cannot be interpreted as if it carried a tacit stipulation that the sword was unsheathed and the spear specially wrapped or turned around for the occasion. The provision in both treaties concerns a bad open wound that could be inflicted with a weapon or with any sharp tool. In the terms defined above, it does not deal with dishonor, but with potential disability. The main distinction between the injury clauses in the treaties and in *Pravda Rus'kaya* resides in the fact that the *Pravda* judges the gravity of a wound by its final consequence: the atrophy of a limb. The latter criterion is absent from the treaties and the reason for this is very obvious. Whether the limb remained functional or not could only be established after the wound had healed, but by that time the caravan of Rus'ian traders would long have left Constantinople. The decision on penalty that had to be taken on the spot reposed necessarily on the "quality" of the wound alone and could not wait for its healing. Otherwise, it stands beyond doubt that both treaties contain, after a clause on murder, a clause on a grave open wound, such as a strike by sword or by spear would normally inflict, not lethal but potentially debilitating. This clause must be related, in the *Pravda*, to the provisions on wounds and not on offensive blows.

This analysis brings us to the main point, which concerns the amounts of fine. The hefty sum of 5 pounds of silver, which, as the treaties say, was collected "according to the Rus'ian law", should be related to the heaviest fine of 40 *grivny* and not to the fine of 12 *grivny*, as commonly believed. The Byzantine *litra*, like the Roman pound, used to be estimated at 327.6 g, but this classical estimation has long been recognized as excessive. In recent studies, the estimated weight of a

49 С.Н. Кистерев, *Русское денежное обращение в трудах В. Л. Янина* (Moscow, 2004), 46–51.

50 Назаренко, *Древняя Русь* (cited n. 32), 123–129.

51 Назаренко, *ibid.*, 130.

litra-pound has been reduced to 326.34 or even 324.72 g.⁵² Thus, the fine stipulated in the treaties should have represented between 1631.7 and 1623.6 g of silver.

In tenth-century Byzantium, however, the silver circulated in coins, not in ingots. Bankers and traders were required to accept coins with no visible defects by count, not by weight. Cécile Morrisson, following Philip Grierson, evaluates the nominal weight of a silver *miliaresion* at 2.98 g, close to the nominal weight of the dirham (the actual weight of the *miliaresia* preserved is mostly lower).⁵³ It would appear that the *miliaresia* were nominally struck at the rate of 108 coins per *litra*, in a clear ponderal relation of 3:2 to the *nomisma*, which was struck at 72 per *litra* but was also slightly underweight at the time. Five *litrai* of silver would then be equated to 540 *miliaresia* in coined money, perhaps a few more to compensate for the deficient weight of the silver coinage.

Tenth-century sources attest two exchange rates for a *miliaresion*: 12 or 14 to one gold *nomisma*. These sources are different in nature. The Book of Ceremonies, compiled towards 946, applies the old official rate of 12 *miliaresia* for 1 *nomisma* to the imperial distributions, and this is also the rate prescribed, not before the second half of the tenth century, in a *scholion* to the Book of Eparch.⁵⁴ These normative indications contrast with the forthright statement of a South Italian act from 959 equating a *solidus* (*nomisma*) with 14 *miliaresia*.⁵⁵ Michael Hendy, who discusses this evidence, also points out that the accounts of the Cretan expedition of 949, appended to the Book of Ceremonies, suggest for a *nomisma* an exchange rate superior to 12 *miliaresia*.⁵⁶ What is more, Grierson and Hendy relate to the same period the indication of a legal *scholion* (one of the *scholiae nomicae*, notoriously difficult to date), according to which the *miliaresion* used to be valued at 1 $\frac{3}{4}$ *keratia* (and thus the *nomisma* at 13 $\frac{5}{7}$ *miliaresia*).⁵⁷ Hendy suggests a fluctuation in the value of silver currency, while Morrisson believes in its slight devaluation in the course of the tenth century,⁵⁸ but the fact of the matter is that all our evidence

52 C. Morrisson, "Byzantine money: Its production and circulation," in A. Laiou (ed.), *The economic history of Byzantium from the seventh through the fifteenth century*, I–III (Washington, D.C., 2002), 909–966, see p. 920.

53 Morrisson, *ibid.*, 930; Ph. Grierson, *Catalogue of the Byzantine Coins in the Dumbarton Oaks Collection and in the Whittemore Collection*, vol. III: *Leo III to Nicephorus III, 717–1081*, part 2: *Basil I to Nicephorus III (867–1081)*, (Washington, D.C., 1973), 554–558 (with the indication of weight of the coins preserved).

54 Constantinus Porphyrogenitus, *De cerimoniis aulae byzantinae*, 55, ed. K. Reiske (Bonn, 1829), 799–800; *Das Eparchenbuch Leons des Weisen*, ed. J. Koder (Wien, 1991), 76 (in the apparatus to ch. I, 4, line 58), cf. introduction, pp. 37–39, on the traces left in the Book of Eparch by the editors between the mid-tenth and the early eleventh century.

55 *Codice Diplomatico Barese*, vol. I : G.B. Nitto de Rossi e F. Nitti, *Le pergamene del duomo di Bari (952–1264)* (Bari, 1897), p. 6 (n° 3).

56 M.F. Hendy, *Studies in the Byzantine monetary economy c. 300 1450* (Cambridge, 1985), 504–506.

57 F. Hulstsch, *Metrologicorum scriptorum reliquiae*, I (Leipzig, 1864), 308–309, with Grierson, *Catalogue* (as in n. 53), part 1: *Leo III to Michael III*, p. 67, and Hendy, *ibid.*, 504.

58 Hendy, *ibid.*, 505; Morrisson, "Byzantine money" (cited n. 52), 930.

is strictly contemporary. I would rather suggest that in the tenth century, the old official rate of 12 *miliaresia* to a *nomisma*, which overvalued the silver, was not abandoned, but in commercial transactions suffered a market correction (that will find an explanation below), which brought it closer to 14 silver coins to a *nomisma*. At the temptingly precise rate of $13 \frac{5}{7}$ to a *nomisma*, 540 *miliaresia* — the minimal equivalent in coined silver of the 5-*litrai* fines of the treaties — would be worth 39.4 *nomismata*, while 548.6 *miliaresia* would equal 40 *nomismata*. Thus the 40-*grivny* fines of *Pravda Rus'kaya* would equal in value 5 *litrai* of silver if we recognize in *grivna* the silver equivalent in of the Byzantine gold coin, the *nomisma*.

c. The “grivna” ingots and the value of the ancient grivna

The reader has understood by now that the ancient *grivna*, the value of which is so hotly debated, existed as “money of account” that had not materialized as a solid piece of silver. Silver ingots, which will be also, with time, designated in the sources as *grivny*, only appeared by the early twelfth century. Four types of ingots of standardized shape and weight circulated in Rus' (Fig. 2);⁵⁹ ingots of a different

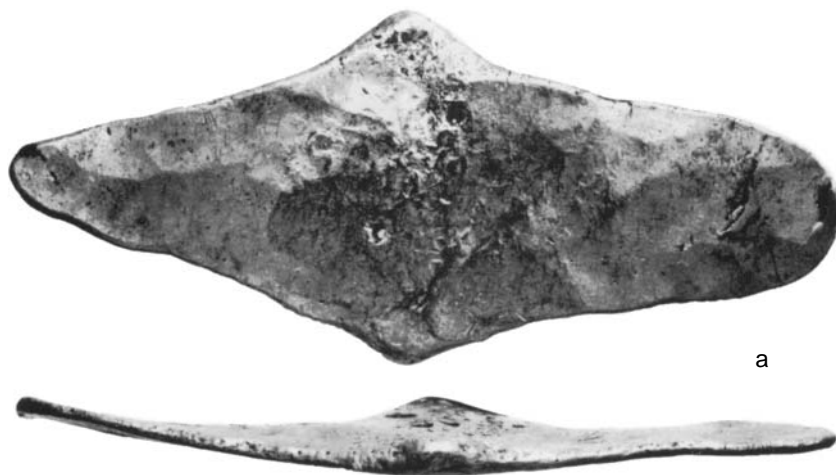


Fig. 2. Rus'ian monetary ingots (reproduced 1:1).

- a. Chernihiv type. Burge (Gotland, Sweden) hoard (Kotljars, cited n. 61, p. 119)
- b. Kiev (regular) type. H. Ivakin's excavations at Saint-Michael the Golden Domes Cathedral in Kiev (courtesy H. Ivakin)
- c. Novgorod type. E. Toropova's excavation at Staraya Russa (web posting).

⁵⁹ These ingots are little known outside the territory of ancient Rus', cf. P. Spufford, *How rarely did medieval merchants use coin?* (Van Gelder-lezingen 5) (Utrecht, 2008), 7, 10.



b



c



type were cast in Lithuania. Nikolaj P. Bauer was the first, to my knowledge, to affirm that the weight of the “*grivna*” ingots should reveal the weight of an earlier *grivna*, since concordant sources equate at least one type of ingots to four “old” *grivny*.⁶⁰ Bauer attributed to this old *grivna* 49.25 g and believed that it had emerged in the eleventh century as an equivalent of 50 West European pennies (cf. below), Yanin considered it to be the second-oldest Rus’ian *grivna* of 51 g from the second half of the tenth century, for Nazarenko, this was the very oldest Rus’ian *grivna* going back to the ninth century (see above), but despite these divergences, Bauer’s observation became the cornerstone for all future research in the field. The fact, however, that the four Rus’ian *grivna* types conform to two distinct ponderal standards complicates the matters considerably.

The latest survey of “*grivna*” ingots belongs to Nikolaj F. Kotljar.⁶¹ Kotljar’s study was prompted by the discovery, in the midst of a huge hoard from the 1140’s, unearthed in Burge (Gotland, Sweden), of 28 whole and 6 fragmentary Rus’ian ingots of the type traditionally designated as “Chernihiv.” Kotljar, following Bauer, contested the attribution of this ingot type to the principality of Chernihiv.⁶² He suggested renaming it “North-Rus’ian”, but the recent discovery of a hoard of such ingots in Chernihiv argues for retaining the traditional name.⁶³ Chernihiv ingots have a distinctive oblate form, yet they are less refined in execution than the other types; they carry traces of hammer blows, the simplest way to effect a rough assay of the silver alloy. Kotljar weighted the ingots from the hoard as well as the Chernihiv-type ingots conserved in the State Hermitage (Saint Petersburg) and the Historical Museum (Moscow), thus reaching a sample of nearly two thirds out of about 120 recorded ingots of this type. The average weight of the 28 whole ingots in the hoard is 196.42 g, in the State Hermitage (31 ex.) 195.74 g, and in the Historical Museum (15 ex.) 195.56 g. Kotljar tentatively suggested for this ingot type the “ponderal standard” of 196.72 g.⁶⁴

Kotljar’s appraisal came very close to the old estimate by Bauer, who indicated for these “flattened out (расплющенные)” ingots the average weight of ca. 197 g, which he considered identical to the ponderal norm of the much more common oblong ingots of the Novgorod type weighing on the average just over 197 g. Kotljar quotes Bauer, yet claims that Bauer was wrong, the average weight of the Novgorod ingots being ca. 204 g.⁶⁵ Likewise, the useful comparative table of ingot types provided by Kotljar (p. 87) indicates, for the “North-Rus’ian” (i.e. Chernihiv)

60 Бауер, “Денежный счет” (cited n. 48), 223ff.

61 Н.Ф. Котляр, “Северорусские («черниговские») монетные гривны,” *Древнейшие государства Восточной Европы. 1994. Новое в нумизматике* (Moscow, 1996), 80–142.

62 Котляр, *ibid.*, 98–99; Бауер, “Денежный счет” (cited n. 48), 225.

63 А. Казаков, Г. Жаров, “Клад гривен из Чернигова,” *Нумизматика*, 2001/2, 48–49.

64 Котляр, *ibid.*, 100–101.

65 Котляр, *ibid.*, 100; Бауер, “Денежный счет” (cited n. 48), 225.

ingots, the average weight of ca. 196 g, and for the Novgorod ingots, of ca. 204 g. Yet, Kotljar's position contains a paradox, which is highly revealing of the confused state of the question. Bauer, the leading specialist in Rus'ian numismatics before his execution by the NKVD in 1942, made no mistake regarding the average weight of the Novgorod ingots, which he personally studied at the State Hermitage. Their real weight, however, has been thoroughly obfuscated by the purely notional figure of 204.756 g, which has been introduced by V. L. Yanin. Removing this ghost figure from the debate is a crucial precondition for resolving the *grivna* question.

Yanin claimed that the ponderal value of the Novgorod *grivna*-ingots was not determined by their actual weight — for which he accepted Bauer's figure — but by the weight of the silver provided for their casting. Ca. 204 g of scrap silver would have suffered a loss (yrap/ burned off) of ca. 7 g in the melting process, yet the reference value of the ingot would still be 204.756 g.⁶⁶ Divided by four, this figure supplied the ponderal value of the ancient *grivna*, 51.19 g, which (as pointed out above) emerged, according to Yanin, in the second half of the 10th century, and according to Nazarenko, a century earlier, and which is at the heart of their respective reconstructions of the Rus'ian monetary system. This figure is currently very popular among historians, as it allows attaching both the ingot and the ancient *grivna* to the Carolingian pound of 409.562 g, a crucial element in Nazarenko's construction. Yet, there is no point in exploring this "link". In a short but seminal study, Ivar Leimus has pointed out that Nazarenko's system of arguments, however ingenious, fails to convincingly demonstrate the origin of the "traditional" *grivna* of 51 g.⁶⁷ As Oleksiy Tolochko has recently observed, Yanin's assumption integrating the burned off in the nominal weight of the ingot has no parallel in numismatics and cannot be sustained.⁶⁸ The actual average weight of the Novgorod *grivna*-ingot divided by four would suggest an "old" *grivna* of slightly over 49 g, which was the figure retained by Bauer. In later layers of *Pravda Rus'kaya*, the *grivna* is subdivided into 50 *kuny* or *rezany*, and so Bauer hypothesized that 49.25 g represented the weight of 50 silver pennies (*denarii*) of ca. 1 g circulating in Novgorod in the eleventh century.⁶⁹ Leimus pointed out, however, that the average indicated by Bauer understated the weight of Western silver coins, which had actually circulated in Novgorod area.⁷⁰ Thus, Bauer's hypothesis does not lead us far.

Bauer, followed by Kotljar, explained the ponderal standard of 197 g for the ingots of "Chernihiv" and Novgorod type as reproducing the Scandinavian *marca*

66 Янин, *Денежно-весовые системы* (cited n. 48), 46.

67 И. Леймус, "Русский денежный счёт XII века — свой или заимствованный?," *Монета*, 8 (Vologda, 2001), 23–30, see p. 24.

68 А. Толочко, "О новгородской гривне серебра," *Ruthenica* 6, 2007, 359–365, see pp. 360–361.

69 Бауер, "Денежный счёт" (cited n. 48), 225ff.

70 Леймус, "Русский денежный счёт" (cited n. 67), 26.

of 8 öre (calculated at ca. 24.5 g).⁷¹ This explanation was revised and developed by Leimus and by Tolochko (who was not aware of Leimus' study). As pointed out by Leimus, ingots of ca. 197 g appear in hoards on the island of Gotland, as well as in Latvia and Estonia, as of the second half of the eleventh century. Then, both scholars insist on the fact that when references to a silver *grivna*, manifestly the Novgorod-type ingot, start appearing in Novgorod's treaties with Gotland and its other trading partners from the late twelfth century on, this means of payment is perceived as common to all contracting parties. They conclude that if the ponderal standard of the Novgorod *grivna* conformed to that of a Scandinavian *marca*, there is no reason to believe that by dividing it by four we would obtain the ponderal value of the ancient Rus'ian *grivna*. Last but not least, they point out that the idea itself of issuing *grivna*-ingots of quadruple weight and value as compared to *grivny* of coins goes back to Scandinavia and specifically to Gotland.⁷²

These valuable observations can be completed with another, also fairly recent and originating this time with V.L. Yanin himself. Unlike Bauer, who believed that Chernihiv-type ingots preceded those of Novgorod, which only emerged in the early thirteenth century,⁷³ Yanin considered the Novgorod type to be earlier in dating its appearance to the eleventh century.⁷⁴ This view has been retained in later publications.⁷⁵ However, in commenting on a newly discovered birch bark letter from the early thirteenth century that mentions a debt of a *grivena novaja* (n° 713), Yanin noted that this "new *grivna*" only emerged in the late twelfth century.⁷⁶ This remark must refer to Novgorod-type ingots, and in the absence of any recent survey of their finds, this personal testimony by the best-informed specialist in the field is precious

71 Бауер, "Денежный счет" (cited n. 48), 227–228; Котляр, "Северорусские" (cited n. 61), 102.

72 Леймус, "Русский денежный счёт" (cited n. 67), 24–25; Толочко, "О новгородской гривне" (cited n. 68), 62. I am very grateful to Dr. Leimus for a consultation (by mail) on the ingots weighing close to 197 g, found in the Baltic region, which he briefly mentions in n. 3. The most significant find is the Rijneki hoard of 18 ingots; if we exclude two aberrant ingots of 103 and of 159.9 g, the remaining 16 weigh 200.33 g on the average (between 189 and 209.6 g), see V. Urtans, *Senākie depozīti Latvijā (līdz 1200. g.)* (Riga, 1977), 66–67 (n°63). Otherwise, relevant finds are extremely rare. Thus, the Baltic trading partners of Novgorod were unlikely to dispose, like their Rus'ian counterparts, of standardized "monetary" ingots; they probably had to pay their fines in irregular ingots or in coins.

73 Н.П. Бауер, Денежный счёт в духовной новгородца Климента и денежное обращение Северо-Запада Руси в XIII в., *Проблемы источниковедения*, III (Moscow-Leningrad, 1940), 175–203, see p. 202.

74 Янин, *Денежно-весовые системы* (cited n. 48), 161–162.

75 Notably Назаренко, *Древняя Русь* (cited n. 32), 120.

76 В.Л. Янин, А. А. Зализняк. *Новгородские грамоты на бересте (из раскопок 1990–1996 годов)* (Moscow, 2000), 13; А. А. Зализняк, *Древненовгородский диалект*, 2nd edition (Moscow, 2004), 426. Meanwhile, Yanin seems to have given up on the ponderal standard of 204 g as well. In a study from 1979, he described the monetary system (кунная система) of the thirteenth-century Novgorod as "based on real ingots of silver with the norm of 196.2 g" (В. Л. Янин, "К истории формирования новгородской денежной системы XV в.," *Вспомогательные исторические дисциплины* 11, 1979, 251–259, see p. 251). It is regretful that the recent reprint of Yanin's studies on the monetary history of Rus', published over half a century (В. Л. Янин, *Денежно-весовые системы домонгольской Руси и очерки истории денежной системы средневекового Новгорода*, Moscow, 2009), provides no guidance to the reader on the evolution of the author's views on the topics discussed.

and its implications are crucial. The propensity to set the emergence of *grivna*-ingots as early as the eleventh century goes back to the early twentieth-century historians, and while its support in the archaeological evidence is meager, its logic is clear. The circulation of dirhams ceased in Southern Rus' in the late tenth century; only in Northern Rus' were the dirhams replaced by imported western coinage, but its circulation declined and nearly ceased in turn in the early twelfth century. Thus, Rus' entered the so-called "coinless" (безмонетный) period, which lasted until the second half of the fourteenth century. The old chronology of the Novgorod-type ingots allowed attaching them directly to the last phase of the monetary circulation in the region. Their new dating makes it clear that they actually appeared after well over half a century without (or with very little) coins. I will argue below that this gap is essential for explaining the ponderal value of the new *grivna*.

My observations so far take stock of the progress achieved in the field of Rus'ian monetary history since the publication of the synthetic studies by Yanin and Nazarenko. The chronology of Chernihiv- and Novgorod-type ingots has been clarified, the former emerging in Southern Rus' no later than the 1130's and the latter, in Northern Rus', hardly earlier than the 1180's. Both ingot types adhere to a single Scandinavian weight standard. Thus, the attempts to reach back to the ancient *grivna* through the intermediary of the Novgorod-type ingots arbitrarily evaluated at ca. 204 g lead to a dead end. I would define both Chernihiv- and Novgorod-type ingots as export currency, ingots cast to facilitate trade with Scandinavia.

The third type of Rus'ian ingots, the "heavy Kiev-type" renamed "pseudo-Kiev-type" by Kotljар who contests their link to Kiev,⁷⁷ belong in the same category. By far the least common, with only about 50 ingots on record, this type has the distinctive shape of Kiev-type ingots (cf. below) and the average weight of ca. 197 g,⁷⁸ but is attached by scholars to the ponderal standard of "204 g";⁷⁹ it is dated to the first half of the thirteenth century.

These preliminary remarks may leave the reader with the impression that my whole aim is to bring him back to Bauer's positions, but this is only partially true. Bauer firmly believed that Kievan Rus' had borrowed its metrological system from the Scandinavians and focalized, therefore, on Chernihiv- and Novgorod-type ingots, which he attached to Scandinavian metrology. Yanin, as opposed to Bauer, sought to break this dependence on Scandinavia, but focalized, nevertheless, on the same Novgorod-type ingots; his attempt to attach them to a different ponderal standard was not a success. Both approaches, however, have left consciously in the shade the so-called "Kiev-type" ingots, distinguished by their hexagonal shape. This situation is at the very least surprising. While Kotljар knew of about

77 Котляр, "Северорусские" (cited n. 61), 87–88.

78 <Бауер>, Романов, "Деньги" (cited n. 31), 395.

79 М.П. Сотникова, "Серебряный слиток с Изяславля-городища," *Культура средневековой Руси* (Leningrad, 1974), 64–66; Котляр, "Северорусские" (cited n. 61), 87.

120 Chernihiv-type ingots in all (cf. above) and an old, probably exaggerated, estimate of Novgorod-type *grivny* from the early thirteenth century and before put their number at 118,⁸⁰ the number of known Kiev-type ingots indicated by Kotljar (p. 87) is over 400. It is admitted by all that the production of such ingots ceased after the Mongol destruction of Southern Rus' in the late 1230's. Thus, the Kiev-type ingot, the Cinderella of the Rus'ian monetary history, was actually by far the dominant "monetary ingot" in pre-Mongol Rus'. Would it not be more likely for this mass product of Kievan Rus' to reflect the ponderal value of the ancient *grivna*?

The emergence of the Kiev-type ingots is variously dated to the eleventh or the twelfth century. Curiously enough, one and the same ingot discovered in an "undisturbed archaeological context" in Novgorod is cited as evidence both for the former⁸¹ and the latter⁸² date. In admitting a compromise dating very early in the twelfth century, I should insist on the fact that Kiev- and Chernihiv-type ingots emerge more or less simultaneously.

In the context of this study, the most striking feature of Kiev-type ingots is their weight. In Kotljar's (p. 87) table, as in many other studies, it is indicated as ca. 164 g, which is, again, a notional figure based on Yanin's (p. 54) claim that the nominal weight of a Kiev-type *grivna* was 163.73 g, a quarter of a Byzantine pound. The average weight of such ingots was calculated by Bauer as 158–159 g, but his own histogram shows that if one excludes a few aberrant specimens under 150 g, his sample of 376 ingots displays an impressive cohesion of weight within the range of 156–165 g,⁸³ with effective average at ca. 161 g. As I have pointed out above, recent studies suggest a slightly lower weight standard for the middle Byzantine pound than the classical estimate of 327.46 g for the Roman pound that served Yanin as reference. Thus, the actual average weight of Kiev-type ingots is still very close to half a pound, but this is not the main point. The weight of a Kiev-type ingot represents exactly four times the ponderal value of the ancient *grivna* as calculated above on the basis of the treaties of 911 and 944 in conjunction with the *Short Pravda*.

The idea of dividing the weight of a Kiev-type *grivna* by four has occurred to scholars before. In a survey of the Rus'ian monetary system published nearly thirty years after his monograph, Yanin asserts that in Southern Rus' the *grivna* acquired a distinctive ponderal value of $(163.73 : 4 =) 40.92$ g, as opposed to 51.19 g in the North.⁸⁴ Yanin's figures are admitted by Pritsak, who adds elaborate tables of ponderal values of the *grivna*'s subdivisions (*nogata*, *kuna*, etc.) in the South and

80 Янин, *Денежно-весовые системы* (cited n. 48), 46.

81 Янин, *ibid.*, 171; В. М. Потин, "Причины прекращения притока западноевропейских монет на Русь в XII в.," *Международные связи России до XVII в.* (Moscow, 1961), 84–115, see p. 107.

82 Сотникова, "Серебряный слиток" (cited n. 79), 64.

83 See Янин, *Денежно-весовые системы* (cited n. 48), 53.

84 В.Л. Янин, "Русские денежные системы IX–XV вв.," *Древняя Русь. Город, замок, село* (Археология СССР) (Moscow, 1985), 364–375, see p. 365.

the North respectively, based on the *grivna*'s presumed weight in each region.⁸⁵ Nazarenko proposes a complex system of his own, also based on the assumption that the “monetary and ponderal system” in Southern Rus’ was “completely different” from that in the North.⁸⁶ Many more studies integrate the idea of two distinct zones of monetary values, Kievan South and Novgorodian North, an idea going back to Yanin and Bauer (and possibly farther back).⁸⁷ Without going into unnecessary detail (and without denying regional varieties in the Rusi’an monetary system), I propose to reject this binary concept *en bloc* for two reasons, which I consider decisive.

One reason is particularly apparent to the readers of this article: if one takes into account the Chernihiv-type ingots, Rus’ cannot be conveniently divided in two zones. Unsurprisingly, these ingots are not discussed by Yanin (1956), Pritsak (1998) or Nazarenko (2001), and are also conspicuously absent from Yanin’s table of *grivny*.⁸⁸ Separating Chernihiv from Kiev and attaching it to the “northern” zone would be absurd. Therefore, Bauer, in half a sentence, and later Kotljar strived to dissociate the problematic “*grivna*” type from Chernihiv.⁸⁹ Rather than proposing another urban centre as an alternative site of production, Kotljar suggested that rural artisans could have fabricated the ingots “in the vast region from Pskov land and the Northern Dvina to the Middle Volga.” However, such a pattern of decentralized production is implausible and the proposed geographical setting is invalidated by Kotljar’s own map of finds. The only major find to the east of Pskov, a hoard of 44 ingots, was discovered near Velikie Luki on the Lovat’, on the main river route from Kiev and Chernihiv to Novgorod and Scandinavia. Thus, it can in no way contradict the southern origin of Chernihiv-type ingots. The four other finds are of seven ingots in all. By way of contrast, Kotljar indicates three hoards of 36 ingots in all from the region of Chernihiv and a hoard of 11 ingots found near Kiev; these are now completed by a significant find from Chernihiv itself (above).⁹⁰

Against this background, I would not follow Kotljar (p. 98) in describing the find near Kiev as “accidental.” I would also not consider at all surprising that ingots destined for foreign trade started being cast in Chernihiv, the economic powerhouse of Kievan Rus’ at the time. Lighter ingots based on the ponderal standard of the Rus’ian *grivna* (regular Kiev-type) were cast at the same time in Kiev. Since no Chernihiv-type ingots have been discovered in hoards associated with Mongol

85 Pritsak, *The Origins* (cited n. 47), 52–55.

86 Назаренко, *Древняя Русь* (cited n. 32), 194–195.

87 On the specificity on the Southern zone, see the important study by Kotljar: М. Ф. Котляр, *Грошовий обіг на території України доби феодалізму* (Kiev, 1971).

88 Янин, “Русские денежные системы” (cited n. 84), 373, tab. 152.

89 Бауер, “Денежный счет” (cited n. 48), 225; Котляр, “Северорусские” (cited n. 61), 98–99.

90 A major hoard including 32 Chernihiv-type ingots and 19 irregular ingots, so-called Kama silver pancakes (which also accompanied Chernihiv-type ingots in the Gotland hoard), was discovered in May 2002 by treasure hunters. It is only known from an announcement on the web (www.grivnaklad.narod.ru), which does not localize the find.

destruction, one may hypothesize that by the early thirteenth century they were replaced as export currency by the heavy Kiev-type ingots.

Throughout the major part of the twelfth century, Chernihiv- and Kiev-type “monetary” ingots were the only ones cast in all of Rus’. Since their respective areas of circulation mostly coincided, they cannot be related to two distinct currency zones. The only plausible distinction between the ponderal standards they represent is functional. The Chernihiv ingots were destined for northern trade, while the Kiev-type ones were cast for internal circulation. I should strongly emphasize that for neither one of these ingot types is there any indication that it was designated in the sources as “*grivna*”. The first ingots to “inherit” the name of the old Rus’ian *grivna* were those cast in Novgorod.

The second reason why the division of Rus’ into two currency zones is unsustainable is that not a single source as much as hints to its existence. Having in Novgorod and Kiev *grivny* distinct in weight and value would have created a huge inconvenience not only in conducting actual commercial transactions, but also in describing any kind of transaction involving both cities. In the famous episode (cf. above) of prince Jaroslav remunerating in the winter 1015/6, in Kiev, the Novgorodians who had conquered for him his father’s throne, it would have been essential to specify whether he paid them in Kiev *grivny* of ca. 40 g or in Novgorod *grivny*, 25% more valuable. In 1130, prince Mstislav of Kiev made a donation, together with his son Vsevolod, to Jur’ev Monastery in Novgorod.⁹¹ In the donation charter, which happens to be the oldest preserved Rus’ian document (other than birch bark letters), the princes use the term *grivna* twice, indicating the amount of annual taxes transferred to the monastery, that is, as a monetary unit, and indicating the weight of a silver plate donated, that is, as a weight unit. In this charter, issued in Kiev and destined for Novgorod, there is no hint that in each city the *grivna* could have a distinct value. The birch bark letters are even more eloquent. Two among the most ancient letters preserved, both dated to the mid-eleventh century, deal with *grivna* debts contracted outside Novgorod. In letter n° 246, which originated according to Andrej A. Zaliznjak in Smolensk or Polotsk, a certain Zhirovit claims from his Novgorod debtor Stojan four and a half *grivny*; he does not specify the *grivna* standard. In letter n° 915, the writer, possibly a Novgorodian, reminds his Novgorodian addressee of the *grivna* of silver he took in Kiev from the writer’s “lad” (*otorok*) and claims “the money” (*kouny*) back. It does not occur to the writer to qualify this *grivna* of silver, even though the transaction involves both Kiev and Novgorod.⁹² The lack of any indication in the sources that *grivna* could differ in silver content from one city to another condemns definitively the theory of distinct currency zones.

91 A.A. Гиппиус, “Загадки Мстиславовой грамоты,” in Ф. Б. Успенский (ed.), *Miscellanea Slavica: сборник статей к 70-летию Б. А. Успенского* (Moscow, 2008), 109–129.

92 Зализняк, *Древненовгородский диалект* (cited n. 76), A.29 (p. 282) and A.5 (p. 243), respectively.

Thus, from the late tenth/early eleventh to the late twelfth/early thirteenth century, the numerous attestations of the term *grivna* present no ambiguity. Before the introduction of ingots in Novgorod — still designated as “new *grivna*” in the 1220’s (cf. above) —, all references to *grivna* indicate ca 40 g of silver, eventually of gold, or the equivalent of ca 40 g of silver in furs. The various ways of expressing the value of *grivna* in furs is a complicated subject, on which I will make only a few brief remarks in the Appendix. However, the knowledge that there was one single *grivna* valued at ca 40 g of silver opens the way for a new appraisal of the ancient Rus’ian economy.

Eleventh- and twelfth-century figures relating to payments in silver and gold have been collected several times, notably by Bauer and Romanov in the volume dedicated to the material culture of pre-Mongol Rus’; the actual value of the amounts involved was, however, openly presented as a mere guess: “If we admit as the *grivna*’s weight not one pound [of ca 409 g, CZ], as do some, and not one tenth of a pound, as do others, but one third of a pound (as calculated by Prozorovski), we obtain...”.⁹³ The advantage of Prozorovski’s *grivna* of 136 g (cf. above) over the other estimations is not explained, and *grivna* values in the rest of the chapter are not translated in grams. Kotljar (p. 85), who believes that the term “*grivna* of silver” could designate an ingot as early as the 1140’s, admits even higher values. Thus, he values the fine of 1400 *grivny* paid by a minor prince, Volodimerko of Galich, to prince Vsevolod of Kiev in 1144 at “230 to 280 kg of silver, depending on the type, Kiev or Novgorod, of the monetary *grivny*.” Following my reasoning, the fine, described as heavy (много заплативъ) in the chronicle,⁹⁴ consisted of mere 56 kg of silver. With a *grivna* at 40 g we can evaluate precisely the various donations to the Caves Monastery in Kiev, expressed in hundreds of *grivny* of silver and/or in tens of *grivny* of gold.⁹⁵ We can also visualize the fine gesture of the young Volodimer Monomakh (in the late 1070’s), who gratified his father Vsevolod at dinner with a present of 300 *grivny* of gold (PVL, p. 159): they would make up a handy bag of 12 kg, probably filled with Byzantine gold coins and gold ingots of irregular weight often found in Rus’ian hoards.⁹⁶ 3000 *grivny* of silver and 300 *grivny* of gold, which are close in value, are the highest “monetary” figures that we find mentioned in

93 <Бауер>, Романов, “Деньги” (cited n. 31), 377.

94 *Ипатьевская летопись*, изд. А. А. Шахматова (Полное собрание русских летописей, 2) (Sankt-Petersburg 1908, reprint Moscow 1998), col. 316.

95 <Бауер>, Романов, “Деньги” (cited n. 31), 380.

96 While Rus’ entertained close political and trade contacts with Byzantium, it found little use for Byzantine gold coins, fairly rarely found in hoards and very rarely mentioned in the sources (as златьникъ/золотъникъ, златъница/златица). Rather than by export controls (which could never be efficient), I would explain this phenomenon by the rapid decline in weight and alloy of the Byzantine gold coins starting from the later tenth century. Thus, the *nomisma*, still the stable “dollar of the Middle Ages” at the time of the treaties, could not serve as reference for the Rus’ian economy during the formative eleventh century.

the sources. It is worth noting that the great Russian historian V.O. Kljuchevskij, the most widely read authority in the last century, belonged to those scholars, left unnamed in the quote above, who valued the *grivna* at one pound. The *grivna* value that I propose divides the “monetary” wealth of the ancient Rus’ by the factor of 10 as compared to his assessments.

These calculations bring us back to the problem of value of the Hebrew *zaquq* in the Kiev Letter. Identifying it with the pound of 12 ounces would set the debt inherited by Mar Jacob at well over 30 kg of silver (above), while equating it with the *grivna* would value the debt at about 4 kg of silver, the equivalent of ca. 1400 dirhams or, in particular in a period so close to the treaties with Byzantium, of 100 *solidi*. I believe that the Letter provides a crucial indication in support of the latter evaluation. It should be pointed out that the amount of debt was divided in two parts: the Jewish community of Kiev paid the creditors 60 *zequqim* and Mar Jacob was sent out with the task to collect the remaining 40 (lines 15-16). Since the Jewish community did not stand surety either for the initial loan or for the repayment of the remainder, this arrangement could have, for both sides, only one possible basis: Jacob’s sale’s value. In other words, the creditors could not expect to obtain for Jacob’s liberation and the Jewish community would not have paid for it much more than the price he would have fetched as a slave. If we value the 60 *zequqim* at 12 or even at 8 ounces of silver, the resulting amount is many times over the attested slave prices. The tenth-eleventh-century standard price in a case of a mass redemption stood at 100 *nomismata* (or dinars) for three captifs.⁹⁷ In the case of a personal redemption, sentimental considerations could bring the price up, but not entirely out of proportion with the market prices. I believe, therefore, that identifying the *zaquq* of the Letter as an Ashkenazi *zaquq* would make very little sense, while equating it with the *grivna* is economically defensible.

An old debate opposes scholars who consider the ninth-tenth-century hoards as witnesses of the monetary circulation within Rus’ to those who view them as, essentially, traders’ capital formed outside the Rus’ian territory and destined for transit to Scandinavia unless stopped by an unforeseen calamity. While it is not my aim to reopen this debate here, I note that the transaction mentioned in the Letter, bearing on many hundreds of dirhams – substantially more than the contents of nearly any preserved hoard – that could be borrowed and eventually repaid in Kiev, presumably in the 950’s-960’s, is our earliest secure evidence for “internal” monetary circulation in ancient Rus’.

97 Y. Rotman, *Byzantine slavery and the Mediterranean world* (Cambridge, Mass, 2009), 199–200.

Epilogue. *Grivna*, *dinar* and *nomisma*, and the open realm of the ninth-tenth centuries trade

Attempts to equate Rus'ian *grivna* and Byzantine *nomisma* go back to the origins of modern Russian monetary history of Rus' and to its founding fathers Iohann Philipp Krug and Dmitrij I. Prozorovskij. Thus, the latter observed that a Rus'ian synod assembled by metropolitan Cyril II in 1273 or 1274 had established the fee for appointing a deacon or a priest at 7 *grivny* in an obvious reference to a Byzantine ruling from ca. 1058 that set the fee at 7 *solidi*.⁹⁸ This fairly valid observation was accompanied by metrological considerations so obsolete that they need not be quoted. The Byzantine connection of the *grivna* was abandoned by the early twentieth century, when the tendency to search for its Oriental (cf. below), Scandinavian or Slavic roots prevailed.

Aleksandr Nazarenko has revived the issue in a study, often referred to above, which was first published as an article and then, in a revised form, as a chapter in a monograph.⁹⁹ He related anew the *grivna* to the Byzantine gold coin in describing it as a “*nomisma* of account” (счетный солид).¹⁰⁰ Far from neglecting the heritage of twentieth-century historiography and from denying the Oriental connection of the *grivna*, however, he pertinently defined it as “the worth of a gold coin [*dinar* or *nomisma*, CZ] expressed in silver”.¹⁰¹ Nazarenko's major aim was, in fact, to demonstrate that the Rus'ian *grivna* constituted the equivalent in silver of the Byzantine *nomisma* and the Islamic *dinar* alike. I cannot acknowledge more strongly the crucial novelty of this thesis, which I share in a somewhat modified form. If I try to support the same idea with evidence of my own, however, this is because I can retain no element of Nazarenko's demonstration based on the *grivna* value of 51 g. The reader would not expect me to take a different position after describing this *grivna* as a phantom. Since I have argued at length for an alternative *grivna* on the basis of Byzantine evidence, I will make this part of the exchange short and dwell more on the Oriental evidence.

In dealing with Byzantine metrological and monetary data, Nazarenko brings in notions and makes assumptions that I find highly contestable. One such notion is the “provincial Byzantine *nomisma*”, supposedly slightly lighter than the gold coin circulating in the capital and thus closer in weight to the *dinar*. This

98 Д.И. Прозоровский, *Монета и вес в России до конца XVIII столетия* (Saint-Petersburg, 1865), 169–173.

99 А.В. Назаренко, “Происхождение древнерусского денежно-весового счета,” *Древнейшие государства Восточной Европы. 1994. Новое в нумизматике* (Moscow, 1996), 5–79; Назаренко, *Древняя Русь* (cited n. 32), 113–218.

100 Назаренко, *Древняя Русь* (cited n. 32), 149.

101 Назаренко, “Происхождение” (cited n. 99), 30–33. My further references are limited to the monograph and provided in the text.

“provincial” *nomisma*, which is said to have circulated in the sixth-ninth centuries (pp. 144–148), is, in fact, an amalgamation of the light *nomisma*, which was struck in Constantinople in the sixth-seventh centuries, and of the *nomisma*, which can be more appropriately described as provincial, since it was struck and only circulated in Byzantine Italy in the eighth-ninth centuries. The two coin series, however, had nothing in common. The light *nomismata* introduced by Justinian¹⁰² had a steady weight and gold content; these issues, duly marked as light, could have no impact on the metrology of the *grivna*, with which they did not coexist in time. As for the gold coins struck in Byzantine Italy, their ponderal indications, which acquire a great significance for Nazarenko as a potential reference for the *grivna*, are basically irrelevant since the coins’ weight was so unstable (rarely reaching 4 g) and gold content so deficient¹⁰³ that they could not serve as reference for any metrological system. After aligning the standard *grivna* on the “provincial” *nomisma*, Nazarenko (p. 149) is forced to introduce a heavier “Constantinople variant of the *grivna*” weighing 54.58 g. Since neither *grivna* would fit with the data of the treaties, the author (p. 183–188) also introduces into Byzantine metrology a silver *litra*, alias “great *litra*” of 392.94 g, hitherto unknown to Byzantinologists; one half of this *litra* (196.47 g) is presented as the *grivna* of the *Pravda Rus’kaya* and the treaties. I spare the reader further revolutions of the *grivna*, based on metrological assumptions so daring that they are difficult to follow.

I believe, for my part, that the Byzantine evidence, as exposed above, is fairly straightforward, yet I would not say the same for the Oriental evidence, which is equally crucial for our topic. Here the problem lies less with Nazarenko’s original arguments than with his basic premise regarding the exchange rate of a gold dinar in silver dirhams, which is admitted by many scholars. Without being an Arabist, I will do my best to expose the state of the question, which, regretfully, lacks clarity in the scholarly literature.

One of the sub-divisions of *grivna* was *nogata*. The term *nogata* has been etymologized from the Arabic word *naqd*, designating a full-value silver dirham coin. An earlier etymology, from *noga* (leg), presenting the original *nogata* as a skin with paws (which, in Slavonic as in English, have never been called legs), was appropriately ridiculed by Bauer. The later part of the *Short Pravda* together with the *Long Pravda* and other evidence show conclusively that one *grivna* contained 20 *nogaty*.¹⁰⁴ Likewise, some Arabic sources equate a gold dinar with 20 silver dirhams (cf. below). Admittedly, this could not be a coincidence. Considering the original *grivna* as the silver equivalent of a dinar valued at 20 dirhams would be an obvious conclusion, except that, as pointed out by Nazarenko (pp. 114, 121), the

102 I have studied the emergence of this coinage in C. Zuckerman, *Du village à l’Empire: autour du Registre fiscal d’Aphroditô (525/526)* (Paris, 2005), 87–91.

103 Grierson, *Catalogue*, III, 1 (cited n. 57), 24–28

104 Бауер, “Денежный счет” (cited n. 48), 196–202, 212–213.

weight of 20 dinars, $2.97 \text{ g} \times 20 = 59.4 \text{ g}$, was hard to reconcile with any ponderal data that could be related to the Rus'ian *grivna*. Yanin and then Nazarenko made attempts to integrate the ponderal value of 59.4 g into their respective concepts of *grivna*, the latter by admitting an "Arabic" variant of the *grivna* with a distinct weight.¹⁰⁵ I find their arguments exceedingly complex and speculative, but what undermines them even more is the fact that the exchange rate at their base seems to me irrelevant.

Some modern authorities still cite the exchange rate of 20 dirhams to a dinar as an axiom, requiring no demonstration. This is notably the case of Ronald Findlay and Kevin H. O'Rourke, *Power and Plenty: Trade, War and the World Economy in the Second Millennium*, pp. 48–49, who indicate this rate for the entire period of classical Islam (I quote this textbook, published by Princeton University Press in 2007, as a random recent example). In vol. I of the first edition of the *Encyclopædia of Islam* (published by Brill in 1913), p. 979, the entry "dirham" by E. von Zambaur contained the explicit statement that "In the early days of Islam the relation of gold to silver was fixed at 14:1 (20 dirhams = 1 dinar)." When the first modern "Islamic" currency was created in Iraq in 1923, the dinar was divided into 20 dirhams. Among the students of medieval economic history, this rate was popularized by the influential studies of Sture Bolin (1953) and in particular Philip Grierson (1960).¹⁰⁶ No wonder that the notion of a dinar composed of 20 dirhams was taken by Yanin and Nazarenko for granted.

When, however, Claude Cahen was invited, in 1964, to provide an Arabist's comment on the post-Pirenne models of East-West trade produced by Bolin and Grierson (on which see below), he did not conceal his embarrassment regarding their monetary aspect. He judged the only testimony for the exchange rate of 20 dirhams to a dinar quoted by Grierson to be late and unreliable.¹⁰⁷ In vol. II of the second edition of the *Encyclopædia of Islam* (published by Brill in 1965), G.C. Miles, the author of entries "dinar" and "dirham", carefully avoided the question of the exchange rate between the two currencies. In 1969, Eliyahu Ashtor's comprehensive survey of medieval Islamic economic data featured one reference to a dinar valued at 20 or 22 dirhams in early-ninth-century Iraq.¹⁰⁸ In raising the issue again fifteen years later, Cahen took a more conciliatory position. He recalled his previous misgivings, yet conceded: "That said, it seems all the same that in the second and the third centuries of the Hijra, the *dirham* and the *dinar* conforming to the Law were both worth from 1 to 20, i.e., the *dirham* weighing .7 of the *dinar*,

105 See Назаренко, *Древняя Русь* (cited n. 32), 149ff, with a criticism of Yanin.

106 S. Bolin, "Mohammad, Charlemagne and Ruric," *Scandinavian Economic History Review*, 1, 1953, 5–39; Ph. Grierson, "The monetary reforms of 'Abd al-Malik: their metrological basis and their financial repercussions," *Journal of Economic and Social History of the Orient*, 3, 1960, 241–264.

107 Cl. Cahen, "Quelques problèmes concernant l'expansion économique musulmane au Haut Moyen Âge," *L'Occidente e l'Islam nell'alto medioevo (Spoleto, 2–8 aprile 1964)* (Settimane di studio del Centro italiano di studi sull'alto medioevo 12), Spoleto, 1965, 391–432, see pp. 396–397.

108 E. Ashtor, *Histoire des prix et des salaires dans l'Orient médiéval* (Paris, 1969), 40.

and there was a gold to silver ratio of 14”.¹⁰⁹ Unfortunately, he provided no source references for the exchange rate that he now admitted as plausible.

The best such evidence, absent from the debate, appears in the eleventh-century Book of Gifts and Rarities (*Kitab al-Hadaya wa al-Tuhaf*), first published in 1959 and now available in an English translation. It states that “When al-Muntasir bi-Allah died, the total sum of his private treasury was a million dirhams. He changed the gold coins (*al-‘ayn*) to silver coins (*wariqan*) because the rate was, at that time, twenty dirhams to one dinar.”¹¹⁰ The author clearly implies that the caliph al-Muntasir profited from a very favorable exchange rate, and the exchange he operated could be the first recorded case of insider trading since, as we shall see, this rate was about to change. What is more, the operation described can be dated with great precision: al-Muntasir ruled for only six months in 861-862. The evidence, ignored by the Arabists, of Rus’ian *grivna* divided into 20 *nogaty* acquires in this context a special significance. At an early stage in their commercial contacts with the Caliphate, Rus’ian traders must have picked up the notion of equating a dinar to 20 silver coins. It is possible that the custom of making a necklace — this is the meaning of the word *grivna* — of 20 dirhams emerged at this early stage.¹¹¹ This custom might explain why the Rus’ retained this division into twenty long after it lost all relevance in the Islamic monetary system itself.

Better evidence on the exchange rate of dinar in dirhams becomes available from the late ninth century, when the bimetallism was generalized on the scale of the Caliphate (previously one metal, gold or silver, prevailed in the monetary circulation of each region). As pointed out by Cahen, an official exchange rate needed to be established at this period for fiscal and other purposes. This rate, calculated at 14 2/7 dirhams to a dinar (based on the gold-silver ratio of 1:10), remained valid through most of the tenth century.¹¹² Thus, in the late ninth — early tenth century, the period of formation of the Rus’ian *grivna*, the dinar was officially valued not at 59.4 g of silver, as Yanin and Nazarenko believed, but at 42 g.

Different, and much more abundant data is provided by the documents from the Genizah of Cairo. They attest, not official norms, but to the real exchanges practiced from the late tenth to the thirteenth century, and there are few documents, in which the exchange rate is the same. Shelomo D. Goitein, who collected and commented this precious evidence, emphasized that there could be no stable ratio between gold and silver coins since both were, in the first place, commodities valued according to

109 C. Cahen, “Commercial relations between the Near East and Western Europe from the VIIth to the XIth century,” in Kh. I. Semaan (ed.), *Islam and the Medieval West: Aspects of intercultural relations* (Albany, 1980), 1–25, see p. 14.

110 *Book of Gifts and Rarities* (*Kitab al-Hadaya wa al-Tuhaf*), translated and annotated by Gh. al-Hijjāwī al-Qaddūmī (Harvard, 1996), 209, §306.

111 Much has been written on *grivna*-necklace, cf. Байер, “Денежный счет” (cited n. 48), 208, but I see no need to engage here in this fairly speculative topic.

112 Cahen, “Commercial relations” (cited n. 109), 14.

their quality (alloy and weight) and availability on the market. The value of silver coins was particularly unstable, the transactions being conducted in debased dirhams. Goitein points out, however, that in many cases the exchange rate of such dirhams reveals the underlying assumption that “1 gold dinar is worth 13 1/3 pieces of full silver”, a ratio intermittently attested for over two hundred years.¹¹³ At this rate, a dinar would be worth 39.6 g of silver in good dirhams of the kind that reached Rus’.

Unlike in Byzantium, where the official exchange rate slightly overvalued the silver currency, silver would seem to have been slightly undervalued by the official exchange rate in the Caliphate. In both cases, however, the market correction was small and, most remarkably, resulted in bringing the two rates closer to one another, to the point of making them, for all practical purposes, identical. This leveling of the exchange rates, which has not been noticed by modern scholars, did not escape the Rus’ian merchants.

The Rus’ian grivna of ca. 40 g emerged after the official exchange rate in the Caliphate changed from 20 to 14 2/7 dirhams to a dinar, that is, after the reign of al-Muntasir bi-Allah in the early 860’s and probably not before the late ninth century (see above). The old exchange rate is perpetuated in the division of *grivna* into 20 *nogaty*, but it is not reflected in its weight. This inconsistency can be explained by the discontinuity of the ninth-century Rus’ian history, which I have once described as “two stages” in the emergence of the Rus’ian state. From the early 830’s to ca. 870, several sources mention Rus’ headed by a *qagan*, which trades and occasionally fights with Byzantium and the Caliphate and which corresponds to the archaeological traces left by Scandinavians in North-Eastern Russia, mostly in the Novgorod and Ladoga region. Then, traces of massive destruction on the sites associated with Scandinavian presence attest to a major upheaval, the supply of Oriental silver to Scandinavia practically grinds to a halt, and signs of a new start can only be seen, in Ladoga and at Rjurikovo Gorodisce, ca 890, while new settlements, such as Gnezdovo (Smolensk) and Shestovica (Chernihiv), are created ca. 900. Numerous tenth-century sources, Byzantine and Oriental, which refer to the Rus’, never designate their chief as *qagan*. I have related this upheaval to the story in the chronicles, going back to the ancient Narration (*Skazanie*), of the expulsion and recall of the “Varangians”. Some Russian scholars deny the historicity of this narrative, other reduce the duration of the Varangians’ exile to a few months (following the chronology of PVL, which they admit to be late and artificial), but actually, as I hope to have shown, the Scandinavian presence in Russia had been interrupted for nearly twenty years.¹¹⁴

113 S.D. Goitein, *A Mediterranean society: The Jewish communities of the Arab world as portrayed in the documents of the Cairo Geniza*, vol. I: *Economic foundations* (Berkeley — Los Angeles, 1967), 368–392 (Appendix D on the exchange rates), see p. 390 for the quote.

114 C. Zuckerman, “Deux étapes de la formation de l’ancien État russe,” in M. Kazanski, A. Nersessian, C. Zuckerman (eds.) *Les centres proto-urbains russes entre Scandinavie, Byzance et Orient* (Réalités byzantines 7) (Paris, 2000), 95–120; cf. К. Цукерман, “Два этапа формирования Древнерусского государства,” *Археология* 2003/1, 76–99, and the same text, slightly abridged, in *Славяноведение* 2001/4, 55–77.

When Rus'ian merchants resumed their trade activities both in Byzantium and the Caliphate, they must have soon observed that the *nomisma* and the dinar, nearly equal in weight, were now also nearly equal in their silver value. Their preferred currency being silver, they devised a unit of account in silver that bridged both markets.

Thus, I believe that the Rus'ian *grivna*, which emerged ca. 900, acquired from the start a fixed ponderal value of ca. 40 g, which remained unchanged until the late twelfth — early thirteenth century when a distinct “new *grivna*” started circulating under this name in Novgorod. This value could easily find its place in the Scandinavian ponderal system as a quintuple of its basic unit, *ertog*, estimated at ca. 8 g. In this system, the *nogata* of ca. 2 g (1/20 of a *grivna*) was a highly convenient sub-division. It is important to notice, however, that Unn Pedersen, in her recent survey of Scandinavian ninth-century weight finds (focused on but not limited to Kaupang), draws attention to the absence, in Scandinavia, of weights of ca. 40 g.¹¹⁵ By way of contrast, weights of ca. 40 g are common among those discovered in tenth-twelfth-centuries Rus',¹¹⁶ otherwise very similar in composition to the Scandinavian finds. The *grivna* of ca. 40 g was a Rus'ian addition to the Scandinavian ponderal system.

Influential studies by Bolin and Grierson introduced the notion of strong disparities in the comparative value of gold and silver between the Merovingian and Carolingian West, the Byzantine Empire, and the Caliphate. I could quote at random a dozen of recent studies carrying speculations on the flow of gold and silver one way or another, depending on where the exchange rate was presumed to be more favorable for each metal. Thus, most recently, in speaking of “monetary realities” of the early medieval economy, Gene W. Heck names as “chief among them . . . a sharp disequilibrium in the gold to silver exchange rate amongst the various proximate commercial regions — a differential that also could have substantially contributed to the disappearance of gold in the Christian West. For the gold silver ratio, it appears from various medieval sources, was then 1:12 in Carolingia; 1:14 within the *Dar al-Islam* itself; and 1:17 in Byzantium.” In such conditions, the merchants become specialists in “currency arbitrage.”¹¹⁷ Peter Spufford muses on the consequences of gold being plentiful and cheap in Byzantium (and, therefore, escaping to the West),¹¹⁸ while Diana Wood reflects, on the contrary, on gold moving to Byzantium and Byzantium losing its silver.¹¹⁹ Such speculations are dangerous without firm evidence and a clearly defined chronological framework. The seventh-early eighth

115 U. Pedersen, *Weights and Balances*, in D. Skre (ed.), *Means of Exchange. Dealing with Silver in the Viking Age* (Kaupang Excavation Project Publication Series 2, Norske Oldfunn 23) (Oslo, 2008), 119–195, see p. 144 (and the tables at the end).

116 See Назаренко, *Древняя Русь* (cited n. 32), 161–163.

117 G.W. Heck, “First century Islamic currency: mastering the message from the money,” in J. Haldon (ed.), *Money, power and politics in early Islamic Syria* (Ashgate, 2010), 97–123, see p. 118 for the quote.

118 P. Spufford, *Money and its use in Medieval Europe* (Cambridge, 1988), 51.

119 D. Wood, *Medieval economic thought* (Cambridge, 2002), 128–129.

century ratios are still fairly obscure,¹²⁰ but when the evidence becomes more plentiful and the Mediterranean trade gains a new momentum, the ratios between precious metals in the different parts of the Mediterranean show a tendency to level out. I should specially emphasize the fact that when, in the late ninth-early tenth century, the gold:silver ratios in Byzantium and the Caliphate reach equilibrium, they meet at the level (of 1:10) traditionally admitted for the Western Europe.

The Rus'ian *grivna*, created by traders active in all three markets, embodies and exemplifies this new equilibrium.

Appendix. The *kuna*

The etymology and the original meaning of the old Rus'ian word *kuna* are debated. Some scholars maintain “that this word – like Old Frisian *cona* (in *skilling cona*) – goes back to the late Latin *cuneus* (> Old French, *coing* > Old English, *coin*) meaning ‘wedge, design stamped on a coiner’s die, coin’”. This would suggest that the word *kuna* was a Scandinavian import in Slavonic.¹²¹ Others stick to the view that the word was Slavonic in origin and always designated a small furry animal, most commonly a marten with which it is exclusively associated in later Russian usage.¹²² The Slavonic etymology of *kuna* is supported with appropriate Baltic parallels yet faces the objection that the word is absent from the written corpus of Old Slavonic and that its attestations in other Slavic languages are fairly late. Since I can claim no competence in this linguistic debate, I see no better way to grasp the meaning of the word than by arraying its actual attestations in chronological order.

In the oldest layer of the chronicle, which I refer back to the Narration of 1016/7, the word *kuna* is used once with the qualifier “black” (черна куна), and in this combination it means incontestably “black marten.” This usage originally belongs in the description of prince Igor’s conquest of the Drevljane tribe in the early 940’s. Victorious Igor and his general Sveneld raised from the Drevljane the tribute of one black marten per hearth (“имаша по чернѣ кунѣ от дыма”) (N1Chr. jun., 109). The author of the *Povest’ vremennyh let* (ca. 1115) deemed it more fitting to attribute the conquest of the Drevljane to Igor’s predecessor, Oleg: “имаша на нихъ дань по чернѣ кунѣ.”¹²³ In this version of the story, the Drevljane rebel after Oleg’s death against their lawful master, Igor, who increases upon his victory

120 Thus, in an unjustly neglected study, Jean Durlat, “La valeur relative de l’or, de l’argent et du cuivre dans l’empire protobyzantin (IV^e–VIII^e siècle),” *Revue numismatique* 6^e série, 22, 1980, 138–154, argued convincingly that Grierson had underestimated the value of silver in the seventh-century Byzantium.¹²¹ Pritsak, *The Origins* (cited n. 47), 40–41.

122 *Этимологический словарь славянских языков*, 13 (Moscow, 1987), 162–164.

123 *Лаврентьевская летопись*, вып. I: *Повесть временных лет*, изд. Е. Ф. Карского (Leningrad, 1926), col. 24; cf. А. А. Шахматов, “О Начальном Киевском летописном своде,” first published in 1897, reprinted in: А. А. Шахматов, *История русского летописания*, I,2 (Saint-Petersburg, 2003), 31–70, see p. 66 (Shakhmatov’s view of the transformation of the text is more complex than mine).

Oleg's original tribute. Yet in transposing the phrase into a different context, the author committed two blunders. Thus, he kept the plural *имаша* (preserved in the Lavrent'evskaja recension, the other recensions of the *Povest'* correct giving the singular *имаше*), which is aberrant in the context of the *Povest'*, where the subject of the sentence is Oleg alone.¹²⁴ What is more, in placing the description of the tribute at the beginning of Oleg's reign, the author carelessly omitted to specify the tax base, the hearth ("дымъ"). Taking this secondary version of the text as primary evidence, Janet Martin transforms a hearth tax into a poll tax: "Oleg, as prince of Kiev 'imposed... a tribute of one black marten skin per man' on the Drevliane (a Slavic tribe dwelling northwest of Kiev). After the Drevliane rebelled, Prince Igor reasserted Kievan authority over them and in 914 'imposed a tribute larger than Oleg's upon them'."¹²⁵ Incidentally, this revision of the tax base, in making every single Drevljanian liable for a black marten, makes the fur-producing capacity of the Rus'ian land go through the ceiling.

Twice the word *kuny* (pl.) is employed in the Narration without any qualification. When Volodimer was faced, ca. 980, with the ultimatum of his Varangian allies to ransom each dweller of Kiev for 2 *grivny* (cf. above), he asked for a one-month reprieve, so that *kuny* could be gathered: "пождите, оже вы куны сберуть, за мѣсяць" (N1Chr. jun., 128). *Kuny* can only be translated in this context as "money", or more specifically "coins." Another passage from the same ancient layer of the chronicle is equally explicit. Late in his reign, Volodimer, now a devoted Christian, extended his charity to every beggar and wretch, who could come to the princely court and be provided with all he needed: food and drink, "и от скотницъ кунами" (and with money from the treasury) (NFChr. jun., 166).

The Chronicle of 1076 adds one independent mention of *kuny*, which is quite revealing. It describes the spoliation of prince Izjaslav's treasury in 1068 by the rebellious people of Kiev, who "took countless multitude of gold as well as of silver in *kuny* and in furs (и кунами и скорою)" (N1Chr. jun., 189). Following Yanin against Nazarenko,¹²⁶ I admit that *kuny* are opposed here to furs and can only mean (silver) coins. The chronicler of 1068, in describing a recent event, clearly conceived of the furs as an equivalent of silver coins: one could pillage wealth (described as "gold" and "silver") in the form of either coins or furs. It is quite instructive that when the abbot Sil'vestr copied the *Povest'* in 1116, he mechanically replaced "скорую," a generic term for furs, by "бѣлью" (as in the Lavrent'evskaja and

124 The same phenomenon, with only the Lavrent'evskaja recension preserving the original unadapted verbal form in a revised context, has been observed by A. A. Shakhmatov in the interpolated part of the description of Olga's revenge against the Drevljane, see A. A. Шахматов, "Киевский Начальный свод 1095 г.," first published posthumously in 1947, reprinted *ibid.*, 428–464, see p. 432.

125 J. Martin, *Treasure of the Land of Darkness: The Fur Trade and Its Significance for Medieval Russia* (Cambridge, 1986), 9.

126 Янин, *Денежно-весовые системы* (cited n. 48), 114; Назаренко, *Древняя Русь* (cited n. 32), 151.

Radzivilovskaja/ Akademicheskaja recensions of the *Povest'*), squirrel furs, the specific furs that were used as coin equivalent in his time and, possibly, already half a century beforehand.¹²⁷

The two remaining mentions of *kuny* in the *Povest'* belong in the description of the infightings that broke out after Volodimer's death. They do not appear in the original account going back to the Narration, preserved in the N1Chr. jun., but rather, as I have recently argued,¹²⁸ in parts rewritten or written in the 1090's. The first passage is not very telling. In N1Chr. jun. (p. 169), Svjatopolk, after he seized the throne upon Volodimer's death, is described as distributing property to the people of Kiev ("нача даяти имъние имъ"). The same scene is more elaborately rephrased in the *Povest'*: Svjatopolk, having gathered people, bestowed (upper) clothes on some and *kuny* on others, and distributed plenty ("созвавъ люди, нача даяти овъмъ корзна, а другымъ кунами, и раздая множество"). There is no indication in the text regarding the means of payment, coins or furs, distributed by Svjatopolk.¹²⁹ The second passage, in the *Povest'* entry for AM 6526 (AD 1018), concerns the voluntary collect of money by the Novgorodians in order to recruit Varangian mercenaries on behalf of Jaroslav, who had sought refuge in Novgorod after the conquest of Kiev by Svjatopolk and his Polish ally Boleslav. Each citizen contributed 4 *kuny*, each village head gave 10 *grivny*, and each nobleman (*bojarin*), 18 or 80 *grivny*. I will not discuss here the historicity of the story as a whole or the divergence between manuscripts regarding the latter figure. What matters is that this is the first datable use of *kuna* designating not a skin or a coin in general, but a specific value unit, obviously a subdivision of *grivna*. It is possible that the context of this use was not chosen at random.

Specialists in numismatics have long debated prince Jaroslav's emission of the so-called "small-sized" silver. In the latest catalogue, Marina P. Sotnikova lists five whole coins of this emission weighing 1.57, 1.55, 1.49, 1.37 and 1.34 g (and a fragment of 1.18 g). She maintains with new arguments the view that these coins were struck in Novgorod to pay Jaroslav's Varangian mercenaries and that they were adapted, therefore, to the ponderal standards of "Scandinavian" *denarii*.¹³⁰

127 The Ipat'evskaja recension of the *Povest'* keeps the original reading скапою, which goes back to the Chronicle of 1068, see *Ипатьевская летопись* (cited n. 94), col. 161.

128 Цукерман, "Наблюдения" (cited n. 34), 252-257.

129 I cannot but wonder why N. F. Kotljар (Н. Ф. Котляр, Еще раз о "безмонетном" периоде денежного обращения древней Руси (XII-XIII вв.), *Вспомогательные исторические дисциплины* 5, 1973, 152-169, see p. 162) quotes this passage as an explicit example of use of the term *kuny* to designate coins (and, incidentally, as a proof that the eleventh and the twelfth centuries were not as "coinless" in Southern Rus' as most scholars believe).

130 М. П. Сотникова, *Древнейшие русские монеты X-XI веков. Каталог и исследование* (Moscow, 1995), 118-120, 211-215. The new text proposes quite a few modifications as compared to the first edition of the catalogue (see М. П. Сотникова, И. Г. Спасский, *Тысячелетие древнейших монет России. Сводный каталог русских монет X-XI веков* [Leningrad, 1983]), notably in removing one dubious item from this series.

The two heaviest coins must be the closest to the norm that Jaroslav had in mind; incidentally, their weight is very close to 1/25 of a *grivna*, ca. 1.6 g. The *kuna* in the Short Pravda is valued at 1/25 of a *grivna* (cf. above), and this cannot be a coincidence. This would suggest that the word *kuna* was only associated with a specific coin and with a specific fraction of *grivna* when heavy Western *denarii* started circulating in Northern Rus' in the late tenth century. Jaroslav's emission appears to be the only attempt to "materialize" *kuny* as coins. Yet, since the weight of the silver coins that continued to come to Northern Rus' was very uneven, the *kuna* was maintained, like *nogata*, as an "immaterial" ponderal sub-division of *grivna*.

Nearly 130 years after Jaroslav struck his "small-sized" silver coins, an Islamic scholar and merchant, Abū Hāmid al-Gharnāī, visited the country of the Slavs (Rus') and discovered with great surprise their monetary system:

Ils utilisent pour les échanges entre eux de vieilles peaux de petits-gris qui n'ont plus de poils, dont on ne peut plus tirer aucun profit et qui ne sont propres à rien. Quand la peau de la tête et [celle] des pattes de devant du petit-gris sont en bon état, dix-huit [animaux] font un dirham d'argent selon leur compte. Il les lient en ballots et le nomment "ğukn". Pour chacune de ces peaux, on a une excellente galette de pain qui suffit à un homme fort. Ils s'en servent pour acheter toutes sortes de biens dont des esclaves de deux sexes, de l'or, de l'argent et des castors. Dans n'importe quel pays, on n'achèterait pas avec mille charges de ces peaux un seul grain, et elles ne seraient bonnes à rien. Quant elles se gâtent dans leurs maisons, ils les portent pour les faire arranger, alors qu'elles tombent déjà en morceaux. Ils les amènent à un marché connu où se trouvent des gens en présence d'artisans; ils les leur confient. Les pelletiers les lient avec des fils solides. Avec dix-huit peaux, ils font un ballot. Ils mettent à l'extrémité du fil un morceau de plomb noir et ils y apposent un cachet à l'effigie du roi. Et de cette manière, ils marquent les peaux les unes après les autres. Il n'est pas possible de refuser une telle peau tant à l'achat qu'à la vente.¹³¹

The Russian translation adds an important detail, which is omitted from the French: the officials, who seal the bundles of squirrel skins, take a skin for each seal.¹³²

The testimony of Abū Hāmid al-Gharnāī on the circulation of "fur-money" in Rus' (ca. 1150) is one of many, but it is by far the most detailed and has the advantage of originating with an eyewitness, who exchanged his own dirhams for worn squirrel furs. This testimony became known too late to be used by

131 *De Grenade à Bagdad. La relation de voyage d'Abū Hāmid al-Gharnāī (1080-1168)*, traduction annotée de J.-Ch Ducène (Paris, 2006), 91.

132 *Путешествие Абу Хамида ал-Гарнати в Восточную и Центральную Европу (1131-1153 гг.)*, публ. О. Г. Большакова, А. Л. Монгайт (Moscow, 1971), 35-36.

Bauer and by Yanin (in his monograph). Since the publication of the text in a Russian translation, it has often been quoted but, to the best of my knowledge, never integrated with the main body of evidence on the “monetary” circulation in twelfth-thirteenth century Rus’, mostly drawn from the *Pravda Rus’kaya*. I will limit myself here to a few short explanatory remarks, and then dwell more on one point, which is particularly relevant for the present study.

Al-Gharnāṭī’s *ḡukn* can only be the *kuna*, as suggested by the Spanish editor of the Arabic text, César E. Dubler, and as admitted, with some hesitation but no alternative proposal, by the Russian editors.¹³³ A *ḡukn* could be exchanged for a silver coin. This means that whatever debased dirhams al-Gharnāṭī brought along from Volga Bulgaria (which did strike its own coins at the time), their silver content was evaluated at ca. 1.6 g, a plausible figure. *Kuna-ḡukn* was composed of 18 squirrel skins, loose or tied in a bundle by the owner, with their heads and paws intact. When these skins got worn and started falling apart, they had to be trimmed and sown in a fixed bundle, officialized by the prince’s seal. This procedure explains the origin of the monetary term *rezana* (meaning “cut” or “trimmed”) as a short way of saying *rezana kuna*, a trimmed *kuna*.¹³⁴ As pointed out above, the name *kuna* is reserved in the *Short Pravda* for 1/25 of a *grivna*, while 1/50 of a *grivna* is named *rezana*; by way of contrast, in the *Long Pravda*, 1/50 of a *grivna* is designated as *kuna*, with no intermediate unit between it and the *grivna*. The change of name of 1/50 of a *grivna* from *rezana* to *kuna*, much debated but never explained, is easy to understand if we admit that a *rezana kuna* started being named simply *kuna* in regions, where there was no longer any other *kuna* in circulation. The obvious advantage of a *rezana (kuna)* over the old-style *kuna* consisted in the fact that the emission of the former was controlled by the prince and brought him profit, while the latter could be injected into the money supply by any hunter. A single worn-down squirrel skin can be identified as *veksha*, the smallest known denomination in the Rus’ian “fur-money” system. 18 such *vekshy* formed a sealed bundle, a *rezana* or a (*rezana*) *kuna*. This number fits with the only indication provided by the *Pravda Rus’kaya* regarding the number of *vekshi* in a *rezana*: they should be more than 12.¹³⁵

133 Большаков, Монгайт, *ibid.*, n. 99 to the text (pp. 73-74); on pp. 110-119, the reader will find an excellent survey of evidence on the “fur-money” in Rus’.

134 The great number of dirhams in the hoards that are either trimmed around or cut in fragments explains the current consensus that the word *rezana* originally described a cut or trimmed silver coin. A. I. Cherepnin and later V. L. Yanin invested much effort in the study of fragmentary dirhams, in an attempt to discover ponderal norms in their great diversity; cf. recently along this line: В. П. Лебедев, А. В. Зорин, “Денежное обращение на территории курских северян в IX-X вв.,” *Стены Европы в эпоху средневековья*, 7, 2009, 575-598. Such studies produced interesting observations, but not the results, at which they aimed. No ponderal norm could be discovered, either for *kuna* or for *rezana*, in the ninth-tenth-centuries numismatic material. I believe that neither of these terms was employed in that period to designate a specific coin, fragment of a coin, or a weight.

135 Cf. Бауер, “Денежный счет” (cited n. 48), 206, 215. Г. В. Семенченко, “Несколько дополнительных замечаний о денежном счете домонгольской Руси,” *Древнейшие государства Восточной Европы. 1991 год* (Moscow, 1994), 245-251, suggests 4 *vekshi* for a *rezana*, with arguments that I do not find convincing.

I should make it clear to the reader that in combining the data of al-Gharnāṭī and of *Pravda Rus'kaya*, I introduce the assumption that a sealed bundle of 18 worn and trimmed squirrel skins, in which I recognize a *rezana*, was worth half of the value of 18 better quality skins, my *kuna*. On this assumption, the “monetary” system witnessed by al-Gharnāṭī ca. 1150 fits the scheme of the *Short* rather than of the *Long Pravda*. For al-Gharnāṭī, one system applied in the whole of Rus', which he crossed from East to West.

From this short commentary I pass to my main point. The long period of “fur-money” circulation, during which the means of payment did not have an intrinsic value (like gold and silver) but only a conventional one, is essential for understanding the transition to the “new *grivna*” in Novgorod. When four old *grivny* (*grivny kun* or *grivny vetkhikh kun*) were recognized as equal in value to a silver ingot of ca. 197 g, the nominal silver value of an old *grivna* was enhanced by nearly 25%. This transition must have occurred in the early thirteenth century, and within two or three generations the old *grivna* disappeared entirely: as pointed out by Bauer, it was last mentioned, in Novgorod, in the testament of Kliment from the 1270's.¹³⁶ About the same time, in an entry dated to AM 6796 (AD 1288), the Galician-Volhynian Chronicle describes the last days of the Volhynian prince Volodimer Vasilkovich who, in order to facilitate the distribution of his wealth to charities, breaks down gold and silver ornaments and vessels and recasts them as *grivny* (*pol'ja v grivny*).¹³⁷ This is the only “Southern” source that I know, which applies the term *grivna* to ingots.*

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¹³⁶ Бауер, “Денежный счет в духовной Климента” (cited n. 73).

¹³⁷ *Ипатьевская летопись* (cited n. 94), col. 914.

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