The Riddle of the Ancient Indian Eras is Not Yet Solved

GÉRARD FUSSMAN

ABSTRACT

The chronology of northern India from the second century BC till the reigns of the Guptas rests mainly on dated inscriptions. Epigraphists have shown that many eras were successively, or concurrently, used but there is no agreement on their starting year nor on their relation with the famous Vikrama and Śaka eras. Three papers by eminent scholars have recently proposed solutions for the Greek (Yavana), Azes and Kanishka eras. They met with almost unanimous approval.

A detailed examination of these papers shows that these solutions are still highly hypothetical. Salomon's paper on the Yavana era uses as main evidence an inscription whose genuineness is not above doubt. The paper by Falk and Bennet on the Azes era rests on readings of a very difficult inscription and the unproved assumption that the Śakas used a Babylonian-Parthian calendar. The wonderful paper by Falk on the Kanishka era rests on an unclear astronomical text and the acceptance of the 'omitted hundreds' theory and the so-called Chinese evidence (i.e. year 1 of Kanishka should be later than AD 125 because Ban Yong's report, dated c. 125, does not name him) – both dubious assumptions.

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1. INTRODUCTION

When Gautam Sengupta and B. R. Mani asked me to write a paper for the new series of Ancient India, I felt very much honoured and readily agreed to send them a paper on a subject on which I was currently working, viz., a survey of our present knowledge about the beginnings of Gandhāran art and the creation of the anthropomorphic Buddha's images both in Mathura and Gandhāra. Needless to say it is a complex subject, not so much because of the huge bibliography that needs to be mastered before beginning to write even a sentence on it, but because one has to enter into the intricacies of the often new epigraphical and archaeological data, take into consideration the fresh light shed by the Gandhāri manuscripts on Buddhist beliefs at the beginning of the Common Era, and understand the foundations for the feelings of the many art historians who have dealt with this subject. That would be too long an article for this issue of Ancient India.

In any case, one has to start with a chronology, i.e. face the difficult problem of the origin of the Vikrama and the Śaka eras, and their relationship with Azes' and Kanishka's eras. Three important papers have been published on this subject in the last ten years by outstanding scholars (Falk 2001; Salomon 2005; and Falk-Bennet 2009). Many believe that the problem is now solved. I am afraid it is not and this paper will try to demonstrate why.

I would like to stress that the chronology of northwestern India (present-day Pakistan) and Mathura in the first centuries BC and AD is now much better known than it was fifty years ago. This is because of well conducted and published excavations (mainly in Sonkh); new inscriptions (the inscriptions of petty kings – rāja – of Odi and Apraca, and the Rubatak inscription written by Kanishka's dignitaries in his lifetime); the discovery of new coins and the study of the contents of hoards and overstrikes by numismatists. As this paper will demonstrate, the disagreement on the starting point of the Azes era is now approximately 10 years only. Year 1 of the Kanishka era will remain, for a long time, a subject of debate.

However, the hesitation about this is limited to AD 78 and c. AD 125 (127 according to Falk 2001). This does not make a great difference to the modern historian of ancient India, who is more interested in demography, ethnic composition of population, class and caste divides, agricultural and manufactured output, changes in urbanization and landscape, religious evolutions, etc., than in the precise dates of kings, about whom we know almost nothing, except – in the main - the order of their succession. In fact, we do not even know the true extent of the territories they governed. The scholars who are most interested in precise chronologies are the historians of art who need a chronological scale to retrace the evolution of early Indian sculpture. They may find this paper interesting although mainly negative. However, being true scholars, I suppose they will appreciate knowing which data are incontrovertible facts, which ones are only assumptions, and which hypotheses are sound.

The best way to test the weight of a hypothesis is to understand if it is simple or complex. The more complex a hypothesis, the less reliable it is. If thesis A is backed by sure data and one hypothesis (Hypothesis 1), it has a great degree of probability. If Hypothesis 1 itself is the
result of another hypothesis (Hypothesis 2), thesis A is more doubtful. If Hypothesis 2, in turn, depends on another hypothesis (Hypothesis 3), then thesis A is only a guess.

2. CALENDARS AND ERAS

Any agricultural community needs a calendar, i.e. a year divided into units (seasons and months or half months), with a fixed beginning and end. It is needed for determining the best or usual sowing-times and religious festivals. For obvious reasons, seasons differ according to the location of the country. Almost everywhere in the ancient world, months were lunations, i.e. the period of time between two successive new or full moons, i.e. 29 or 30 days. Given this, many possibilities were open: the beginning of the month may be the new moon (šukla<paksha>ādi or amānta) or the full moon (pūrṇimānta). There may be intercalary months,₁ as in Greece and India, or not, as in the Muslim calendar.

The way of intercalating months is not the same in every country and may change with time. The first day of the year also differs from place to place. It is often an astronomical event, e.g. the summer or winter solstice. It may be quite arbitrary, for instance, a king may decide to start a new era on the day (or the fortnight) of his coronation, a great victory, the birth of his son, etc. (Samuel 1972: 245-248).

Agricultural societies may dispense with an era, i.e. a computation of the years with a fixed starting point. An era, or any way to count the elapsed years, is needed mainly for administrative purposes. One may need to show a dated receipt of the taxes paid, or a dated chart of the tax exemption given to one’s family or community. In long-distance trade, one needs to keep track of the number of years elapsed since money was lent to a merchant, who may not return before two, three years or more; etc.

In western Asia, both for prestige and practical purposes, two main eras were successively in use. The Seleucid era had, as its starting point, the accession year 1 of Seleukos I, i.e. April 3, 311 BC in Babylon, but autumn of 312 BC according to the Macedonian calendar (Samuel 1972: 245). The Arsacid era started at the vernal equinox of 247 BC, for reasons unknown. The Arsacid era was less widely used than the Seleucid era (see below). The Vikrama and Śaka Indian eras are well known, but not attested under these names in early documents. Instead, we suppose the existence in the north-west of India of many eras. Three of them are sure and have a name (the Yavana era, the Azes era and the Kanishka era) but, as this paper will try to demonstrate, none has a definite starting point.

This is not surprising. We are far from knowing every intricacy of early Indian epigraphy or from mastering the Middle-Indian vocabulary it uses. For instance, nobody is able to explain the exact meaning of the often used and seemingly synonymic formulae iše kshunami, etaye purvaye (etāye pūrvaye), atra divase, etc., or tell whether these three expressions are exactly equivalent. In the same way, there is no sure explanation of the use, in some inscriptions, of the Iranian sasta in lieu of the much more frequent Indian divasa, etc.
3. THE YAVANA ERA

The existence of an Indian era going back to the time of the Greek (yavana) conquest of northern India was suspected for a long time because some dated Kharoshthi inscriptions used an era whose precise starting point was unknown, but was obviously to be placed during the time when Greeks from Bactria governed northern India. The existence of this era was demonstrated when I published the undoubtedly genuine Maghera inscription, found in 1988, 17 km north of Mathura (Fussman 1993: 113-117; Mukherjee 1992). The Maghera inscription begins thus: yavana-rajaśya sōḍaś-uttare varsha-śate 100 + 10 + 6 hemata-māse 4 divasa 30 etāye purāye.... This translates to 'in hundred years with sixteen added of the Greek sovereignty 116, in month 4 of the winter season, in day 30, at that very moment...'

This is the last day of the month of Phālguna, i.e. the well-known chāturmāsi festival known as Holi. We do not know in which year the era started, but judging from its script, the Maghera inscription was engraved in the first century BC or the early first century AD. According to this, the starting point of the era is probably dated sometime in the second century BC. We do not know which Greek king instituted this era, nor the historical event to which it refers, not even if it is a Bactrian era used in the Indian dominions of the Greek invaders, or a Greek era used only in India.

In 2003, during the first international colloquium held after the end of the Taliban regime on Afghanistan archaeology and ancient history, R. Salomon presented a new and sensational Kharoshthi inscription engraved inside a reliquary. It is now published in a long paper that deals with the reliquary, the objects found within and the inscription (Salomon 2005). The reliquary is, no doubt, genuine and its detailed publication by R. Salomon is exemplary. The problem lies with the inscription. Here it is, as read and translated by R. Salomon (Salomon 2005: 367), only printed in a different way to make the comments easier to understand.

\[\begin{align*}
\text{vashaye satavişaye } & 20 + 4 + 1 + 1 + 1 \text{ isparasā Vijayamitrāsa Apacharajasa anuśastiye} \\
\text{ye vuchati Ayasa } & \text{vashaye trisatimae } 20 + 20 + 10 + 1 + 1 + 1 \\
\text{yoṣaṇa vashae ekadusatimaye } & 2 \times 100 + 1 \\
\text{śravāṇasa masasa divasaye aḥamaye iṣa divasaṃṇi} \\
\text{pratitiḥ avidu thuve} \\
\text{Rukhunye Apacharaja-bharyae} \\
\text{Vijayamitreṇa Apacharajena Imdravarmena strategena} \\
\text{sabharyerehi sakumarehi}
\end{align*}\]

In the twenty-seventh - 27 - year in the reign of Lord Vijayamitra, the King of Apacha
In the seventy-third - 73 - year which is called 'of Azes';
In the two hundred first - 201 - year of Yonas;
on the eighth day of the month Śrāvana; on this day
was established <this> stūpa
by Rukhunā, the wife of the King of Apacha,
<and> by Vijayamitra, the king of Apacha,
I immediately told R. Salomon that I had problems with the wording of that inscription. One should keep in mind the fact that there are many fake Gandhārān sculptures and Kharoshthī inscriptions. An inscribed reliquary fetches a much higher price than a plain one. Writing a Kharoshthī inscription, if its contents are simple, is not so difficult. The above reliquary, whose whereabouts are not known to me, appeared in the market c. 2002, at a time when, since 1978, many genuine inscriptions of unknown provenance of the kings of Apracha had been published and helped enforce the idea that the Azes era was identical to the Vikrama era, i.e. it began in 58 BC (see below).

This is the only Kharoshthī inscription engraved only inside the reliquary. There are some instances of inscriptions engraved inside reliquaries, but these always supplement earlier inscriptions engraved on the outer face, e.g. the Bajaur reliquary, whose inscription is partly fake (Falk 2005), and the Trashaka or Natrashaka inscription (see below). In both cases, the inscription engraved inside the reliquary either complements the outer one, or was added later when, for some reason, the reliquary was re-installed, with a new ceremony and usually a re-building of an earlier stūpa. In the reliquary mentioned above, there is no outer inscription. The outer surface of the reliquary is adorned with very simple decorative motifs. I do not see any other reason for not engraving the inscription in the usual place of honour, i.e. outside, than, if an error were made while engraving it, it would have been easier to erase everything and begin anew. That could also have been the forger's reasoning if he did not want to take the risk of damaging a genuine reliquary which could fetch a high price in the market.

This is also the only Kharoshthi inscription, probably also the only Indian inscription, with a triple date whose purpose is not obvious at all. There is, as pointed out by R. Salomon, another Kharoshthi inscription with a double date: the famous Takht-i-Bahai inscription from the time of Gondophernes' reign (Konow 1929: 57-63), undoubtedly authentic for discovered c. 1860. Its wording and purpose are obvious:

*Maharayasa Gudu ---- vharasa vasha 20 + 4 + 1 + 1
samba[tśara ti]sati tame 100 + 1 + 1 + 1 Vesākhasa masasa divase [pra]thame etc.*

In the year 26 of the King R' Gondo ---- phernes,6
in the 103rd year, on the first day of the month Vaiśākhā...

The inscription mentioned above was thus engraved in year 103 of an untold era, probably the Azes era (thus in c. AD 46, see below), in the twenty-sixth regnal year of Gondophernes. The double date makes sense. The way it is expressed also makes clear an opposition between *varsha*, i.e. regnal year, and *samvatsara*, i.e. calendric year. In the triple date we are examining, neither purpose nor wording is clear.

The first date uses a Sanskrit word (*anusāstīye = Sk. anusāsti*) in a quite unattested meaning, literally 'the ruling, the governing'. Till now, *anusāsti* was only known from Aśokan inscriptions and some Pali texts (*anusattī*), always with the meaning '<religious> instruction'. It can be
argued than the use of such a rare word with such an improbable meaning points to the genuineness of the inscription. I agree with that. Nevertheless, I would prefer to get one more occurrence of that very specific use.

The second date (ye vuchati Ayasa vashaye trisatimae) should be translated as: ‘which is called the year seventy-third – 73 – of Azes’. This is a very awkward expression: we should expect either nothing like in the Gondophernes inscription, or a word meaning ‘equivalent to’ not a sentence meaning ‘which is said’.

The third date (yoana vashaekadusatimaye) looks better, but indeed uses a western formula: an Indian would have probably written ‘in year 201 of the Greek Kings’ or ‘in year 201 of the Greek kingdom’, not ‘in the year 201 of the Greeks’, for that era was also used by Indians, as demonstrated by the Maghera inscription mentioned earlier in this paper. Of course, that double date is the central point of Salomon’s paper: 201 = BC 57 (Azes era) + 73 = AD 16. The starting point of the Greek era, so long searched after, would now be fixed in BC 136/185 (AD 16 less 201), provided it is proved beyond doubt that the inscription is genuine.

The remaining part of the inscription is also somewhat surprising. It states that ‘a stūpa was established’ (pratihavidu thuve), without making any mention of relics. A reliquary is meant for relics. Establishing relics, which are difficult to procure, is a more meritorious act than building a stūpa. We should have expected at least a stūpa and relics were established.’ The absence of any mention of relics is, to say the least, very strange.

The donors’ list also raises problems. The names were known from previously published inscriptions of the Apracha king, but the way they are listed is puzzling. Usually, the gift of relics, or the building of a stūpa, is made by one person (here it would be Rukhunā/Roxane) who gives back part of the merits thus gained to other partners. That would be expressed by saha or sāṛddham, ‘together with’, followed or preceded by a name in the instrumental case, or by a sa compound like at the end of the inscription (sa-bharyarehi sa-kumarehi). There is no saha nor sāṛddham in the inscription.

Or we could expect a list of donors on an equal footing, linked by cha, ‘and’. There is not one cha in the inscription. R. Salomon had to add them in his translation. That absence could be explained by lack of sufficient space. But how do we explain the fact that the wife of the Apracha king is named before her husband, especially if she is only one of the donors, not the main one? When husband and wife are named together, the husband’s name comes first. If Rukhunā was indeed the main donor, we would understand her name being inscribed before her husband’s name, but we would have also expected, as just explained above, a saha or sāṛddham construction!

Lastly, who are these ‘wives and royal princes’ (not ‘sons’) associated with the gift (sabharyarehi sakumarehi)? If we are to take the inscription at face value, that would mean that Rukhunā had wives and the sons of the strategē were ‘royal princes’. We could easily understand that last part of the dedication if the donors had been listed as Vijayamitra, Rukhunā, Indravarman. With Rukhunā being named first, the wording is, to say the least, very awkward.

I readily agree that many of the best known Kharoshṭhī inscriptions are still, entirely or partly, enigmatic. For instance, the Mathura lion capital (Konow 1929: 30-49) was a discarded
piece of sculpture that was later reused as material on which to practice his skill by a scribe (Fussman 2008: 709-710, despite Falk 2011). The Ara inscription (Konow 1929: 162-165), where there is no good explanation available of the not fully legible word \( [ka]{\text{[sara]}}sa \), or the Takht-i-Bahai inscription referred to in endnote 6 are other examples. These inscriptions are undoubtedly genuine. Their provenance is well known. The enigmatic three-dates inscription published by R. Salomon is without any known provenance. There are too many fakes now in the market to exclude the possibility of its being one of them, i.e. a fake inscription engraved inside a genuine reliquary. Nobody will ever be able to prove it. Nor will anybody be able to prove it is genuine. Nevertheless, all the data listed above make its genuineness doubtful, and a doubtful document is of no help in establishing a solid truth.

Although the discovery of a new and undoubtedly genuine inscription may prove, in the years to come, that the data given by the triple-dated inscription are correct, till that discovery, we should not use that document for any purpose. The starting point of the Yavana-rājya era is not yet known.

4. THE AZES ERA

The meaning of the word \( ayasa \), in the dates of some Kharoshṭhī inscriptions, was settled when two new inscriptions were published showing that \( ayasa \) meant 'of King Azes'. One was engraved on a reliquary (now in the Metropolitan Museum of Art in New York) given by Prince Indravarma of the Apracha dynasty 'in year 63 of past King Azes' (maharayasa \( ayasa \) atidasa). The other was engraved on a stone inscription, whose whereabouts are now unknown, recording the establishment of relics by Ramaka 'in year 74 of the great king Azes of former times' (maharayasa mahatasa \( ayasa \) purvakalisa). The palaeography of the inscriptions dates them to sometime in the first decades of the Common Era.

That evidence was reinforced by the fact that the stone inscription of Ramaka was found together with a reliquary of the same Ramaka, bearing no date (now in the Metropolitan Museum of Art) and a ceramic dish of rouletted ware, which also pointed towards a date close to the CE (Fussman 1980: 31). A simple calculation demonstrated that the starting point of the Azes era was so close to the starting point of the Vikrama era that both could be equated (Fussman 1980; Bivar 1981; Salomon 1982) and almost everybody agreed on this.

My long 1980 paper would be longer still if I had to publish it again. We now know many more male and female members of the Apracha family and have to face, in addition, the Azes/Gondophernes conundrum. Like everybody else, I believed in 1980 that there were two Azes and one Gondophernes. Since then, R. C. Senior has brought forward many numismatic data which support the existence of only one Azes era and two or more Gondophernes,7 and also contradict the wording of the two inscriptions referred to above.8 Nevertheless, I still stand to the main conclusion of the 1980 paper mentioned above: year 1 of the Azes era is so close to year 1 of Vikrama era that it is simpler to equate both, although there is no definite proof of that affirmation.
So I was not surprised when, in 2009, Harry Falk and Chris Bennett tried to establish the date of the Azes era in another way. Their starting point is a reliquary inscription I published long ago although I confessed not being sure of my deciphering and despite not understanding every word of it (Fussman 1985). There are, in fact, three inscriptions: one on the outer face of the lid (A), one on its inner face (B), one on the bottom (C). Here is the way it was transliterated and translated9

(A) 1 Hora
   2 sanvatsaraye sha-paṇḍhaśaśadama mase ire daasa 20 + 1 + 1 + 1 Trashake Hiphuaṣutra
   Muniṣa Trashaka-putra thuvaṃ
   3 pratiḥaveti apratiḥavita-pruve sarva-budhana pujae matra-pidu pujae / budhana koshi
      pravunama niashpiho /

(B) 4 ime Bhagavado śarira pratiḥapiṣa sava-budhana pujae Apabrukasa Hiliuphila-putrasa
   5 duasya [s]atrieṇa Trama Gupriya yanbulima masa saste 4 + 4

(C) 6 Avinavuliehi

   I pointed out that a number of mistakes were evident in the writing, e.g. the way of writing the date, koṣi for boṣi = bodhi, etc., and that there were many words I did not understand at all, e.g. the word niashpiho. I assumed that in year (56?), presumably of Azes, Trashaka and his son established a stūpa (A), and that some time later, in the same year, one Trama established the relics and completed the ceremony (B). I gave the following translation in my 1985 paper (at that time it was in French):

(A) 1 Gift
   2 In year (56?), month of Heriaos, day 24, Trashaka, Hiphua’s son <and> Muniṣa, Trashaka’s
      son, establish a stūpa
   3 In a place where there was no previous foundation, in honour of all Buddhas, in honour of
      their father and mother. Let us get the Buddhas’ bodhi. NIASHPIHO

(B) 4-5 These corporal relics of the Lord are established in honour of all Buddhas (by Trama,
      representative of the ambassador?) Apabruka, Heliophilos’ son
   5 intercalary Gorpiaios month, day 8

(C) 6 By the Avinavulia’ (meaning unknown)

H. Falk, in his paper published in 2009, tried to first solve the difficulties of the first line. With Salomon 1995: 130-131, he proposed to read the year as 156 and not 56, which looks better (Hypothesis 1); to understand ire as a loan word, viz. the Babylonian month Ayaru (Hypothesis 2); to read the number expressing the day as 23, which solves a problem that I pointed out, but entails the consequence that Trashaka, which looks like an Indian name (roots TRI or TRISH), becomes Natrashaka, of unknown etymology, and the problematic Muniṣa becomes Mumi. That has no bearing on the date.

Hypothesis 1 solves some of the problems I faced, without success, while editing the inscription. To make things short, I explained sha-paṇḍhaśaśadama as shat + paṇḍhašat + tama

with two unexplained (intrusive? faulty?) akṣhara: iṣa. Salomon (1995: 130-131) and Baums (2006:
37-38) pointed to the existence, unknown to me at that time, of a word *panchaisa = 50* and read shad-panchaisa-satama = 156, which entails no correction but leaves panchaisa without no sure etymology. Another problem with that date will be pointed out later in this paper.

Hypothesis 2 is a pure guess. Nobody knows what *ire* is. I suggested the Greek month Hērātōs, phonetically close to *ire*, but which is not a Macedonian month as would be expected. Falk prefers the Babylonian month Ayaru, which entails phonetic difficulties: if *aya > e* is not a problem, and if we can agree than this *e* may have been written as an *i*, how does one explain the disappearance of *u* in Ayaru? And are we not to expect that a Babylonian loan word should be exactly transcribed, i.e. written *'ayarue* and not follow the phonetic rules of a Sanskrit word evolving into a Gāndhārī one?

H. Falk’s other suggestions are of no relevance for the problem of the eras. One may agree with them or not. Then Falk tried to solve the enigma of line 5 (*duasya [s]atriena Trama*) and proposed a new reading, *vis.*, dua-satat-satama = dva-saptati-satama = the ordinal 172 (Hypothesis 3).

This entails many difficulties: the first *sa* of Falk looks like a clear *syā*, his *sa* is written in two parts, with a break in the middle of the upper line, contrary to the ordinary ductus of the Kharoshthī script, and the word for ‘year’ is omitted, which never happens. Moreover, it is hard to understand why they needed 16 years (172 less 156) to put relics in a reliquary ‘established’ in a previously built stūpa, where this same reliquary (the one inscribed with the name of Trashaka or Natrashaka) was already in place, but seemingly empty. When one puts a reliquary inside a stūpa, it should have been first filled with tiny relics and some offerings.

The translation I gave did not succeed in translating *duasya [s]atriena Trama* in a satisfactory way. But it easily explained the two inscriptions. Gandhāran stūpas have no aperture. When they are entirely built, nobody can take out the reliquary without first demolishing the whole building, except in exceptional cases. A Gandhāran stūpa is built on plain ground. The relic casket is enclosed in a big stone box which protects it. That stone box is either placed in the ground when the building process begins, or at the bottom of the *anda* (cupola) when the lower bases are already built. Trashaka (Natrashaka) gave the money for building the stūpa and had his name engraved on the empty reliquary. Then work, for some reason, had to stop for a few weeks or months, probably because they were waiting for an auspicious day or the coming of Heliophilos’ son or due to somebody's death, etc. When work resumed, there was a great ceremony with many monks and guests present. They engraved inscription B when putting the relics inside the casket, put the casket in place and completed the building of the stūpa. Sixteen years are not needed for this process.

Then comes C. Bennett, who in the second part of the same paper (Falk-Bennett 2009) tries to find in which calendar, corresponding to which era, a second Gorpiosios month was intercalated in year 172. I do not give the details of his research, many of which can be contested. I jump to his conclusion. According to him, 'on circumstantial (sic) grounds we might suppose that the Bactrian Greeks used the Seleucid calendar, though they certainly abandoned the Seleucid era. Equally, we might suppose, with RAPSON, that the Indo-Scythians, Indo-Parthians and the Kushans used the Parthian Macedonian calendar in light of their Parthian background and the close connections
between them' (Falk-Bennett 2009: 204-205) (Hypothesis 4). Following that hypothesis, a complex calculation shows that the Azes era would have started in spring of 47 BC or autumn of 48 BC and is not to be equated with the Vikrama era.

Rapson deserves respect, but he wrote before 1914. We may agree with the postulate that the Indo-Parthians (Gondophernes and others) used a Parthian calendar, but that is very doubtful for the Indo-Scythians (now called Šakas: Azes and others) and Kushans who fought against them. Moreover, the Arsacid era and calendar were not widely used even within the Parthian dominions; the Arsacid coins, when they bear a date, use Macedonian months and refer to the Seleucid era till at least as late as Orodes II (c. 57-58 BC) and Phraates IV (c. BC 38-2) (Callataj 1994: 27, with a long bibliography). It is quite unlikely that the Arsacid calendar and era were ever used in Gandhāra. Hypothesis 4 is a pure guess.

Last, but not least: if the dates written on that reliquary are 156 and 172, and if they refer to an era starting in 47 or 48 BC, the 156 date would be AD 108. If they refer to the 57/58 BC era, the 156 date would be AD 98. That does not fit with the writing of the inscriptions which, on palaeographical grounds, cannot be later than AD 50. If we stick to the readings of the dates by Falk, which may be true, the dates of the casket refer to an era which started at least 50 years before the Vikrama era, and which cannot be the era started by Azes, whose name in any case is not inscribed on the casket.

The 47 AD era remains a pure and difficult guess, depending on too many hypotheses and faulty reasoning. The problems raised by the Trashaka or Natrashaka inscription are not yet solved. For the time being, the only thing we can say is that the starting point of the Azes era, as deduced from inscriptions where the word Ayasa ensures us that it is used, is very close to year 1 of the Vikrama era. We cannot prove it, but if we use the Vikrama era for calculating the dates expressed in the Azes era, we shall not be far from an exact result. That approximation is no problem for the historian. And if we suppose that year 1 of Azes is the starting point of the Vikrama era, we solve, without being able to adduce any proof, the conundrum of the origin of that era. That will nevertheless remain a (sound) hypothesis.

5. YEAR 1 OF THE KANISHKA ERA

In 2001, Harry Falk published a paper which seemed to solve the enigma of year 1 of the Kanishka era. It developed, with slight changes, a thesis expressed earlier in a series of remarkable Berlin lectures edited by him (Falk 2002). The paper is well informed, well argued and rests on a very good knowledge of Sanskrit and Indian astronomy. This is why most people jumped to accept its conclusion, which fitted well the British assumption that year 1 of Kanishka should be placed c. AD 125 because of the Chinese evidence (see below). Almost everybody now agrees that year 1 of Kanishka should be placed in AD 127, as admittedly demonstrated by H. Falk.

I am not as good a Sanskritist as my friend Falk, I do not know anything about astronomy, be it Indian or contemporary, and I am at a loss when an Indian number is expressed by a noun used as a metaphor (akshī, ‘eye’ = 2 etc.). I shall nevertheless try to analyze Falk’s remarkable paper. The
starting point of Falk's demonstration are two stanzas from the last chapter of the Yavana-jātaka, or ‘Greek horoscope', edited, translated and commented upon in 1978 by the celebrated scholar David Pingree. It was completed in Śaka 191, i.e. AD 269/270 by an otherwise unknown rāja (who obviously was not a king) named Sphujidhvaja (stanza 79, 62), to expound in Sanskrit and in indravajrā stanzas (in fact in upajīti), i.e. in the language and style of an Indian śāstra, a Greek treatise on horoscopy written in Śaka 71 by a Greek astrologer, Yavanarāja.

That Yavanarāja would have himself translated his treatise from Greek into Sanskrit or Prakrit (stanza 79, 61), which is not a small achievement, and that Indian (prose?) translation would have been the source of Sphujidhvaja's śāstra. The difficulty of editing and understanding Sphujidhvaja arises from the fact that for most of the text, we have only one very incorrectly written manuscript to rely on. The errors... occur, on the average, at least once in every line' (Pingree 1978: Vol. 1, 22). That manuscript, written in northern Nāgarī, dates back to the early thirteenth century AD. The style is usually very simple: it is neither kāvyā* nor śūtra-style. The main difficulties are the technicalities related to the subject and the way it expresses numbers.

The last chapter (79) is a general exposition of the principles of mathematical astronomy. It begins thus: ‘The wise say that the observed course of planets is the supreme eye of the entire body of the rules of horoscopy. I shall explain it concisely according to the instructions of the Greeks' (79, 1). Then Sphujidhvaja explains what a solar yuga of 165 years is according to the Greeks, what are the positions of moon and sun on the day it begins, how long a tīthi is, how many days are in a yuga and in a sidereal month, etc. Then come the two crucial stanzas, which are the foundation of Falk's new calculation and which Pingree translated thus:

When 66 years of the Śakas have elapsed, that is the truth (i.e. foundation) of the calculation of time. At dawn on Sunday begin that year and the yuga of the Sun. (79, 14)

Take the number of years that have passed of the Koshānas, add 149, and subtract from this <sum> the time of the Śakas (i.e. the year in the Śaka era); the <remainder> is the number of years in the yuga which have elapsed (79, 15).


The two stanzas are commented upon at length by Pingree (1978: Vol. 2: 407-408). Stanza 14 relates the beginning of a yuga with the Śaka era in a very obscure way (Pingree 1978). The number, as given in the manuscript (shadagre'ardhaśate), could be read either as 56 (one half-hundred and six) or 156 (one hundred and half and six). Pingree had to find out whether the relative positions of sun and moon in 156 + 78 or 56 + 78 corresponded with the beginning of a yuga as defined by Sphujidhvaja. They did not fit. So, he corrected the text, printed shad-eke'ardhaśate (one half-hundred and sixteen or one hundred and half and sixteen) to get 66 or 166. In Śaka 66 only = AD 144 (66 + 78) the position of the moon and sun was as it should be at the beginning a new yuga, as defined by Sphujidhvaja.

Falk rightly pointed out that shad-eka (6-1) would be an unexpected way of writing 16 and went back to the text as written in the original manuscript. He translated the first part of stanza 79, 14 as: ‘When 56 years <of the yuga> have gone, this is the state of <the sky leading> to the
epoch of the Śakas’ (p. 124). That will date the beginning of the yuga in AD 22 (78 less 55), a
date compatible with the astronomical data as checked by Falk.

That is a sound hypothesis as far as I can see, but it remains a hypothesis because Falk had
to add important words (those indicated above in angular brackets), choose for gate a suitable
meaning and prefer for ardhāsate the easier translation 50, not the more difficult one 150. Falk
does not ask nor does he answer an important question: why did Sphujidhvaja, writing in Śaka
191 (AD 269/270) choose the year AD 22 as the starting point of the yuga, and not 165 + 22
= AD 187, viz., the beginning of the yuga he was living in? The answer is obviously that he was
following, without any changes, the chapter as written in Śaka 71 (AD 149) by Yavanarāja, i.e.
during the yuga whose year 1 started in AD 22. Hence, there is no solid objection to the transla-
tion as given by Falk and we may adopt it as quasi-fact.

For stanza 15, Pingree could use his corrupt manuscript and a quotation of the stanza by
Utpala on Brīhajjātaka 7, 9. None of these texts, according to him, made sense, so he translated a
text that he had corrected. I print his emendations in bold, i.e. the readings which are not
present in the manuscript nor in the commentary by Utpala. I make clear the disposition in pada-s.

\[
\text{Gatena sādhyardha-śatena yuktā}
\]
\[
vyekeṇa koshāṇa-gatā-saṃkhyā \}
\]
\[
kālaṃ śākāṇāṃ pariśodhya tasmād
\]
\[
attavarśāḥ yugavarshajātāḥ \}
\]

Both emendations are slight: kālaṃ for kālaḥ in 15c, jātāḥ for jātaṃ in Utpala (the manuscript spelt
it as yātaṃ, i.e. the usual pronunciation of jātaṃ) in 15d. That meant mainly the replacement of a
visarga (h) written by two dots after the akṣhara by an anusvāra (m) written by one dot above the
akṣhara in pada c and the reverse in pada d. These changes were necessary to keep the syntactic link
between kālaṃ and pariśodhya in the same pada, as usual in simple Sanskrit stanzas, and because
Pingree needed a subject for "varśāḥ in pada d. Pingree’s emendations are much less important than
the ones preconized by the eminent philologist J. Brough for Newar manuscripts (Brough 1954:
354-364, reprinted in Collected Papers: 132-142). According to Pingree, the meaning of stanza 79,
15 is so obscure and doubtful that ‘it cannot be used to date the Kuśāṇas.’

Both Pingree and Falk agree that at least stanza 79, 15 states that Śaka years and koshāṇa
years are not identical, and both – (Pingree with reservations) – agree that koshāṇa years are years
of the era started by Kanishka I. In other words, year 1 of Kanishka I cannot be AD 78. At the
time Pingree wrote that, it was a sound hypothesis. Nevertheless, it is a hypothesis (Hypothesis 1)
because, in inscriptions, the era is always referred to as Kanishka’s era, and because there may have
been another Kushan era, e.g. the era of the Bactrian documents published by N. Sims-Williams
and the Tochi valley inscriptions (now in Pakistan) which, according to F. de Blois, started in 223-224
(de Blois 2008), a date close to the following (see below) calculations of H. Falk.

Falk, using a very sound method, checked the manuscript and tried to understand it as it is. He
reproduces the exact spellings of the manuscript, which gives the following text (pada-s are mine):

\[
\text{Gatena sādhyardha-śatena yuktā}
\]
\[
vyekeṇa koshāṇa-gatābda-saṃkhyā \}
\]
THE RIDDLE OF THE ANCIENT INDIAN ERAS IS NOT YET SOLVED

kālāḥ śakānāṃ pariśodhya tasmād
atītam anyad-yuga-varsha-yātāḥ ॥

He translates: 'The elapsed years of the Koshāna (koshāna-gatābda-samkhya) in combination with (yuktyā) 149 (sādhya-rđha-śatena ...vyekena = 150 less 1) <change into> the time of the Sakas (kālaḥ śakānāṃ). Subtracting from this (pariśodhya tasmād) the elapsed (atītam) <yuga> <produces> the elapsed years of another yuga (anyad-yuga-varsha-yātāḥ)' (p. 127).

That translation fits perfectly with Sanskrit grammar even if the first gatena is not translated. We have two nominal sentences, the meaning of which Falk makes clearer by adding 'changes into' and 'produces', not the usual 'is, are'. But the addition of yuga is not a translation: it is an interpretation. Falk's translation produces a very rough Sanskrit, at least as far as I can judge from my own knowledge of Sanskrit. The crucial vyekena ('less one') is both separated by a word from the main number (sādhya-rđha-śatena) and in another pada. The first sentence ends in the middle of pada c. Pariśodhya and atītam are not in the same pada and three different words (gata-, atīta-, yāta-) are translated as 'elapsed'. All that is possible in this kind of Sanskrit, but it raises doubts about the translation, which should be called partly hypothetical (Hypothesis 2).

In any case, that translation enables Falk to explain the difficult stanza 79, 15 in the following way: 'Take the current Śaka year, elapsed, add 56 <as indicated in stanza 79, 14> and the result gives the years since the beginning of the first yuga. Subtract 165 (<yugam> atītam) and the result is the current stage of the second yuga, current in <Sphujidhvaja’s> time... Take the current Koshāna year, elapsed, and add 149: the result is the current Śaka year elapsed... A date Koshāna elapsed plus 149 produces Śaka elapsed’ (Falk 2001: 127). That is, year 149 of the Śaka era is year 1 of the Koshāna era; so (in short) year 1 of the Koshāna era is 78 + 149 = AD 227.

The least which can be said is that it is a very convoluted way to say the following: Yavanarāja’s yuga began 56 years before year 1 of the Śaka era (i.e. in AD 22). The following yuga thus began 165 years later, in Śaka 109 (109 + 56 = 165, the duration of a yuga) or AD 187, i.e. forty years before the koshāna era (227 less 40 = 187). Sphujidhvaja’s two stanzas (79, 14-15), with several important words lacking and a broken order of words, are thus very difficult to understand, even for astrologers, whereas Sphujidhvaja’s style and syntax are usually very simple and easy to understand, the only difficulty being the way the numbers are expressed. That is the consequence of Falk’s Hypothesis 2. It is not a new hypothesis, but it does not strengthen Hypothesis 2.

One may wonder why Sphujidhvaja wanted to indicate (moreover in a way almost impossible to understand) how to calculate the beginning of the second yuga. Any astrologer knowing that the duration of a yuga is 165 years and that a yuga began 56 years before AD 78, is able to know what the current yuga is and how many years have elapsed since its beginning. Falk gives us an answer: ‘Sphujidhvaja want(ed) to find the actual year, elapsed, of the yuga’ in the year he was completing his treatise, i.e. in Śaka 191 (Falk 2001: 127). But that hypothesis (Hypothesis 3) has no basis at all. Sphujidhvaja knew perfectly in which year of which yuga he was writing. He did not write his treatise for his own use. The people for whom he was writing, his contemporaries and future astrologers, did not have to be told when the second yuga began. They knew it. Once told that Yavanarāja’s yuga began 56 years before year 1 of the Śaka era, they only had to make a few simple additions and subtractions.
Of course, Falk knows that AD 227 is too late a date to be year 1 of Kanishka. He solves the difficulty by supposing that Sphujidhvaja was referring to a second koshāna era, beginning a mere hundred years after Kanishka's year 1. In other words, he adopts Lohuizen-de Leeuw's 'omitted hundreds' theory (see Appendix 1). There are, in fact, two hypotheses: 1. that Sphujidhvaja either referred to an era not much used as far as we know (the second koshāna era) or used an elliptic way (omitting the hundreds) to indicate the date (Hypothesis 4); and 2. that the 'omitted hundreds' theory is valid (Hypothesis 5). Without these two hypotheses, neither particularly strong, Falk would not be able to arrive at the AD 127 date for year 1 of Kanishka, which he supports by a number of other well-known hypotheses. Among these other hypotheses is the so-called Chinese evidence (see Appendix 2) which is his Hypothesis 6, and not the stronger one.

Hypothesis 4 contradicts Sphujidhvaja's purpose of teaching the principles of Greek astronomy, of course in the clearest possible way (stanza 79, 1). A writer able to tell, in one Sanskrit stanza, that 'a <synodic> month of the moon, which ends with a conjunction, consists of 29 days and 32 kshānas minus 4 kalās and 107 sixtieths of a kalā' (79, 12) would have had no problem at expressing such a simple date without alluding to a shortened (omitting the hundreds) koshāna date and without making his previous indications need long explanations, which are not included in the text nor in the available commentary.

I shall briefly refer in the following sections to Hypotheses 5 and 6, which are not Falk's own, but which Falk must adopt to support the result of his calculations. But we may already conclude that Falk's paper, although well informed and very intelligent, does not solve the enigma of Kanishka year 1. Six hypotheses backing each other, some of them strong, others weaker, do not constitute proof. We are still unable to establish incontrovertibly whether year 1 of Kanishka should be placed in AD 78 or 127.

6. A BASIC GANDHĀRĀN CHRONOLOGY

Our knowledge of Gandhāran history during the period 50 BC–AD 50 has greatly progressed in the last thirty years, thanks to the publication of many inscriptions of the petty rāja-s of Oḍi and Apracha (feudatories first of the Śaka kings, then of the Kushans); and to the publication of many new coins, mainly by O. Boparachchi and R. C. Senior. The provenance of these documents is undocumented, or documented only by hearsay, which is not any better. But, on the whole, we now get an approximate chronology of the rulers during these hundred years. That was a time of recurring invasions and wars between the last Indo-Greek kings, reigning in limited and overlapping territories, sometimes intermarrying with the newcomers; the Śaka warlords, the Indo-Parthians seemingly coming from the south and the first Kushans. That is the best evidence to calculate the date of Kanishka. It is summarised below. I assume that there were two Azes and one Gondophernes. The chronology will not change substantially even if there were one Azes and two Gondophernes.13

The last Indo-Greek kingdoms of northwestern India were conquered by the Śakas under the overlordship of Azes. Some Indo-Greek rulers may have subsisted for a while after 58 BC, in reduced territories, maybe as Śaka feudatories. On the whole, however, by c. 58 BC the Śakas were firmly in
power. The Kushans under Kujula Kadphises made inroads into Gandhāra c. AD 20. Their advance was checked by a newcomer, the Indo-Parthian Gondophernes, whose reign of more than twenty-six years began c. AD 20, as shown by the corroborating evidence of the Takht-i-Bahai inscription referred to above and his imitating coins of the Parthian Gotarzes I (c. AD 40-51).

Gondophernes and Soter Megas were contemporaries as shown by overstrikes of Gondophernes-Sases on Soter Megas coins (Senior 2009). They evidently fought against each other for possession of Gandhāra and Punjab. Apparently, Soter Megas, after his first victory, was defeated by Gondophernes-Sases, who thus controlled a territory where Soter Megas’s coinage was the main currency. Then Gondophernes-Sases was definitely vanquished and the Kushans established themselves firmly in Gandhāra and northern India, either under Soter Megas if he was able to take his revenge against Gondophernes-Sases, or under Wima Kadphises. In other words, Soter Megas was present in Gandhāra c. AD 40-50. He was succeeded by Wima Kadphises who, on his first coins, looks like a quadragenarian. Kanishka succeeded his father Wima Kadphises.

The determination of year 1 of Kanishka depends on that data. If you stick to year 1 = AD 78, i.e. the beginning of the Śaka era, two Kushan kings appear to be reigning one after the other between c. AD 40 and 78, namely Soter Megas (whose reign in Bactria may have begun before AD 40) and Wima Kadphises. That gives a total and not too long duration of c. 40 years for the reigns of both these kings put together. That does not prove that Kanishka’s year 1 should be placed in AD 78, but it does make it possible, and would solve the enigma of the origin of the Śaka era. At least, it is as good a hypothesis as Falk’s hypothesis that the Śaka era is an astronomer’s era (Falk 2001: 133).

If one does not wish to equate the Śaka and Kanishka eras, one may place Kanishka later. If one still believes in the Chinese evidence (see below), despite all the arguments against it, one may equate Kanishka’s year 1 with AD 125 or 127. However, it will have to be admitted that the combined duration of the reigns of two important kings, one of them (Wima Kadphises) c. 40 years old when he ascended the throne, must have been at least 85 years, a very long duration indeed. That late date (c. AD 125) is still more difficult to agree with, although not impossible, if, as it appears, it was Wima Kadphises who defeated Gondophernes c. AD 50. That would give his reign a duration of c. 75 years. Moreover, a date of c. 125 makes it more difficult (but not impossible) to understand the relationship between Gandhāran and Mathuran art.

The choice is open: it will not make a big difference for the historian.

Appendix 1: The Omitted Hundreds Theory

Falk’s Hypothesis 4 (the ‘omitted hundreds’ theory) and Hypothesis 5 (the Chinese evidence) have been, for more than fifty years, the subject of unending discussions. Here I shall content myself with a few words. The reader will easily find all the huge bibliography and the many detailed papers which have already been published on these subjects.

Let us begin by talking about the seemingly self-evident fact that Kanishka, not being a Śaka, could not be the founder of the Śaka era. The Rabatak inscription makes almost sure that Kanishka boasted about having conquered Ujjain,12 which we know, thanks to other evidence, as having been
ruled by the Śakas. He also boasted about having created a new computation,\textsuperscript{13} which we know he imposed on all his dominions, and therefore in Ujjain too. If the Ujjain Śakas used the Kanishka era, the Ujjain era used by Indian astronomers could have been called the Śaka era. That is, of course, also a hypothesis.

The 'omitted hundreds' theory was invented by the great historian of art, J. van Lohuizen-de Leeuw, to solve a contradiction: a seated Mathura Buddha, dated year 22 (without indication of the era), was assumed to have been 'established' in year 22 \textless of Kanishka\textgreater{} by Daya Ran.\textsuperscript{14} This date is much too early according to Lohuizen-de Leeuw, as the statue is quite different from the Kapardin Buddhas carved in Mathura at least till year 51 of Kanishka (Härtel 1983: 656-657) and looks like a Mathuran copy of a Gandhāran Buddha (Lohuizen-de Leeuw 1949, Pl. XXX, fig. 54). Lohuizen-de Leeuw added some palaeographic considerations, which were later discarded, and solved the contradiction by arguing that the date should be read as \textless 1 \textgreater{} year 22 \textless of the Kanishka era\textgreater{}, i.e. with omitted hundreds like in the well-known Kashmir laukika computation (Lohuizen-de Leeuw 1949: 232-235).

Härtel added weight to Lohuizen-de Leeuw's theory by adding that the Mathura inscriptions on the Kapardin Buddhas do not refer to Kanishka with the title devaputra before year 33. Therefore, an inscription dated year 14 (Lüders 1960: 116-119) using the word devaputra should be placed in year \textless 1 \textgreater{} 14 (Härtel 1996).

The 'omitted hundreds' theory was soon adopted by historians of Gandhāran and Mathuran art, for when an inscribed relief bears a date which does not fit with the evolutionary schemes they are adhering to, they have only to add 100 or 200 years to that date to make it compatible with their theories. Their reasoning is: if we do this when a date is expressed in the laukika era, why should we not do it when the date is expressed in the Kanishka era?

Härtel's devaputra theory rests on the assumption that dedicatory Buddhist inscriptions are official documents, and thus should only use the set of official titles permitted or imposed by the Kushan chancery. But these inscriptions are private documents, never written by the Kushan officials nor under their control. They use respectful titles for the king, but may drop one word if they want to, for instance, due to lack of space. In any case, the word devaputra was used in the first years of Kanishka: it is a calque of Bactrian Bago-pooro (Bag-puhr) used by the dignitaries who wrote the Rabatak inscription when referring to the still alive Kanishka.\textsuperscript{15} It is also written in the inscription on the Mathura statue of Kanishka, but we do not know whether that statue was carved during his lifetime.

Lohuizen-de Leeuw's theory rests on three untold assumptions or hypotheses, one of them now proved wrong, the others very dubious. The first assumption is that the classical Gandhāran Buddha image was carved not long before Kanishka. We now know that it dates back to c. AD 50 or slightly later, i.e. 42 years before the Mathuran image dated year 14, if year 1 of Kanishka corresponds to AD 78, and even more (91 years) if it corresponds to AD 127 (127 + 14 less 50). The second assumption is that the Mathuran carvers were so conservative that they did not succumb to Gandhāran influences before year 51 of Kanishka. But under the Indo-Greeks, the Śakas and the Kushans, Mathura and Gandhāra were under the same foreign masters. Officials, merchants and
monks plied along the Grand Trunk Road, which was used since the time of Aśoka, at least. The monks in Gandhāra and Mathura belonged to the same orders (nikāya). The contents of the sūtra-s they used to read and preach were the same.

Art historians admit that the Mathuran statues were known in Gandhāra before Kanishka: the archaic sitting Buddhas of the Swat valley were surely influenced by early Mathuran effigies. How could we believe that there was no reverse influence from Gandhāra on Mathura till 51 + x years after Kanishka, i.e. till a time when the Kushan empire was on the verge of crumbling? Is it possible that the scribe who entered in Kharoshthi script a list of Śaka dignitaries on the Mathura lion capital did not know anything about the development of art in Gandhāra? Can we believe that nobody in Mathura knew, at least by hearsay, the Gandhāran statue of a standing Buddha shown on the reverse of some Kanishka coins?

The third assumption supposes a linear evolution of styles and buyers’ tastes. According to this assumption, every artisan in every Mathuran workshop would carve reliefs in exactly the same way. And no buyer of a Mathuran statue would have been given any choice between different types of effigies. I wonder whether such a harmony ever existed. The production of statues in ancient India was a craft. There was obviously a market and it did adapt to the tastes of the customers. On the same day, and for the purpose of dedicating it to the same shrine, an old farmer and a young landlord would probably have bought quite different effigies if they were given the possibility to choose. An innovative workshop may have easily found buyers. And there were probably many different workshops in Mathura, one or some of them producing Kapardin Buddhas with conventional inscriptions, others Gandhāran-like Buddhas. Till now, the omitted hundreds theory is only an easy way to solve the difficulties of a history of Indian art that refuses the possibility of a plurality of styles, at the same moment, in the same locality, for a plurality of potential customers in a plurality of workshops.

Appendix 2: The Chinese Evidence

The Chinese evidence may be summarized thus. Hou Han-shu (118, 9) gives a history of the Yuezhi and the Kushans till the beginning of their conquest of India. Its source is supposed to be an official report of Ban Yong dated c. AD 125. That report gives the names of the two first Kushan kings, Qiujuque (Kujula Kadphises) and Yangaozhen. It does not name the most important king, Kanishka, so Kanishka was not yet ruling c. AD 125. It is an argumentum e silentio (using the absence of data as evidence), i.e. a very weak way of reasoning.

Every scholar who knows Chinese and has studied ancient Chinese histories has cautioned against using these Chinese sources without knowing the context and the way ancient Chinese historians, who copy or quote each other, were working (Chavannes 1895; Haneda 1933; Bielenstein 1953; Watson 1958; Hulsewé 1961 and 1979; Zürcher 1968: 357-351; Thierry 2005; Markley 2011). In short, the phraseology and syntax of the ancient Chinese is not as precise as it could appear to be from the extant translations in Western languages. The Chinese annals are riddled with inconsistencies and misleading information as Chinese historians use extracts from earlier
documents and delete, without any warning, information which does not interest them or which would be detrimental to the prestige of the ruling dynasty. So, the absence of Kanishka's name may be attributed either to the fact that Ban Yong, a Chinese general warring in Xinjiang, was more interested or knew more on the Bactrian Yuezhi-s, his neighbours, than about the far-away Indian Kushans, or to the fact that his informants did not have any recent information on northern India, which in any case the Chinese almost did not know at that time, or due to any other reason (lastly Thierry 2005: 482-484).

The Chinese evidence, thus, is no evidence at all. It may even be misleading if you take it at face value. We know the sequence of the first Kushan kings: Kujula Kadphises, Sōter Megas, Wima Kadphises and Kanishka. *Hou Han-shu* (118, 9) gives the names of the first two kings: Qiujuque (Kujula Kadphises) and Yangaozhen. Formerly, when most numismatists believed that the coinage of Sōter Megas was the last issues of Kujula Kadphises or the first issues of Wima Kadphises, Yangaozhen was equated with Wima Kadphises, although it sounds entirely different. We are now quite sure that Soter Megas17 was not Wima Kadphises. So if we trust the *Hou Han-shu*, we should equate Yangaozhen with Sōter Megas, and deduce from the same argumentum e silentio that Wima Kadphises, not mentioned in Ban Yong's report, was not yet reigning in AD 125. That would push year 1 of Kanishka after AD 150. Hence, the Chinese evidence does not lead us anywhere. There is no Chinese evidence.

Endnotes

1 You need them because the lunar year (12 x 29.5 = 354 days) is shorter than the solar year (365¼ days).
2 For a fuller discussion of that date, see Fussman 1993: 116-177.
3 Every Kharoshthī document, accompanied by its complete bibliography and necessary comments, is now easily available through the wonderful website established by R. Salomon's former students Stefan Baums and Andrew Glass: http://gandhari.org.
4 I use angular brackets < > to enclose words added by R. Salomon to make the English translation less awkward. R. Salomon used square brackets [ ] for that purpose. As in all my publications, I use these square brackets to enclose syllables (akshara) which cannot be read with certainty.
5 Mahārāja does not mean 'Great King'. It translates the Greek title βασιλέας in the bilingual Indo-Greek coinage as pointed out by Maricq (1958: 375-378).
6 We are far from understanding all the details of that celebrated inscription. One of the strangest thing is that there is a large gap between Gudu and vharasa. It could have been easily avoided if the line had been engraved 5 cm below. In the same way, there is a still bigger gap between erjhuna Kapa and the genitive ending -sa. That can be explained by the fact that the stone had been used for grinding spices. But if that explanation is true, we should not print with Konow erjhuna Kapasa puyae, 'in honour of Prince Kapa', but erjhuna Kapa [XXXXXXXXX] sa puyae, with ten akshara entirely worn out.
7 See Senior 2008 for his last evidence and previous references.
8 My reasoning was that the existence of two Azes was the only way to explain why the redactors of both inscriptions felt it necessary to inform us, in an euphemistic way, that the Azes who started the era had passed away (atiadasa, purvakalisa), i.e. was Azes I and not the reigning one, i.e. Azes II. It may be too subtle to be true.
To make it simpler, I do not print sarva'ttsaraye etc. as in the original publication for the superscript \( \text{m} \) were only the graphic representations of a floritura.

A Greek name (Heliophilos) is not impossible in the early second century AD, but fits better with an earlier datation.

For more details and references, see Fussman 1998: 627-631. More detailed chronologies are easily available. They are also more hypothetical. The latest one is Falk 2011: 133-136, with detailed bibliography. Here I give only the basic data, not susceptible to greatly change.

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