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The Nature of the Verses of the *Kāśyapaparivarta*

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The topic of “negative results” and their absence from the published literature, so-called “publication bias,” has attracted particular attention in recent years in the hard sciences and medicine (e.g., Song et al. 2010, Fanelli 2012, and the cited literature). It is harder to pinpoint such effects in the humanities, but the overall trend is doubtless the same: absence of correlation and rejected hypotheses become invisible, a process sometimes called the “file drawer effect,” in which research which seems void of positive result is simply filed or thrown away. While the present paper was not (perhaps needless to say) originally conceived as a contribution to the collection of null results, I believe that the process through which I reached a sort of negative conclusion is nevertheless not devoid of interest.¹

The *Kāśyapaparivarta* (below, KP) belongs to the group of ‘earlier’ Mahāyāna sūtras, but as with almost all Indian texts, it is very difficult to date precisely.² Even more problematic than the question of the date of composition of the text as a unit is the question of the date of its verses. The latter stands at the center of a hypothesis I once entertained, but which in the course of preparing the present remarks I came to doubt, to wit, that the verses we find in the unique, nearly complete Sanskrit manuscript of the KP (and evidenced by several translations) are not written in a genuine dialect, but are rather a result of intentional archaization.³ The reasons I held this hypothesis, and why I have given it up for the present, at least in its strong form, frame the following discussion.

My initial doubts stemmed from questions of grammar and of metre. I felt that both were, in the verses of the KP, irregular in ways that set them apart from otherwise comparable texts and since I (thought I) knew that the verses postdated

the prose, perhaps considerably, I suspected that these ‘irregularities’ might point to their composer’s discomfort with his medium. Crucial to the problem is the extant evidence and its nature.

The bulk of the KP is available in a single, almost complete, Sanskrit manuscript from Central Asia, SI P/2. Almost certainly copied in Khotan, it has been dated on paleographic grounds to the 7th–8th centuries.⁴ Especially important are other probably more or less contemporaneous Sanskrit fragments, since these contain material corresponding to the prose of SI P/2, but omitting the interspersed verses.⁵ Fragments in Sanskrit without verses were also found in Sri Lanka on copper plates dated paleographically to the 8th–9th century.⁶ Other sources include a 9th century Tibetan translation transmitted in the Kanjurs, which duly translates the verses and is on the whole rather close to the text of SI P/2, but alongside of which we have a probably also 9th century Dunhuang manuscript fragment of another Tibetan version, Pelliot tibétain 676, free of the verses.⁷ The sūtra’s Indian commentary, of undetermined authorship,⁸ available at present only in Tibetan and Chinese translation but originally dating from perhaps 6th century India, does not refer to the verses (with the exception noted below), but the *Prasannapadā* of the 7th century philosopher Candrakīrti quotes three.⁹ We have moreover five Chinese translations of the sūtra, dating from the Han dynasty through the Song, of which only the very last, from the 10th century, translates the verses.¹⁰ Finally, additional evidence comes to us from fragments of a Khotanese translation of the sūtra, datable on paleographical grounds to between the 5th and 6th centuries, which has been interpreted to witness a version without verses (Martini 2011: 167–68, 173).

In sum: The sūtra itself existed at least as early as the 2nd century C.E. A 7th century philosophical treatise, the 7th-to-8th century Sanskrit manuscript from Khotan, the 9th century Tibetan translation and the 10th century Chinese translation provide evidence for a form of the sūtra with verses accompanying a great many sections, while the remaining sources—including material later than the 7th century—bear witness to the circulation of a version without these verses in something close to the same period. It is possible that from early on at least two major versions or recensions of the text circulated side-by-side, one with and one without verses;¹¹ we should not, however, automatically assume that differences in versions of a scripture are necessarily the result of linear development. While it is clear that the KP (as probably most, if not all, such sūtras) was transmitted in a flexible form (the precise historical relation between its extant versions remaining, however, unclarified), all things considered it seems to me not very likely that there were two major contemporaneous recensions, one with and one without verses. Much more likely is that verses were added to (some version[s] of) the text at some point during or before the 7th century, but since this addition could not extend to all circulating versions, those available in Sri Lanka and Dunhuang continued centuries after this addition to be copied in a form free of verses. Although it is not possible to prove this—indeed, I am not sure what sort of evidence *could* prove it—the hypothesis raised serious questions.

My assumption of this hypothesis was supported by forceful authorities. For J. W. de Jong (1977: 255): “The *Kāśyapaparivarta*, in which the verse parts are later than the prose parts, offers an interesting example of a text in which the verses, written in Buddhist Hybrid Sanskrit, are definitely later than the prose parts, the language of which is much closer to standard Sanskrit.” De Jong evidently wished to propose some contrast to the usually imagined scenario in which a sūtra text was originally composed both in prose and verse together, or even with the verses preceding the prose, both being originally in some form of Middle Indic, which was then later updated or Sanskritized. In such a case, the verses, subject to the constraints of metre, perforce retained more archaic forms than did the prose which, free of such constraints, was moved in the direction

of a more standardized Sanskrit form. In the case of the KP, in contrast, the so-called hybrid verses are, for de Jong, in terms of their date of composition newer than the now more modern looking prose. As a general rule, prose is easier to Sanskritize than verse but consequently, as can clearly be seen for example in the *Dharmapada/Udānavarga* text corpus, verses may be entirely recast or rewritten, since in the transformation from Middle Indic to Sanskrit their metrical form cannot be preserved.¹²

I am not sure that the form in which we now find the verses in the KP really looks older than does the prose, as de Jong seems to imply, but what is more important is the question whether it is not only that the verses are newer than the prose of the sūtra in terms of their date of composition, but that their grammar, while seemingly older looking (in the sense that Buddhist Hybrid Sanskrit allegedly looks older than standard Sanskrit), may possibly be artificial. The hypothesis I worked with raised the question whether we might be justified in seeing these verses as composed in an intentionally archaizing style by an author who did not most naturally—as did the authors who first composed the earlier Mahāyāna sūtras—compose in a language, or dialect, or idiom called “Buddhist Hybrid Sanskrit.”¹³

While the dating of texts, which may be revised or altered by their copyists down through the generations, is an especially tricky problem, inscriptions provide us some framework through which to view language change. In this regard, for Richard Salomon “the available evidence suggests that hybrid Sanskrit arose in the course of a gradual Sanskritizing movement which had its origins in the late centuries B.C., expanded in the early centuries of the Christian era, and culminated in the final triumph of classical Sanskrit in the Gupta era.” He goes on:

From the less formal Buddhist texts, and especially from the epigraphical remains of the period in question, we . . . get the impression that a variety of dialects, or perhaps rather of dialectal styles, covering a broad spectrum from pure M[iddle] I[ndo] A[ryan] to pure Sanskrit were available for varying purposes. The choice of a given dialect by a given writer was, to be sure, governed to some extent by his knowledge and level of education; but to an equal, and perhaps greater, extent, it was the content and nature of the document he was writing which would determine the appropriate level of Sanskritization. (Salomon 1998: 84–85)

Although there is no necessary and direct connection between inscriptional and manuscript use, we should note Salomon's judgement (1998: 93) that "by about the end of the fourth and beginning of the fifth centuries A.D., Sanskrit had at last established itself as virtually the sole language for epigraphic use throughout India. Prakrit, from this time onward, virtually fell out of epigraphic use. . . ." If we are justified in concluding from this that in the period between the 5th and 7th centuries Sanskrit was widely known (by those who would dare to author texts in the first place), then we may also speculate that the choice of an idiom other than classical Sanskrit was intentional; as Salomon suggests, the "nature of the document . . . would determine the appropriate level of Sanskritization."

The idea of intentionally archaizing holy language is not a new one. The case of the *Aśvistuti* in the *Mahābhārata* has been studied for more than 150 years. In his recent remarks, Minkowski (2012: 79) characterizes the discussion as one over whether the verses represent "ancient material fortuitously preserved in a later text, or a later archaizing confection." In discussing the question of the relation of grammar and metre, he refers (2012: p. 81, n. 7) to Renou who stated (1939: 187) "Bref, si le style is pseudo-védique, le rythme est upaniṣado-pali," commenting "There are of course some dangers of circularity here, as decisions about permitted choices for reconstructed forms in the language of the verses presuppose a metrical shape, and vice versa." I will return to this key point below. In a similar example, van Buitenen (1966) discussed the *Bhāgavata Purāṇa* as composed in an archaized 'Vedic,' and in another parallel with our situation, he suggested that the motivation of the composers of the *Bhāgavata Purāṇa* was precisely to assert their orthodoxy also through their linguistic usage. Was this also the case (*mutatis mutandis*) for the author of the verses of the KP?

As just alluded to, our ability to make informed judgements about the language of the KP is sharply restricted by our sources. For most of the verses of the KP (as for most of its prose), we have only a single Sanskrit manuscript. We do have a Tibetan translation, Chinese translations, a commentary and other sources which enable us to gain some grasp of the overall trajectory of the text, even when the Sanskrit version remains frustratingly unclear (or is not preserved).¹⁴ The

manner in which the Indic text has been transmitted, however, has left us with a scripture the exact wording of whose extant Sanskrit recension is at times extremely difficult to establish with certainty. A result of this situation is that while the drift of the text may on the whole be plain, in any given case it may be very difficult, if not impossible, to distinguish genuine linguistic features from the quirks of the scribe.

In a valuable study, Seishi Karashima catalogued some "peculiar forms and features" of the language of the *Kāśyapaparivarta* not discussed by Franklin Edgerton in his epoch-making *Buddhist Hybrid Sanskrit Grammar and Dictionary* (1953). While carefully acknowledging (2002: 45) that "some peculiar grammatical forms . . . could be merely scribal errors," Karashima nevertheless considered as linguistic some features which I believe may be better understood as scribal.¹⁵ He referred, for instance, to alternation between *e* and *ai* and *o* and *au*, seeing as genuine linguistic forms *karauti*, *sagoravau* (nominative) and *klaiśa*,¹⁶ where I see instead optional writings. I thus do not think that we need to make room grammatically for a nominative-accusative dual masculine -*o* (Karashima's §9.5): the form is normal -*au* written as -*o*. The same may be said for instrumental plurals in -*eḥ* rather than -*aiḥ* (§9.16.2).¹⁷ It seems to me that for the scribe of SI P/2, *e* and *ai* and *o* and *au* are, respectively, the same vowels. The problem is phonemic rather than graphic: *karauti* and *karoti* must have sounded the same to the scribe. As another example, trivial in itself, there are any number of cases in which we have forms which are 'irregular' only in lacking a final nasal. I tend to consider the vast majority of these cases, unless guaranteed by the metre, as merely artifacts of accidental omission of anusvāra, and therefore not linguistically significant, a possibility Karashima duly considers.

The large-scale issue is this: for grammar, it is patterns that are important, not *unica*. How to determine the border between individual scribal quirk (or idiolect) and genuine feature is naturally a difficult—though fundamental—question for any grammatical description.¹⁸ We must begin with individual texts, and more than that even, individual manuscripts, and try to establish their patterns. But if we consider features that do not extend beyond the boundaries of a single work of a single scribe as linguistically significant *patterns*, we will find ourselves without any tools

with which to distinguish idiosyncrasy from socially sanctioned 'language.'

As a means of breaking free of the morass of linguistic forms per se being difficult to distinguish from scribal habits, particularly in the absence of anything approaching an agreed-upon norm, I assumed, when I began this study, that metrical criteria belong to a wider sphere of agreement, and I expected that there I would find criteria to distinguish the genuine from the imitation (as implied above by my use of the expression "guaranteed by the metre"). Among the factors I did not sufficiently reckon with, however, is the potential for circularity mentioned by Minkowski, in which "decisions about permitted choices for reconstructed forms in the language of the verses presuppose a metrical shape, and vice versa."

The KP, as we have it in Sanskrit, contains 158 verses, although the Tibetan and Chinese translations make clear that the texts upon which they were based (and almost certainly SI P/2 when it was complete) possessed several more verses in portions now missing. Of the preserved verses, which almost without exception restate or summarize the prose, 10 are original: their presence in the text is attested by Chinese translations and the Indian commentary, suggesting if not virtually proving that they formed part of the sūtra before, as I see it, the addition of the majority of verses in some later century. These original verses are, perhaps not incidentally, the last 10.¹⁹ The 158 preserved verses, with the exception of 4 in śloka,²⁰ are in triṣṭubh (including also jagati). Studies give us the impression that as far as triṣṭubh is concerned, with some latitude the form is predictable (Edgerton 1936, 1939, 1946, as well as standard works on Vedic and Classical metre), and this gave me the impression that irregularities were signs of the inability of the composer of the KP's verses to work confidently in his medium. However, studies of Epic and Pāli make clear that this is simply not so (Warder 1967: 202–12; Fitzgerald 2006, 2009; Tokunaga 1993).²¹ In the KP overall, the verses are in a mix of Indravajrā or Upendravajrā and Vamśastha. The scheme is often depicted ideally, with indication of the opening, break and cadence, as – – ∪ – | – ∪ ∪ | – ∪ – – || and ∪ – ∪ – | – ∪ ∪ | – ∪ – ∪ – ||. Altogether we have somewhere around 600 lines of triṣṭubh.²²

In his 1973 remarks on the metre of the *Ratnaguṇasamcaya-gāthā*, often claimed to be

one of the very oldest Mahāyāna texts,²³ Akira Yuyama identified a number of features to which I would like to draw attention. I refer to his observations for two reasons. First, Yuyama made great efforts to establish the genuine readings of his text, and while ambiguities and problems still remain, he certainly had a very good idea of the transmitted readings. Moreover, it seems that this text, which is composed entirely in verse (exclusively Vasantatilakā), presents a true "hybrid" language. Or, that is to say: many of the features of its language appear Sanskritic but are to be understood as concealing beneath them Middle Indic. Yuyama wrote (1973: 244 [§5]):

To fulfill the metrical requirements, lengthening of a naturally short syllable, shortening of a naturally long syllable, and loss of a vowel in sandhi are very frequently observed throughout the text. Some Middle Indic spellings remain un-Sanskritized. Middle Indic forms in declension and conjugation are abundant.

These features are in close conformity to the norms asserted by Edgerton in his 1946 article, "Metre, Phonology, and Orthography in Buddhist Hybrid Sanskrit," in which he emphatically asserts that, just as with Sanskrit, authors who wrote in BHS were rigid in their conformity to metrical norms. "Seeming exceptions," he wrote (1946: 198 [§3]), "are due either (1) to orthographic habits which misrepresent actual pronunciation (some-what as in the tradition of Vedic verses), or (2) to corruptions in the tradition as found in our mss. and editions."²⁴ A Gordian knot, however, binds together manuscript readings and linguistic features, while the nature of the metre and its flexibility beyond the bounds recognized by Edgerton calls into question the wisdom of emendations, such as those to vowel length, for the sake of regularization. While one is probably justified in a presumption that those who composed verses (it would certainly be wrong in a great many cases to call them poets) understood the metre they intended, there is no corresponding assurance that the rigidity of metrical structure can provide an argument for reading or restoring precisely such features as morphologically unjustified vowel length.²⁵

Edgerton wrote (1946: 200 [§40], and earlier 1936: 43): "If the preceding syllable ends in a short vowel, that syllable is metrically short, regardless of the number of consonants written at the beginning of the following word." After discussing the

corresponding shortening *metri causa*, as he calls it—an expression which Helmer Smith found close to meaningless—he went on: “In short, metrical shortening and lengthening of final vowels takes place before initial (written) clusters exactly as before initial single consonants. This must mean that the clusters are graphic only; a single consonant was pronounced. Apparent counter cases, in which a short final vowel occurs in a metrically long syllable before an initial consonant cluster, are extremely rare.”²⁶

In the KP there are, in the roughly 600 lines amenable to examination, 31 cases in which the cluster *pr* does not make position, that is, in which a short vowel standing before the cluster *pr* must be read as metrically short, just as Edgerton predicted.²⁷ The other cases of clusters not making position are much rarer.²⁸ When *pr* does make position, only 2 out of 21 occurrences²⁹ are in word-initial position.³⁰ When *pr* does not make position, it is found 8 times in the opening, 9 in the break and 14 in the cadence (the least variant portion of a line), and when it does make position it occurs 5 times in the opening, 5 in the break and 11 in the cadence. Statistically, then, there is evidently no preference for either usage. It is frequent for the caesura to fall not only after the 4th or 5th syllable, but also after the 6th (and in some cases it is difficult to identify any caesura at all), something found in the Epic (Fitzgerald 2006, 2009, Tokunaga 1993) and elsewhere in Buddhist literature, but not acknowledged by Edgerton. On the basis of a revised picture of metrical possibilities, I now believe that there are only two examples in which a case could be made on metrical grounds for Middle Indicisms in the KP verses: in *evam hi śīlāśrutadhyānayukto* (91.2a), *dhyāna* could be read as **jhāna*, and *anopalambhaṃ āryāṇa gotraṃ* (137.3c) is (more regularly) metrical if we read **ariyāṇa*.

Reference to other texts here is limited by space, but it should be noted that Edgerton’s principles are not necessarily generalizable, a fact which earlier also led me to doubt their applicability to the KP. As just one example, in the *Ratnakūṭuparivarta*’s *triṣṭubh*, we find initial clusters, almost all of which are *pr*, regularly making position. In this text, which in other respects contains many Middle Indic elements (Kurumiya 1978: xxiv–xxxviii), I noticed only two examples of an initial cluster which *failed* to make position.³¹

Other features which struck me as odd as I studied the verses of the KP include that the manuscript as we have it 4 times writes *bhavati*, which the metre (if regular) in each case proves must be read rather as *bhoti*, a form which, however, appears in the manuscript a further 13 times.³² Moreover, while once we find *bhonti*, 4 times we have *bhavamti*, a form also guaranteed by the metre.³³ I wondered whether in a genuine idiom *bhavamti* could coexist with *bhonti*—but both forms occur repeatedly in the *Saddharma-puṇḍarīka*, for instance, along with forms like *bhavatī*, all guaranteed by the metre.

At least 40 times in the KP verses we find initial resolution of two light syllables for one heavy at the beginning of a line. When this happens elsewhere than in initial position, it occurs 12 times in the 5th syllable, and twice in the 4th, in line with the relative frequency of resolution elsewhere.³⁴ But correcting for hyper-Sanskritization and writing conventions, we have just 6 examples in the 5th syllable and 2 in the 4th. This distribution is in line with what was detected by Edgerton in his 1946 study.³⁵

Looking more directly at morphology, we find the non-Sanskritic gerunds *paśyitva* and *pūrvitvana*, locative singulars *yahim* and *śatrumhi*, the present indicative *vuccati* and the past passive participle *vukta*, many other irregular forms (some of which, of course, may well be scribal errors), and too many assorted endings, lengthenings and shortenings, to mention. With regard only to the last feature, it remains to be determined whether in a line like *satveṣu ca saṃgrahayo jinoktā* (19.1b) we are justified in writing *cā*, or whether in *karṣāpaṇānām ca bahu āyu bhoti* (92.1b) we should remove *ca*, yielding fully metrical lines in both cases. As suggested above, given the flexibility of metres evident in the Epics, for example, despite their antiquity in comparison with the KP, such changes may be unnecessary; at the present state of research, and once Edgerton’s ‘rules’ have been cast into question, it is simply not possible to say.

The clues I once thought I had detected as suggestive of an imperfect attempt to reproduce a style not entirely familiar to the author of the KP’s verses came to seem less incriminating than I had thought. What remains—at least for me—is the chronological conundrum. If we accept that the verses now found in the KP were added at some time between roughly the 5th and 7th

centuries (and again, there does not seem to be any way to prove or disprove this), could their author really have used the language of these verses as a, so to speak, contemporary idiom? The idiom we call Buddhist Hybrid Sanskrit, or Sanskritized Middle Indic, *seems* to belong to a considerably earlier period. While writing in Middle Indic, by Buddhists and by others, continued for centuries afterwards,³⁶ in the case of the KP verses, we are talking about scripture, *buddhavaṇa*. Was the language of such works esteemed in such a fashion that, being inspired to restate the gist of the prose in verse, their author felt it apt to utilize a then archaic or hallowed idiom? Was he, as Salomon suggests in a different context, choosing to write in this idiom for a specific purpose?

It seems to me possible to imagine an author who added verses to the KP in perhaps the 6th century or so. Being very familiar with a style of 'Buddhist Hybrid Sanskrit,' and knowing scriptures were composed in this idiom, though he himself belonged to a period in which Middle Indic had largely given way to Sanskrit³⁷—for reasons which we have no way to ever know, but which might have to do with the common form of other scriptures, which mix prose and verse—he composed appropriate verses in a suitably toned, perhaps to him archaic sounding, shape. If this is indeed what happened, then we may question whether it is right to really term the language of the resulting verses any form of Middle Indic at all.

In the end, I found myself unable to sufficiently sustain the hypothesis with which I began, in part perhaps because some received wisdom about the shape of Buddhist Sanskrit grammar and its relation to metre apparently stands in need of further reconsideration. The result may fall (at best?) into a sort of no-man's land between a null hypothesis and some suggestive but nevertheless still vague suspicions. I would not dare to claim certainty for any of it. What I am quite certain of, however, is that potentially very Rich questions can be raised, questions which may well require the wisdom of Solomon (alternatively: Salomon) to resolve!

Notes

1. It is interesting to consider the suggestion of Anderson 2012 regarding "the low profile of negative results publications" that "negative results are be-

lieved to indirectly communicate to the scientific community that a study was poorly designed and researchers were either unknowledgeable about the phenomenon or incapable of tailoring more robust research hypotheses." This is probably true for the humanities as well. Another parallel phenomenon in the humanities and hard sciences is the exaggeration of the importance of one's (putatively) positive results, as when papers bear titles claiming "paradigm shifts" (Fanelli 2012: 892). This trend is only further fueled by Ph.D. programs which state their expectation that students produce (often within 3 years!) "a significant new and original contribution to the discipline."

2. Our oldest evidence is the Han period Chinese translation attributed to Lokakṣema. There may be reasons to doubt this precise ascription, but the existence of the text in the 2nd century is a certainty. While it is most likely that the 'original' name of the text was *Ratnakūṭa*, in order to avoid misunderstanding I continue to refer to it as *Kāśyapaparivarta*.

3. This idea led me to title my 2009 presentation at the xivth World Sanskrit Conference in Kyoto, albeit with an interrogation, "Imitation Hybrid Sanskrit in the verses in the *Kāśyapaparivarta*?"

4. Vorobyova-Desyatovskaya 2002: vii. Martini 2011 provides a detailed discussion of the sources, particularly those in Sanskrit and Khotanese, and it is convenient to assume some of her results here (and thus inter alia not to repeat her references). Her paper could take advantage of the reading version of my presentation at the World Sanskrit Conference, and my draft edition and annotated translation of the KP. This has led to a certain intertextuality between my work and hers.

5. SI P/85A; Hoernle 143, S.B. 38, 39; Mannerheim 3—for all see now Vorobyova-Desyatovskaya 2002: 58–62.

6. Parānavitana 1939, fragment 51 reads *///vā strata jñāna draṣṭavyam* [?] *tadyath* *///*, which belongs with §§78–79 or §§79–80, but in either case demonstrates that the text here did not have the intervening verse.

7. It is likely that the Kanjur translation slightly post-dates this Dunhuang version, of which it may be a revision or a recasting, with inter alia the addition of the verses. A scenario which could account for this sees a version without verses revised in light of a Sanskrit manuscript other than that used for the initial translation, this second manuscript containing verses subsequently added at the time of the revision. Since we do not know where the Dunhuang version was translated, or by whom, it is difficult to say more.

8. See the discussion in Silk 2009. The *Madhyāntavibhāṅgāṭikā* (Yamaguchi 1934: 245.12–250.23), which is not necessarily the work of the same Sthiramati who authored the KP commentary, quotes §§66–71 without verses. However, since it is quite possible that the commentator simply chose to skip the verses,

perhaps as unnecessary to support his point, given that they substantially repeat the prose, their absence here is not probative.

9. The verses quoted by Candrakīrti (La Vallée Poussin 1903–1913: 156.2–157.4) are linguistically and metrically similar to those in SI P/2. However, in §71.1ab where the two texts overlap the readings do not entirely coincide: *yathāpi dīpo layane cirasya kṛto bhaveta puruṣeṇa kenacit* in the sūtra, while in the *Prasannapadā* (I ignore variants): *yathā hi dīpo layane cirasya kṛto hi gehe puruṣeṇa kenacit*. Twice in the 12 lines quoted in the *Prasannapadā* the cluster *kl* does not make position; there are only 2 other examples of this in the entire sūtra.

10. Without the verses are T. 350 *Weiyue monibao jing* 遺曰摩尼寶經 (cp. T. 1469 *Jiashe jinjie jing* 迦葉禁戒經, essentially a copy of §§111–138 of T. 350), T. 351 *Moheyan baoyan jing* 摩訶衍寶嚴經, T. 310 (43) *Puming pusa-hui* 普明菩薩會, and T. 659 *Dacheng baoyun jing* 大乘寶雲經, *juan* 7, the *Baoji pin* 寶積品. The verses are found only in T. 352 *Dajiashe-wen da baoji zhengfa jing* 大迦葉問大寶積正法經.

11. I avoid entering into the labyrinthine puzzle of the origins of the KP as a unit.

12. For a good example from the *Anavataptagāthā* see Salomon 2008: 171–72. It is a pity that Hideaki Nakatani, whose lectures on the *Dharmapada/Udānavarga* I attended more than 20 years ago, has yet to publish his extensive notes on the relations between various versions of this corpus, in which among other things he extensively investigates the impact of Sanskritization on the metrical recasting of verses (including, but not limited to, a comparison of the Subaṣi manuscript and the later *Udānavarga* recensions). An example of the fascinating results that careful and creative examination of metre can provide is found in Nakatani 1994.

13. To be more explicit: the Indic versions we now have of the KP all show, in different ways, ‘hybrid’ or Middle Indic features. The implication is that in the case of the prose portions this character of the language somehow goes back to the early composition of the text, while in the case of the verses, it does not.

There is a possibility that even the composition of the verses of the KP, and not only the copying of its manuscripts, may have taken place in Central Asia—but if so, how would Candrakīrti have had access to the verses? Given his familiarity with the sūtra containing the verses, it seems most likely that they were after all added within India (or could the thus augmented sūtra have been ‘reimported’ into the subcontinent?).

An additional note: it has been suggested. (e.g., Ruegg 2000) that we prefer the emic term *ārṣa* in place of BHS to refer widely to Buddhist Indic idioms. While there are certainly objections to be raised against the expression ‘Buddhist Hybrid Sanskrit,’ for the time being I retain it.

14. For an example of the sometimes seemingly hopeless situation in which we find ourselves, despite the existence of Sanskrit material, see Silk 2010.

15. It is a peculiarity that Indic Buddhist scribes—in contrast, for example, to scribes of the Hebrew Bible—have evidently not considered the nature of the Buddha’s word such as to motivate them to pay particular attention to its careful and strictly correct copying. Note that in what follows I focus my attention on the verses, although Karashima considered the text as a whole.

16. He speaks 2002: p. 47, n. 15 of “the Middle Indic development *ai > e*,” and at 48, n. 17 of the development *au > o*.

17. Karashima 2002: p. 56, n. 87 points out that *te* for *taiḥ* is found in a *Saddharmapuṇḍarīka* manuscript as well. I am not certain that this is the same phenomenon, but since Karashima cites it from the prose, here I set it aside.

18. In this context one must not forget the guidance of Brough 1954, who greeted Edgerton’s 1953 opus with appreciation, but also a stern warning about the difference between what a scribe wrote and what is meant to stand in the text.

19. I plan to treat these 10 verses in detail on another occasion, especially in light of the fact that they appear in all versions, and are discussed by the commentary. Metrically, they are not different from the other verses.

20. These are pathyā mixed with ma-vipulā: 115.1, 116.1, 133.1–2.

21. Probably a number of Helmer Smith’s studies are relevant here, but I confess my continued inability to penetrate the code in which they are written.

22. Some lines remain incomplete, damaged, or corrupt beyond my ability to repair them so far; I leave these out of consideration.

23. Stefano Zacchetti suggests to me however that this assumption is in need of serious reconsideration. While speculating that it may have been this very model of the *Ratnaguṇasaṃcaya-gāthā* (and the *Aṣṭasāhasrikā prajñāpāramitā*) which de Jong had in mind in making the comments cited above, Zacchetti wonders whether this too might be a case in which the verses were, after all, composed after the prose.

24. In regard to Edgerton’s reference to Vedic, one might now see Pierce 2011 and the literature cited therein, which discusses controversies around Edgerton’s understanding of Vedic metre.

25. Another factor: compositions are copied by scribes, who make mistakes, to be sure, but are also quite capable of ‘correcting’ a text, and particularly in the case of metrical passages in a well known metre, such correction, especially if it is a simple matter of vowel length, might well happen subconsciously. Once again, with only a single manuscript at our disposal, we have few tools with which to address this issue further.

26. Edgerton's claim (1936:43) that "initial consonant combinations . . . do not make a long syllable if the preceding word ends in a short vowel," but that "medial consonant combinations . . . always make a syllable long," and that the same is usually but not always the case at the beginning of the 2nd member of a compound may have had in mind a line like that in the *Saddharmapuṇḍarīka*: *muñcanti kṣetreṣu svakasvakeṣu* (1.8b), in which *sv* both makes and does not make position within the same word.

27. I denote the verses by section number followed by verse number and pāda letter (1.3a = section 1, verse 3, pāda a): 1.3a, 2.2a, 3.1a, 4.3c, 5.2b, 6.4c, 8.3c, 9.2c, 9.3c, 10.3d, 12.1d, 14.1c, 14.1d, 17.2c, 18.2a, 19.2d, 20.4b, 26.2b, 27.3b, 28.3a, 28.3d, 30.2c, 31.1c, 71.3a, 83.1d, 126.3c, 136.4b, 137.2a, 137.3b, 137.5b (x2). Of these cases 9 are pāda final (*jina*)*praśāstā* and 11 pāda final *proktā*.

28. *kr*: 7.1b; *kl*: 49.1c, 71.3b, 71.3d, 130.1c; *kṣ*: 16.1c, 27.2c, 32.1a, 106.1b; *gr*: 107.3a; *jñ*: 11.3a, 26.1c, 35.1c, 36.1c, 42.1d, 48.1c, 71.4a, 137.5c, 137.5d; *ty*: 112.1d; *tr*: 107.1a; *dv*: 111.1c; *dhy*: 27.3b, and for 137.3c, see below; *br*: 27.5d; *vy*: 129.1c, 130.1c; *vr*: 107.6c, 128.1d; *śr*: 5.2a, 46.1d, 126.4a, 126.5c, 129.2d; *śl*: 126.4d; *śv*: 6.1c; *sth*: 137.1d is interesting because the Middle Indic form would also be a cluster, and thus it is metrically indifferent; *sp*: 27.5a, 83.1c, but note that at 83.2c it does make position.

29. 1.2b, 2.2d, 2.3d, 3.3d, 6.4d, 9.2a, 10.2c, 12.1c, 12.2c, 14.3a, 18.1b, 18.2d, 20.1a, 27.2c, 28.2c, 35.1a, 39.1d, 42.1d, 85.1a, 136.2a, 136.2c. In the case of 2.2d, *dhāritva dharma pratipattiyā sthitaḥ*, we should perhaps read **dharmam*, in which case this example is to be deleted. Both here and elsewhere, while the possibility of scribal error necessitates taking any given example cum grano salis, overall the flavor of the conclusions should not be problematic.

30. 2.2d, 3.3d. Other word-initial clusters which do make position are *dv* 28.1d, *sm* 107.6b, *śr* 127.1c. In the case of *śr* 83.2d and *śv* 107.6d, it is not possible to tell whether they make position because the cluster comes after the first syllable, which is metrically free.

31. I checked only the verses in triṣṭubh. Examples of initial *pr* (I omit a few problematic cases): I.40d, III.14c, 15a, 66c, 67b, 68b, IV.5c, 18c, 19a, 19d, 21a, 27c, 28d, 37c. Other cases are I.36c *sn*, I.43c *dv*, III.14a *tr*, III.82b *sv*, III. 82d *sp*, IV.27b *śr*, IV.39c *tv*. The two initial clusters which failed to make position occur in III.71b *kṣ*, IV.120a *vy*.

In his grammatical sketch Kurumiya 1987: xxiv writes "Double consonants treated prosodically as single; the preceding short vowels are metrically short. This occurs very rarely." He then gives two examples, the first of which is on page 7, line 10, *na tvaritaṁ*, in the line which, however, he prints: *kasmā<t> tvam vigatāmayo na <tv>ritaṁ nirvāsi kālo hy aya<m>*, so that the apparently offending conjunct is in fact recon-

structed (see his n. 19 on p. 7—the reconstruction may be confident, but I would hesitate to base a grammatical claim on an unattested reading). The other example he offers is on p. 36, line 15, *sugata praśamaratikara* in the line *paramagatigata sugata praśamaratikara* in which he speculates with hesitation (n. 27) that the metre is Āryāgīti. Given the uncertainty of the metre, I do not follow how any claim can be made about necessary vowel length here. These examples illustrate the problematic nature of such investigations.

32. *bhavati* written for *bhoti*: 1.1a, 2.1a, 10.2b, 15.1a. *bhoti* written and necessary: 1.1b, 2.3a (2.3b?), 9.1a, 16.2a, 17.2b, 28.2b, 46.1a, 46.1b, 71.1c, 92.1b, 110.1b, 137.4b. We also once find *deśayati*, 1.2b, which the metre proves must be read *deśeti*.

33. *bhonti* 126.3a (*bhavamti* here impossible); *bhavamti* 18.2c, 20.2c, 45.1b, 45.1d.

34. 5th: 1.1a, 1.2a, 2.1a, 5.2b, 10.2b, 12.2c, 15.1a, 18.1b, 43.1a, 78.1d, 85.1b, 113.1b; 4th: 8.1b, 137.3a. Of the 12 cases in 5th position, 4 are to be resolved as *bhoti* and one as *deśeti*, as mentioned above, showing that the scribe did not understand quite what he was writing, or that reading conventions allowed *bhavati* to be written where *bhoti* should be read.

35. Edgerton 1946: 200 (§36) speaking about the third chapter of the *Saddharmapuṇḍarīka*: "two shorts may be substituted for the first (optionally long) syllable; also for the fourth or fifth, but never for the second, eighth, or tenth. . . . The substitution is much commoner in the first syllable than in the fourth or fifth."

36. See for instance Fussman 2008: 645 on the 12th century colophon of the *Vimalakīrtinirdeśa* manuscript. His comment regarding the language of the colophon deserves to be borne in mind: "Cela devrait nous mettre en garde contre la tentation de dater un texte en fonction de son seul aspect linguistique."

37. Regretfully, we have even less idea of the location where this may have taken place than we do of the time period.

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