

## How old is the Suttapiṭaka?

The relative value of textual and epigraphical sources for the study of early Indian Buddhism.

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The Tripiṭaka, or parts of it, survives in several languages. The Sūtra and Vinaya sections are generally accepted to be its oldest portions, and most scholars have assumed that they contain the oldest sources for the study of Indian Buddhism. In more recent times, however, this assumption has been much debated: the antiquity of the canonical texts, and their reliability as a source of historical information, has been called into question. In the following, I will consider the evidence for the dating of the Pāli canon, particularly the Suttapiṭaka, and I will assess the extent to which it can be taken to include information about early Indian Buddhism. Although the results of this investigation will have implications for the dating of all the early sectarian literature, I am concerned more or less exclusively with the early Pāli literature and its history.

According to the Sinhalese chronicles, the Pāli canon was written down in the reign of King Vaṭṭagāmiṇī (29-17 B.C.).<sup>1</sup> It has been generally accepted, therefore, that the canon contains information about the early history of Indian Buddhism, from the time of the Buddha (c.484-404 B.C.) until the end of the first century B.C.<sup>2</sup> That the canonical texts are a record of the period of Buddhism before they were written down in Sri Lanka seems to be confirmed by the fact that their language, Pāli, is north Indian in origin. Thus the Pāli canon shows 'no certain evidence for any substantial Sinhalese additions ... after its arrival in Ceylon.'<sup>3</sup> If the language of the Pāli canon is north Indian in origin, and without substantial Sinhalese additions, it is likely that the canon was composed somewhere in north India before its introduction to Sri Lanka, and is therefore a source for the period of Buddhism in northern India before this. The Sinhalese chronicles state that the canon was brought to Sri Lanka by Mahinda during the reign of Aśoka, implying that it predates the middle of the third century B.C.<sup>4</sup> According to this history, the Pāli

<sup>1</sup> Dīp XX.20-21, Mhv XXXIII.100-01; See Collins p.97.

<sup>2</sup> Accepting Richard Gombrich's dates; see below p.11 n.32.

<sup>3</sup> Norman 1978 p.36.

<sup>4</sup> On this evidence, see below pp.19-20.

canon, particularly the Vinaya and Sutta portions, is a reliable source for the early history of Indian Buddhism in the period before Aśoka.<sup>5</sup>

This version of events is not accepted by all, however. Gregory Schopen in particular has argued against the view that the canonical texts can be taken as accurate historical sources for the earliest period:

Scholars of Indian Buddhism have taken canonical monastic rules and formal literary descriptions of the monastic ideal preserved in very late manuscripts and treated them as if they were accurate reflections of the religious life and career of actual practising Buddhist monks in early India.<sup>6</sup>

This point of view has two aspects to it. On the one hand, normative religious literature must not be taken at face value, as if it contains evidence of real historical events. As Schopen puts it:

Even the most artless formal narrative text has a purpose, and that in “scriptural” texts, especially in India, that purpose is almost never “historical” in our sense of the term.<sup>7</sup>

On the other hand, Schopen doubts that texts preserved in ‘very late manuscripts’ contain accurate historical evidence – he wishes us to believe that the canonical texts cannot be taken as evidence for the period before the fifth century A.D.:

We know, and have known for some time, that the Pāli canon as we have it – and it is generally conceded to be our oldest source – cannot be taken back further than the last quarter of the first century B.C.E, the date of the Alu-vihāra redaction, the earliest redaction that we can have some knowledge of, and that – for a critical history – it can serve, at the very most, only as a source for the Buddhism of this period. But we also know that even this is problematic since, as Malalasekera has pointed out: “...how far the *Tipiṭaka* and its commentaries reduced to writing at Alu-vihāra resembled them as they

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<sup>5</sup> It is unlikely that the Abhidharma works of various schools were fixed at this date. See below p.15.

<sup>6</sup> Schopen p.3.

<sup>7</sup> Schopen p.3.

have come down to us now, no one can say.” In fact, it is not until the time of the commentaries of Buddhaghosa, Dhammapāla, and others – that is to say, the fifth to sixth centuries C.E. – that we can know anything definite about the actual contents of this canon.<sup>8</sup>

A central theme running through Schopen’s work is his claim that we cannot know anything for sure about Indian Buddhism from its texts that were redacted in the fifth century A.D. (for the Pāli canon), or the fourth century A.D. (approximately, for the canonical material of various sects preserved in Chinese translations). Consequently, Schopen believes that the only way we can find out anything about Buddhism before this time is through accurately dated epigraphical and archaeological material. It is clear from Schopen’s work that this evidence has not been given the attention it deserves; it is vitally important to study the material remains, which tell us something concrete about what Buddhists were doing at particular places in particular times. But does this mean that we should concentrate exclusively on the material remains? Should we throw out the texts, or merely allow their evidence to be restricted and subordinated to the material evidence? The impression given by Schopen’s work is that the study of early Buddhism can only progress by subordinating the literary evidence to the material evidence, an approach which seems to have become standard in some quarters. But before we consign ourselves to a radical reorientation in the study of early Buddhism, we should critically examine some of the presuppositions of this approach. There seem to be three questions of importance here:

- 1) How old are the canonical texts?
- 2) Are the canonical texts purely normative, or do they include descriptive material which can be used to reconstruct historical events?
- 3) And finally, how much importance is to be assigned to the epigraphical and archaeological evidence?

I radically disagree with Schopen’s answers to each of these questions. In what follows, I hope to show why Schopen’s views are untenable, and I will argue that the

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<sup>8</sup> Schopen pp.23-24.

only way of knowing anything about early Buddhism is through its texts. I will begin with the last point first: it seems to me that the worth of the epigraphical and archaeological evidence has been overstated by Schopen. This is not to deny its great importance for the study of Indian Buddhism – without it, the historian is fumbling in the dark, and his conclusions will lack verisimilitude. Be that as it may, the material evidence has its own limitations, and the fact is that it does not tell us that much about the thought and practices of Buddhists in ancient India. So although Schopen has used this evidence to draw attention to hitherto neglected aspects of Indian Buddhism (e.g. that monks and nuns probably instigated the cult of the image, or that monks and nuns were involved in the *stūpa* cult from the earliest times), he does not acknowledge the fact that this does not tell us very much about Indian Buddhism as it was practised. It does not allow us to probe very far into the beliefs and practices of Buddhist monks and nuns in India; its content is limited, much more limited than the content of the early texts, which seem to me to contain a wealth of information on the diverse beliefs current in early Buddhism. According to Schopen, the epigraphical material ‘[t]ells us what a fairly large number of Indian Buddhists actually did, as opposed to what – according to our literary sources – they might or should have done.’<sup>9</sup> What exactly Schopen has in mind when he says ‘a fairly large number of Indian Buddhists’ is unclear, but certainly misleading: the relevant inscriptions number only a few thousand, which is evidence, surely, for the activity of a small minority of monks and nuns. They can hardly be taken as indicative of the activity of the Buddhist populace at large – just over a couple of thousand inscriptions does not, to my mind, represent a large number of Indian Buddhists, considering that this must have been a tiny fraction of the number of Indian Buddhists from about 400 B.C. to 500 A.D.

In other words, there is a tendency in Schopen’s work to make generalisations about Indian Buddhism based on a very small amount of evidence. Even if the generalisations were true in every respect, it would only reveal the historical reality of a tiny part of Indian Buddhism. Perhaps if there were epigraphical evidence representing

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<sup>9</sup> Schopen p.56. See also Schopen’s comments, p.71 n.50: ‘We do know, however, that from the very beginning of our actual epigraphical evidence (Bhārhut, Sāñcī, etc.), a large number of monks were doing

every Buddhist who existed in ancient India, it would be similar to the evidence of the extant inscriptions. But we cannot presume what is not there. For all we know, the inscriptions might represent only a small minority of the ancient Indian Saṅgha, the minority who had personal wealth and who could endow Buddhist institutions in different ways.

In this situation, we should not underestimate the worth of the textual evidence, even if its antiquity cannot be established accurately. For example, Schopen records that two inscriptions at Mathurā record the donations of monks who are called *prāhaṇīka-s*, ‘practisers of meditation’.<sup>10</sup> But without consulting the evidence of the Pāli canon for the word *padhāna* or the Buddhist Sanskrit evidence for the word *pradhāna/prahāṇa* (or variations on them), we would have absolutely no idea what the term signified for the two monks, and why they used it. The fact is that the texts are indispensable: the literary evidence, even if only normative, and even if it was periodically revised until the rather late redactions, is most certainly a useful record, not to be used as subsidiary to the material evidence, as Schopen believes, but in tandem with it, so that the two sorts of evidence are used equally. In short, if the inscriptions are to have any significance for the study of early Indian Buddhism, they must be considered alongside the canonical evidence, as has been argued by Hallisey:

It will only be after we have learned to combine our interest in “what really happened” with a sensitivity to the changing thought-worlds of the Theravāda that we will begin to discern the historical reality behind the literary and archaeological traces of ancient Buddhist monasticism.<sup>11</sup>

It seems to me that Schopen’s work is most convincing when he follows this method, and uses the literary, epigraphical and archaeological sources equally,<sup>12</sup> instead

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exactly what the data indicate they were doing at Ajaṇṭā.’

<sup>10</sup> Schopen p.31.

<sup>11</sup> Hallisey p.208.

<sup>12</sup> See in particular his article ‘Monks and the Relic Cult in the *Mahāparinibbāna-sutta*: An Old Misunderstanding in Regard to Monastic Buddhism’ (= Schopen 1997 pp.99-113).

of just dismissing the literary evidence out of hand.<sup>13</sup> Unfortunately, in his eagerness to point out that the studies of previous generations of Buddhist scholars were one-sided, Schopen has created another one-sided version of history. What is needed is a balanced approach that gives both sets of evidence, the literary and material, their due worth.

But what is the worth of the literary evidence? This brings me to two of the questions posed above, viz. the age and nature of the canonical texts. Schopen's position on these two points is quite clear, as we have seen, although it is strange that he does not give any evidence to support his view that the narrative Buddhist literature is 'almost never historical', as if this were a self-evident fact. As for his point that we cannot know if the canonical material is old, he attempts to demonstrate this by claiming that the general method of higher criticism – the method which is often used to prove the antiquity of canonical texts – is inapplicable. He sums up this method of higher criticism as follows: '[I]f all known sectarian versions of a text or passage agree, that text or passage must be very old; that is, it must come from a presectarian stage of the tradition.'<sup>14</sup> The alternative explanation of the agreement of 'all known sectarian versions of a text or passage' is that the agreement was produced by the different sects sharing literature at a later date. It is this hypothesis which Schopen attempts to prove by showing that the similar versions of the story of the *stūpa* of Kāśyapa at Toyikā, found in Mahāsaṅghika, Mahīśāsaka, Dharmaguptaka and Theravādin texts, are later than versions found in the Mūlasarvāstivādin Vinaya and in the Divyāvadāna.<sup>15</sup> The former group of texts claim that the Buddha manifested a *stūpa* momentarily, after which a *stūpa* was built (by monks) or appeared. The version of the story in the Mūlasarvāstivādin Vinaya and in the Divyāvadāna, however, is described by Schopen as follows: 'Firstly, it has none of the various subplots found in the other versions – a fairly sure sign of priority – and, second, it knows absolutely nothing about a *stūpa* at Toyikā or its construction.'<sup>16</sup> Schopen's main argument then is that the story in the Mūlasarvāstivādin Vinaya and the

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<sup>13</sup> See in particular his article 'Two Problems in the History of Indian Buddhism: The Layman/Monk Distinction and the Doctrines of the Transference of Merit' (= Schopen 1997 pp.23-55), parts of which I will consider below.

<sup>14</sup> Schopen pp.25-26.

<sup>15</sup> Schopen pp.28-29.

<sup>16</sup> Schopen p.29.

Divyāvadāna is earlier because it does not mention a *stūpa*: ‘This version, in short, reflects a tradition – apparently later revised – that only knew a form of the relic cult in which the *stūpa* did not yet have a part.’<sup>17</sup>

The first thing which I find odd about Schopen’s assessment of this story is his claim that, on the basis of the evidence in the Mūlasarvāstivādin Vinaya and Divyāvadāna, there was a form of the relic cult that did not include the *stūpa*. The narratives in these texts mention *caitya*-s, and although Schopen states that this term has nothing to do with *stūpa*-s, this is not at all clear. In his article ‘The *Stūpa* Cult and the Extant Pāli *Vinaya*’,<sup>18</sup> he has in fact argued that in the Pāli literature, the word *cetiya* is equivalent to *stūpa*.<sup>19</sup> It could easily be the case that the word has the same meaning in the Mūlasarvāstivādin Vinaya and the Divyāvadāna. But even if not, are we to accept a form of relic worship without a *stūpa*? If we take the canonical texts seriously, it is hard to imagine that this could ever have been the case. The *Mahāparinibbāna Sutta*, for example, states that the Buddha’s relics are to be contained in a *stūpa*,<sup>20</sup> which suggests that the *stūpa* goes back to the very beginning of Buddhism. The *stūpa* was certainly a feature of Buddhism by the time of Aśoka, who records in his Nigālī Sāgar Pillar Edict that twenty years into his rule, he had the *thuba* of Konākamana doubled in size.<sup>21</sup> Moreover, Aśoka seems to have known a portion of the text found in the Sanskrit version of the *Mahāparinirvāṇa Sūtra* – in his Rummindei inscription, he records that he visited Lumbini and worshipped there saying ‘Here the Blessed One was born’,<sup>22</sup> which corresponds to the Sanskrit version of the *Mahāparinirvāṇa Sūtra* (41.8: *iha bhagavān*

<sup>17</sup> Schopen p.29.

<sup>18</sup> Schopen pp.86-98.

<sup>19</sup> Schopen pp.89-91.

<sup>20</sup> See D II.142.5ff: ...*cātummahāpathe rañño cakkavattissa thūpaṃ karonti. evaṃ kho Ānanda rañño cakkavattissa sarīre paṭipajjanti. yathā kho Ānanda rañño cakkavattissa sarīre paṭipajjanti evaṃ tathāgatassa sarīre paṭipajjitabbaṃ. cātummahāpathe tathāgatassa thūpo kātabbo*; and D II.164.28: *aham pi arahāmi bhagavato sarīrānaṃ bhāgaṃ, aham pi bhagavato sarīrānaṃ thūpañ ca mahañ ca karissāmi*. The Sanskrit *Mahāparinirvāṇa Sūtra* edited by Waldschmidt also mentions *śarīrastūpa*-s in portions of text which correspond to these Pāli references: 36.7 corresponds to D II.142.5, 50.5 corresponds to D II.142.5. The compound *śarīrastūpa* also appears at 46.7, 50.16, 50.20, 51.9, 51.22.

<sup>21</sup> Hultsch p.165: (A) *devānaṃpiyena piyadasina lājina chodasavasā[bh]i[si]t[e]n[a], budhasa konākamanasa thube dutiyaṃ vaḍhite*.

<sup>22</sup> Hultsch p.164: (A) ...*atana āgācha mahīyite hida budhe jāte sakyamunī ti*.

*jātaḥ*).<sup>23</sup> This part of the text is close to the parts in the Pāli and Sanskrit versions which mention *stūpa-s*, and so it seems natural to conclude that *stūpa* worship was not only a part of Buddhism at this date, but also that it was mentioned in canonical Buddhist texts at this point. This is an important point, for according to the most plausible theory of sect formation (the theory proposed by Frauwallner), some of the Sthavira sects formed as a result of the Aśokan missions in 250 B.C. (see below p.11 ff). If the Aśokan evidence suggests that by about this time the *stūpa* was a feature of Buddhism and its texts, a pre-sectarian period that did not relate relic worship to the construction of *stūpa-s* is hardly plausible. It seems that there are no obvious reasons for taking the story in the Mūlasarvāstivādin Vinaya and Divyāvadāna to be older than the versions in the Mahāsaṅghika, Mahīśāsaka, Dharmaguptaka and Theravādin texts.

However, even if Schopen has got it right and his argument is valid, it actually shows that the Pāli canon was closed to material received from other sects. What Schopen fails to mention is that the method of higher criticism used to establish old strata in the Buddhist literature usually compares the canonical literature of different sects: he is reluctant to note that the Pāli version of the story of the *stūpa* of Kāśyapa at Toyikā is found in the *Dhammapada-aṭṭhakathā* – this information is conveniently confined to footnote 28. This means that if Schopen is correct, it seems then that whereas some of the other sects periodically shared literature and changed their canonical material in the sectarian period, the Theravādins of Sri Lanka did not: they confined the received material to non-canonical books. It seems that Schopen might have inadvertently proved that the Pāli canon was relatively closed after its redaction at an early date. This depends of course on whether or not he has interpreted the different versions of the story about the *stūpa* at Toyikā correctly, and this is far from clear. A thorough study of the different versions of the story is surely necessary. However, it is worth taking a short digression to show that another inadvertent proof of the antiquity of at least the Suttapiṭaka is given by Schopen in the very same article (‘Two Problems in the History of Indian Buddhism: The Layman/Monk Distinction and the Doctrines of the Transference of Merit’).

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<sup>23</sup> Waldschmidt p.388. The Pāli version is only slightly different: *idha tathāgato jāto ti* (D II 140.20 = A II 120.24).



In this article, Schopen shows that the belief in the transference of merit was widespread in India from the third century B.C. onwards (pp.34-42). Thus, a late Mauryan/early Śuṅga inscription from Pauni, a few inscriptions from third century B.C. Sri Lanka, a singular early inscription from Bhārhut, as well as a significant number of later Hīnayāna inscriptions from various parts of India all record the idea. If the idea was a standard belief of Buddhists in early times, even in Sri Lanka, and if the Suttapiṭaka was not finally closed until the Alu-vihāra recension in the fifth century A.D., then it is reasonable to expect that it should be well attested in the Suttapiṭaka. But this is not the case – although much is said on the subject of meritorious activity, the idea of the transference of merit is found in only two separate occurrences in the four principle Nikāya-s.<sup>24</sup> How can we explain the fact that Theravāda Buddhists of Sri Lanka did not compose more texts which included the idea of merit transference? There can only be one answer – the texts were closed in an earlier period, when the belief was marginal in Buddhist circles. This is surely the only answer to the problem. Even if this does not definitely prove that the canon was closed at an earlier date, the fact that the ancient guardians of the Suttapiṭaka did not compose texts on the transference of merit shows that they must have had some idea of canonical orthodoxy, which means that the canon must have been relatively fixed. By attempting to show that the canonical texts are not reliably old, and that we must turn to the epigraphic evidence to gain any idea about the historical reality of ancient Indian Buddhism, Schopen has inadvertently shown that some collections of texts must indeed be old and contain evidence for the period before most of the inscriptions.

Exactly the same fact emerges from Schopen's article 'The *Stūpa* Cult and the Extant Pāli Vinaya'. He attempted to show that '[t]he total absence of rules regarding *stūpas* in the Pāli *Vinaya* would seem to make sense only if they had been systematically removed',<sup>25</sup> meaning that the Pāli canon was altered '[a]t a comparatively recent date', after the supposed recensions made in the first century B.C. and the fifth century A.D.

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<sup>24</sup> D II 88.28ff = Ud 89.20 = Vin I 229.35; A V.269-73. On these passages see Gombrich 1971 p.267 and p.272.

<sup>25</sup> Schopen p.91.

This argument is based upon the fact that all the other extant Vinayas include rules concerning the construction and cult of the *stūpa*, whereas the Pāli Vinaya does not. There are two possible explanations for this fact. Either it is because the Pāli Vinaya was closed before these rules were formulated, or it is because these sections were written out of the Pāli Vinaya, accidentally or on purpose; Schopen chooses the latter option. But Gombrich and Hallisey have shown that this interpretation is based on a mistranslation of the twelfth century Sinhalese inscription, the *Mahā-Parākramabāhu Katikāvata*.<sup>26</sup> It therefore seems likely that the other solution to the problem is correct – the Pāli Vinaya was closed before this section was composed and added to the other Vinayas. Gombrich notes: ‘One does not have to posit that it received no further additions after the first century B.C., merely that the Pali tradition had left the mainstream and naturally failed to record later developments on the Indian mainland.’<sup>27</sup> But because we know that the Pāli tradition remained in contact with the Indian mainstream (it received texts from north India after the first century B.C.), I think it more likely that no further additions were made after the first century B.C.

The points Schopen makes about the post-canonical sharing of literature, the transference of merit, and the Pāli Vinaya’s evidence on *stūpa*-s, if correctly interpreted, suggest that the Pali canon was relatively fixed from at least the first century B.C. onwards. This is despite the fact that the Pāli tradition remained in contact with other Buddhist sects in India, as has been noted already by scholars such as Oldenberg and Norman. According to Norman, ‘[s]ome of the best known stories in Buddhism ... are known in the Theravādin tradition only in the commentaries, although they are found in texts which are regarded as canonical in other traditions.’<sup>28</sup> Such stories must have reached Sri Lanka before Buddhaghosa, for he includes them in his commentaries. But

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<sup>26</sup> Gombrich 1990 pp.141-142, Hallisey pp.205-206. It seems to me that Hallisey has made it clear that: ‘Buddhaghosa, Sāriputta, and the other *ākācariyas* did not include the observances concerning *stūpas* and *bodhi* trees among the observances specified in the Vinaya itself’ (p.205). This does not explain the passage in the Visuddhimagga quoted by Schopen p.88, which still presents difficulties about the exact meaning of the compound *khandhakavattāni*, although Hallisey notes: ‘Perhaps it grouped a range of practices according to their family resemblances, rather than by their common origin in specific parts of the *Vinaya*.’ (p.206).

<sup>27</sup> Gombrich 1990 p.143.

<sup>28</sup> Norman 1997 p. 140.

why were they not inserted into the canon? Norman thinks that it was because ‘[a]t least the Vinaya- and Sutta-piṭaka had been closed at an earlier date.’<sup>29</sup> Norman has also pointed out that certain Pāli works for which a North Indian origin is supposed, such as the *Milindapañha*, the *Peṭakopadesa* and the *Nettipakaraṇa*, are highly respected by the commentators but are not given canonical status by them. They even contain ‘[a] number of verses and other utterances ascribed to the Buddha and various eminent theras, which are not found in the canon ... [T]here was no attempt made to add such verses to the canon, even though it would have been a simple matter to insert them into the Dhammapada or the Theragāthā.’<sup>30</sup> The point that the Pāli tradition received literature from other sects but excluded it from the canon had been made already by Oldenberg in 1879: ‘These additions are by no means altogether unknown to the Singhalese church, but they have been there placed in the Aṭṭhakathās, so that the text of the Tipiṭaka, as preserved in Ceylon, has remained free from them.’<sup>31</sup> This suggests that they arrived in Sri Lanka ‘[a]fter the closure of the Canon’.

If we remind ourselves of Norman’s point that the Pāli canon contains no definite evidence for a substantial amount of Sinhalese prakrit (see above p.1), it seems quite clear that after the Tipiṭaka was written down in the first century B.C., it was not substantially altered, at least in content, and as such, it must have been very similar to the extant Pāli Canon. This means that the Suttapiṭaka in existence today can be taken as an accurate record of Buddhist thought from the time of the Buddha (c. 484-404 B.C.) until the first century B.C. at the latest.<sup>32</sup> This is significantly older than Schopen is willing to acknowledge, but the *terminus ante quem* can be pushed back even further; it depends upon the date when the Pāli texts reached Sri Lanka, i.e. the date at which the sectarian period began.

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<sup>29</sup> Norman 1997 p.140.

<sup>30</sup> Norman 1997 p.140.

<sup>31</sup> Oldenberg 1879, p.xlviii.

<sup>32</sup> Accepting Richard Gombrich’s dating of the Buddha: ‘... the Buddha died 136 years before Aśoka’s inauguration, which means in 404 B.C.’ (1992 p.246). Gombrich estimates the margin of error to be 7 years before to 5 years after this date, i.e. 411-399 B.C. (p.244). But he also notes that uncertainty about the date of Aśoka widens the margin of error, making the upper limit 422 B.C. K.R. Norman comments: ‘If we take an average, then the date is c.411 ± 11 B.C.E.’ (Norman 1999 p.467).

According to Schopen, '[w]e do not actually know when the sectarian period began.'<sup>33</sup> To support this view he cites Bureau's work which points out that the Buddhist sects all give different dates for the schisms.<sup>34</sup> But he does not make any mention of what is probably the most convincing work on the subject. Erich Frauwallner, in *The Earliest Vinaya and the Beginnings of Buddhist Literature*, used a mixture of epigraphical and literary sources to argue that some of the Sthavira sects owed their origination to the missions said to have taken place in the reign of Aśoka, c.250 B.C. Firstly, there are records in the Sinhalese chronicles (and the *Samantapāsādikā*) of a series of Buddhist missions which went out to different parts of India and neighbouring kingdoms in the reign of Aśoka. Although these Pāli accounts as we have them do not seem plausible – they might have been embellished to trump up the monastic lineage from which the Pāli texts issued<sup>35</sup> – the missions are confirmed by the inscriptions found on a couple of reliquaries unearthed in the ancient Buddhist centre of Vidiśā. According to Frauwallner, these reliquaries contain the remains of the Hemavata masters Dudubhisara, Majjhima and Kāssapagotta, names which he identified with the missionaries Durabhisara, Majjhima and Kassapagotta, all of whom travelled to the Himavanta according to the chronicles.<sup>36</sup> Willis has recently pointed out that Frauwallner misread this evidence slightly by mistaking the relics of Gotiputa, heir of Dudubhisara, for Dudubhisara himself,<sup>37</sup> but at the same time he has argued that all five names on the two different reliquaries correspond to the five names in the chronicles.<sup>38</sup> It strongly implies that the missionaries to the Himavanta hailed from Vidiśā and that some of their relics were returned there some time after their death. The chronicles also record that Mahinda's mother was from Vidiśā, and that he stayed there before journeying to Sri Lanka.<sup>39</sup> This is an impressive correspondence of epigraphical and literary evidence, and it makes it almost certain that the account of the missions in the Pāli chronicles contains some historical truth.

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<sup>33</sup> Schopen p.26.

<sup>34</sup> Schopen p.26 on A. Bureau, *Les sectes bouddhiques du petit véhicule* (Paris, 1955).

<sup>35</sup> See below pp.19-20 on the notion that the *thera* Moggaliputta sent the missions.

<sup>36</sup> Frauwallner 1956 pp.13-14.

<sup>37</sup> Willis p.226 n.26.

<sup>38</sup> Willis pp.222-23.

Frauwallner equated this epigraphic and literary evidence with further epigraphic evidence from Aśoka's thirteenth Rock Inscription: on pp.15-17 of *The Earliest Vinaya*, he noted that the areas mentioned in this edict, to which he despatched emissaries, correspond to the areas of missionary activity mentioned in the Pāli chronicles. Both sources, according to him, mention the North-West, West and South but omit the East, and he comments 'This is certainly no freak chance.' Lamotte's table (p.302) shows at least a superficial agreement between the places mentioned in both sources, but Gombrich is probably correct in commenting: 'The geographical identifications are too uncertain to help us'.<sup>40</sup> With the geographical identifications uncertain, Lamotte was sceptical of the notion that there was one concerted missionary effort in Aśokan times. He argued that the Buddhists were natural missionaries and would have spread Buddhism throughout India from the beginning.<sup>41</sup> Thus he concluded his study of the early Buddhist missions by stating 'Whatever might have been said, Aśoka was not directly involved in Buddhist propaganda.'<sup>42</sup> Gombrich, on the other hand, agrees with Frauwallner, and notes:

While Lamotte is right to point out that some of the areas visited, notably Kashmir, had Buddhists already, that does not disprove that missions could not be sent there. The chroniclers, as so often happens, had no interest in recording a gradual and undramatic process, and allowed history to crystallize into clear-cut episodes which could be endowed with edifying overtones; but this over-simplification does not prove that clear-cut events never occurred.<sup>43</sup>

The notion that there was a clear-cut missionary episode in the spread of Buddhism across India seems to be confirmed by the epigraphical record. L. S. Cousins has surveyed the references to the sects in inscriptions (pp.148-51), and noted that the related Vibhajjavādin sects (the Vibhajjavādin-s made up a subset of the ancient Sthavira-s) were most widespread of all Buddhist sects in the first few centuries C.E. On the other

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<sup>39</sup> Dīp XII.35ff, Mhv XIII.18-20.

<sup>40</sup> Gombrich 1988 p.135.

<sup>41</sup> Lamotte p.297.

<sup>42</sup> Lamotte p.308.

<sup>43</sup> Gombrich 1988 p.135.

hand, the other sects were distributed randomly across India. This is exactly what is to be expected if there was a gradual diffusion of Buddhism throughout India, as well as a concerted missionary effort by one ancient monastic community, which thereafter separated into separate sects due to the geographical dispersal. Cousins comments on the tradition of the Buddhist missions as follows:

It seems clear that whatever the traditions about these [missions] may or may not tell us about events in the third or second century BCE, they do certainly correspond to what we know of the geographical spread of the schools early in the first millennium CE. They must then have *some* historical basis. Vibhajjavādins really were the school predominant in Ceylon and Gandhāra at an early date, as well as being present, if not predominant, in other parts of Central Asia, China, South India and South-East Asia by around the turn of the third century CE at the latest. No other school has a comparable spread at this date.<sup>44</sup>

It seems then that there is no reason to doubt that there was some sort of mission in the third century B.C. which set out from Vidiśā to the far North-West, West and South of India. Frauwallner thought that this missionary activity founded the Sarvāstivādin sect in the North-West, as a result of Majjhantika's mission to Kaśmīr and Gandhāra,<sup>45</sup> whereas Cousins considers only the Vibhajjavāda sects in the North-West and South. Was the Sarvāstivādin sect of the North-West produced by a missionary effort that otherwise seems to have produced only Vibhajjavādin sects? This is certainly possible. Frauwallner made it quite clear that the formation of distinct communities ought to be distinguished from schools of thought: '[f]rom the first we have stressed the principle that the foundation of communities and the rise of dogmatic schools are two quite separate things.'<sup>46</sup> This led him to conclude that the dogmatic affiliation of the Mūlasarvāstivādin and Sarvāstivādin sects came later than the original foundation of

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<sup>44</sup> Cousins p.169.

<sup>45</sup> Frauwallner 1956 p.22: 'The mission of Kassapagotta, Majjhima and Dundubhissara gave origin to the Haimavata and Kāśyapīya. The mission of Majjhantika led to the rise of the Sarvāstivādin. The Dharmaguptaka school is perhaps issued from the mission of Yonaka-Dhammarakkhita.'

<sup>46</sup> Frauwallner 1956 p.38.

these two as monastic communities.<sup>47</sup> It is possible then that different dogmatic affiliations could have been produced in the sects founded by missionary activity, and that the dogmatic affiliation to *sarvāstivāda* ideas by the community that came to be known as the ‘Sarvāstivādin’ sect came about later. This seems to be shown by the fact that the literature of the Sarvāstivādin sect is in many regards similar to the literature of the other Vibhajjavādin sects.

In the beginning of *The Earliest Vinaya*, Frauwallner notes that the Skandhaka section of the Sarvāstivādin, Dharmaguptaka, Mahīśāsaka and Pāli Vinaya-s are ‘strikingly close’; the Skandhaka of the Kāśyapīya school is not considered because it has not survived.<sup>48</sup> According to Frauwallner then, the Sarvāstivādin Skandhaka is closer to the Skandhaka-s of sects known to be Vibhajjavādin in affiliation than it is to the Mūlasarvāstivādin Skandhaka. Elsewhere, Frauwallner has noted that the Sarvāstivādin Abhidharma contains much that ‘[w]as held in common with the Pāli school.’<sup>49</sup> And the Śāriputrābhidharma, which according to Frauwallner is a Dharmaguptaka text, is also a development of the same material inherited by the Sarvāstivādin and Pāli schools.<sup>50</sup> This is again in contrast with the Abhidharma of the Mūlasarvāstivādins, which according to Frauwallner ‘[p]ossessed only one Māṭṛkā.’<sup>51</sup> The canonical literature of the sect in the North-West that came to be known as the Sarvāstivādin-s is therefore closer to the Vibhajjavādin sects, particularly the literature of the Pāli tradition.

It seems likely that all these sects share a common antecedent, which we can think of as the ancient Sthavira community of Vidiśā. Nevertheless, it is striking that only one of the sects produced by the missions adopted *sarvāstivāda* ideology, whereas the others seem to have been affiliated to the *vibhajjavāda*. I think the best explanation of the evidence is that this *sarvāstivādin* development must have occurred later on within the community founded by Majjhantika in Kaśmīr-Gandhāra. Originally, the Abhidharma

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<sup>47</sup> He says this on p.38 in his discussion about the difference between Sarvāstivādin and Mūlasarvāstivādin, but it applies in general to his thought on the dogmatic affiliation of all the sects resulting from the missions.

<sup>48</sup> Frauwallner 1956 p.2.

<sup>49</sup> Frauwallner 1995 p.37.

<sup>50</sup> See the chapter on the Śāriputrābhidharma in Frauwallner 1995.

literature of the missionary community was less fixed than its Sūtra and Vinaya sections. It allowed considerable room for development. In Aśokan times the dogmatic outlook of the missionary community was *vibhajjavāda*, but at a later date, *sarvāstivāda* ideology came to dominate in Kaśmīr. Incidentally, the Mūlasarvāstivādin school, originally from Mathurā according to Frauwallner, came to exist in the North-West and claimed that it originated from the mission of Madhyāntika.<sup>52</sup> It is plausible to think that this claim – almost certainly an interpolation into the Mūlasarvāstivādin Vinaya, as Frauwallner has shown – was taken from the old Sarvastivādin-s of the North-West, and used as part of the Mūlasarvāstivādin strategy of claiming supremacy in the North-West. This claim would only have been borrowed if it was thought to be true, so it seems that yet another piece of evidence supports the accuracy of the Theravādin tradition of the missions.

The evidence for a Sthavira mission taking place in the third century B.C., probably from a school that was *vibhajjavādin* in the dogmatic sense, is very good. But was this mission related to Aśoka? Despite Lamotte's doubts, I think that the Aśokan inscriptions show that this must have been the case. The confusion on this point seems to have been caused by a failure of previous scholars, especially Lamotte, to distinguish Aśoka's references to his 'Dharma-ministers' (*dharma-mahāmattā*) from the evidence contained in the thirteenth Rock Edict. Lamotte's table on the sources of the missions (p.302) sums up the evidence, presented on the previous page, of the second Rock Edict, the fifth Rock Edict and the thirteenth Rock Edict. But RE II has nothing to do with missionary activity – nor does RE V, which mentions the *dharma-mahāmattā*. In fact every mention of the *dharma-mahāmattā* limits them to Aśoka's Kingdom, and so they should have been, for according to the inscriptions they were involved in all sorts of activities which might be called 'social welfare', and which cannot have been carried out

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<sup>51</sup> Frauwallner 1956 p.39.

<sup>52</sup> Frauwallner 1956, pp.26-31, especially p.31: 'We come thus to the conclusion that the episode of Madhyāntika and of the conversion of Kaśmīr represents a late interpolation in the Vinaya of the Mūlasarvāstivādin.' In the light of Frauwallner's work, the Sarvāstivāda inscriptions of Mathurā mentioned by Lamotte, p.523, might be those of the school that in the North-West came to be known as the Mūlasarvāstivādin-s. See the appendix for a further consideration of the Sarvāstivādin/Mūlasarvāstivādin issue.



in other kingdoms.<sup>53</sup> Socially beneficial activity in other kingdoms is mentioned in RE II, which states that Aśoka provided medicines and medical herbs for men and cattle, and had wells dug,<sup>54</sup> but this falls well short of the activities of the *dhaṇṇma-mahāmattā*. Whether or not Aśoka really carried out such beneficial acts in neighbouring kingdoms – it might have been an exaggeration of his own righteous endeavours – this activity can hardly have been carried out by his *dhaṇṇma-mahāmattā*. In the same way, any missionary activity initiated by Aśoka cannot have been undertaken by his *dhaṇṇma-mahāmattā*. This brings us to RE XIII: Aśoka tells us that he has achieved a ‘Dharma-victory’ (*dhaṇṇma-vijaya*) in his own kingdom as well as others, and even in places where his ‘envoys’ (*dūta-s*) have not gone.<sup>55</sup> From this we can see that this victory must have been achieved by his envoys – *dūta-s*, not *dhaṇṇma-mahāmattā*. Who were these envoys then, sent by Aśoka to the border areas of this kingdom, as well as to neighbouring kingdoms, through which he attained his victory of *dhaṇṇma*?

The obvious answer is that they were the people responsible for taking medicines and medicinal herbs to other Kingdoms, and for having wells dug there, mentioned in RE II. Supporting this idea is the fact that the areas outside Aśoka’s Kingdom mentioned in RE XIII and RE II are almost identical: in RE XIII the kingdoms mentioned are those of Aṃtiyoka, the Yonaraja, and his four neighbours in the North-West, as well as the Choḍa-s, Paṃda-s and Taṃbapaṇṇiya-s in the South; in RE II, the Satiyaputra-s and Keraḍaputra-s are added to the list of southern kingdoms, and the neighbours of Aṃtiyoka are not named. This is almost an identical correspondence. Nevertheless, it is problematic to think that the *dūta-s* mentioned in RE XIII were merely carriers of medicines and supervisors of well-digging. After all, Aśoka says that through them he has achieved his *dhaṇṇma-vijaya*: can a ‘Dharma-victory’ have been achieved by the implementation of some social policies? In other words, would socially beneficial acts undertaken by Aśoka in his kingdom and elsewhere have induced him to claim that he had achieved a

<sup>53</sup> They are mentioned in RE V, RE XII and PE VII; it would have been beyond the jurisdiction of a visitor to another kingdom to carry out some of these duties.

<sup>54</sup> RE II (Shahbazgarhi, Hultzsche p.51): (A) ...*du[vi] 2 chik[i]sa [kr]i[ta] manuśa-chikisa... pa[śu-ch]ikisa [cha]* (B) *[o]sha[ḍha]ni manuśopakani cha paśopakani cha yat[r]a yatra nasti savatra harapita cha vuta cha* (C) *kupa cha khanapita pratibh[o]gaye paśu-manuśanaṃ*.

<sup>55</sup> RE XIII (Shahbazgarhi, Hultzsche p.68/211): (S) *yatra pi Devanaṃpriyasa duta na vrachamti...*

Dharma-victory? One might think that this is possible, because the sort of *dharma* promoted by the Dharma-ministers was exactly this sort of socially beneficial action. But against this idea is the fact that in PE VII at Delhi-Toprā, the same sort of socially beneficial deeds are declared to be ‘[of little consequence]. For with various comforts have the people been blessed both by former kings and by myself.’<sup>56</sup> After this declaration, Aśoka states that he has merely provided material needs so that the people will conform to the practice of *dharma*.<sup>57</sup> We have then a distinction between Aśoka’s ideal of *dharma* and socially beneficial policies which might be called *dharma*. Which of the two meanings of *dharma* did Aśoka mean by the word in the compound *dharma-vijaya*? I find it unlikely that Aśoka would have proclaimed a *dharma-vijaya* because of his social policies, which in PE VII he claims are of little value. It is much more likely that when Aśoka spoke of his *dharma-vijaya*, he had in mind a victory of *dharma* in its higher meaning of a set of ethical practices and attitudes. In the Delhi-Toprā edict, this ideal of *dharma* is outlined as follows:

(FF) King Devānāmpriyadarśin speaks thus:

(GG) Whatsoever good deeds have been performed by me, those the people have imitated, and to those they are conforming. (HH) Thereby they have made progress and will (be made to) progress in obedience to mother and father, in obedience to elders, in courtesy to the aged, in obedience to Brāhmaṇas and Śramaṇas, to the poor and distressed, (and) even to slaves and servants.<sup>58</sup>

This ideal of the *dharma* is outlined in more or less the same fashion by Aśoka in RE III, IV, IX, XI, and crucially, in RE XIII, the edict in which he claims his *dharma-vijaya*.<sup>59</sup> The natural conclusion is that the dissemination of these ideas (and their implementation) is what Aśoka had in mind when he claimed his *dharma-vijaya*. This

<sup>56</sup> Hultzsch’s translation, p.135, of PE VII (p.132): (U) [la]... esa paṭbhoge nāma (V) vividhāyā hi sukhāyanāyā pulimehi pi lājīhi mamayā cha sukhayite loke.

<sup>57</sup> PE VII, Hultzsch p.132: (W) imaṃ chu dharmānupaṭpatī anupaṭpajanti ti etadathā me esa kaṭe.

<sup>58</sup> Hultzsch’s translation, p.136, of PE VII (p.133): (FF) Devānāmpriye [P...s. l]ājā hevaṃ āhā (GG) yāni hi [k]ānichi mamiyā sādhanāni kaṭāni taṃ loke anūp[a]ṭpaṃme taṃ cha anuvīdhiyanti (HH) tena vaḍhitā cha vaḍhisanti cha mātā-pit[is]u sususāyā gulusu sususāyā vayo-mahālakānaṃ anupaṭpatiyā bābhana-samanesu kapana-valākesu āva dāsa-bhaṭakesu saṃpaṭpatiyā.

<sup>59</sup> In RE XIII, the crucial passage outlining his *dharma* is found in section G, Hultzsch p.67/208.

‘victory’ was the spread of ideals such as respect to *śramaṇa-s* and *brāhmaṇa-s*, obedience to mother and father, courtesy to slaves and servants etc. If the envoys who took these ideals to the distant corners of Aśoka’s kingdom and beyond were not Dharma-ministers, who were they? Are we to believe that Aśoka had a class of officials who went out and taught what are essentially religious ideals? This is hardly likely. The more plausible answer is that the *dūta-s* included the professional religious men and women to whom Aśoka was partial, i.e. the Buddhists. In other words, it is likely that the envoys who spread Aśoka’s ideals included Buddhist monks and nuns.<sup>60</sup> There is even some indication in the Sinhalese chronicles that is indeed what happened. In chapter XI, the Mahāvamsa describes how envoys were sent by Aśoka to King Devānaṃpiyatissa of Laṅkā:

33. The Lord of Men [Aśoka], having given a palm-leaf message (*paṇṇākāraṃ*) at the appropriate time for his friend [Devānaṃpiyatissa], sent envoys (*dūte*) and this palm-leaf message concerning the true doctrine (*saddhammapaṇṇākāraṃ*), [which said:]

34. “I have taken refuge in the Buddha, the Dhamma and the Saṅgha, I have indicated that I am a lay disciple in the instruction of the Son of the Sakya-s.”

35. “O Best of Men, you too, having satisfied your mind with faith, should take refuge in these supreme jewels.”

36. Saying: “Carry out the consecration of my friend once more,” having honoured his friend’s ministers, he despatched them.<sup>61</sup>

There is no mentions of Buddhist monks and nuns in the imperial embassy of *dūta-s*, but the implication is that if there were contacts such as this between Aśoka and his neighbouring kings, then Buddhists must have been involved, or would have followed soon afterwards. This is also indicated by the Dīpavaṃsa which, although including a standard account of the missions sent by Moggaliputta (at VIII.4-13), also includes three

<sup>60</sup> Erich Frauwallner related the Buddhist missions to Aśoka precisely because of the reference to *dūta-s* in RE XIII (1956 p.15 n.1). He did not mention the evidence in the chronicles for the *dūta-s* of Aśoka, however.

<sup>61</sup> Mhv XI.33-36: *datvā kāle sahāyassa paṇṇākāraṃ narissaro, dūte pāhesi saddhammapaṇṇākāraṃ imam pi ca* (33). “*ahaṃ buddhañ ca dhammañ ca saṅghañ ca saraṇaṃ gato, upāsakattaṃ desesiṃ sakyaputtassa sāsane* (34), *tvam pi ’māni ratanāni uttamāni naruttama, cittam pasādayitvāna saddhāya*

accounts of the *dūta-s* sent to Laṅkā by Aśoka, each account describing how Mahinda arrived in Laṅkā soon after the envoys, without any mention of Moggaliputta. These accounts imply that the envoys paved the way for the Buddhist monks who followed. In the most elaborate account, Dīp XII.8ff, after describing how Aśoka sent gifts and a request that Devānaṃpiya of Laṅkā should have faith in the triple jewel, it says that the *thera-s* of the Asokārāma requested that Mahinda establish the faith in Laṅkā.<sup>62</sup> And at Dīp XI.41 and Dīp XVII.91-92, it says that Mahinda arrived in Laṅkā one month after the Aśokan envoys, without any mention of Moggaliputta.<sup>63</sup> It seems that the author of the Sinhalese chronicles, as well as Buddhaghosa, had various sources available to them recording different versions of the mission to Sri Lanka.<sup>64</sup>

The version that eventually became the orthodox account was of course the one that had Moggaliputta as the organiser of the missions. But the accounts in the Dīpavaṃsa that do not mention Moggaliputta seem much more plausible in the light of the evidence from RE XIII. It is clear that some of the information in the chronicles is accurate: the name of the missionary monks, for instance, as the evidence at Sāñchī indicates. But the Sthavira tradition from which the missions came could hardly have made the monks merely part of Aśoka's ministerial envoys, or even following in the wake of these envoys – they probably felt that they had to exaggerate the prominence of their tradition with the idea that Moggaliputta sent them.<sup>65</sup> We can conclude that the imperial envoys (*dūta-s*) of Aśoka, which for him had effected a *dhaṅṅma-vijaya*, probably did include Buddhist monks. By welcoming these envoys, and heeding Aśoka's written requests that they take refuge in the triple jewel, the neighbouring kings

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*saraṇaṃ vaja*” (35). “*karotha me sahāyassa abhisekaṃ puno*” *iti, vatvā sahāyāmacce te sakkaritvā ca pesayi* (36).

<sup>62</sup> Dīp XII.8: *asokārāme pavare bahū therā mahiddhikā, laṅkātalānukampāya mahidaṃ etad abravuṃ* (8). *Samayo laṅkādīpamhi patiṭṭhāpetu sāsanaṃ, gacchatu vaṃ mahāpuñña pasāda dīpalaṅjakaṃ* (9).

The expression *gatadūtena te saha* at the end of v.7 is ambiguous. Oldenberg reads it with what follows in v.8 and translates ‘As soon as the messengers had departed’ (p.168). It must mean that the elders of the Asokārāma requested Mahinda to go to Laṅkā as soon as the envoys had been sent.

<sup>63</sup> Dīp XI.41: *tayo-māse atikkamma jeṭṭhamāse uposathe, Mahindo sattamo huvā jambudīpā idhāgato*. Dīp XVII.91cd-92ab: *dutiyābhiseke tassatikkantā tiṅsarattiyo, mahidogaṇa pāmokkho jambudīpā idhāgato*.

<sup>64</sup> As Norman points out (1983 p.118).

<sup>65</sup> Although there is every possibility that Moggaliputta, *thera* of the Asokārāma, aided Aśoka in organising the missions.

maintained good relations with the mighty Indian emperor, and Aśoka himself propagated *dharma*.<sup>66</sup>

The result of this long digression into the evidence for the Aśokan missions is that it appears that Frauwallner was correct to relate the evidence of the Sinhalese chronicles and inscriptions of Vidiśā to Aśoka's RE XIII. We can therefore date the arrival of the canonical texts in Sri Lanka to the middle of the third century B.C. Frauwallner has shown, however, that the lower limit of the early Buddhist literature can be pushed back even further. He noted that the Mahāsaṅghikas had a version of the old *Skandhaka*, meaning that it must have been composed before the schism between themselves and the Sthaviras, which certainly occurred before the Aśokan missions. The Mahāsaṅghika Vinaya also includes the account of the second council at Vaiśālī: the old *Skandhaka* must have been composed, or at least redacted, after this council, and before the schism between the Sthaviras and Mahāsaṅghikas.<sup>67</sup> Frauwallner thought that the old *Skandhaka* was probably composed shortly before or after this council,<sup>68</sup> which Gombrich reckons that it took place about 60 years after the Buddha's death, c. 345 B.C.<sup>69</sup> If this is correct, it means that very sophisticated literary tracts were being composed little more than half a century after the Buddha's death.

There is great significance in these investigations for the date of texts contained in the Suttapiṭaka. According to Frauwallner:

At the time of the compilation of the old *Skandhaka* work ...<sup>70</sup> the Buddhist tradition had already reached an advanced stage of development. A collection of sacred scriptures, including Dharma and Vinaya, was already in existence. The Vinaya included

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<sup>66</sup> On the idea that Moggaliputta sent out the missions, Frauwallner states: '...we must remember that the data of the Sinhalese chronicles are uncertain on this point.' (1956 p.17). He concludes: 'The mother community tried apparently to enhance the glory of its patriarch by putting on his merit the sending out of the missions.' (1956 p.18).

<sup>67</sup> Frauwallner 1956 p.54.

<sup>68</sup> Frauwallner 1956 p.67: 'It must have been composed shortly before or after the second council'.

<sup>69</sup> Gombrich 1992 p. 258: 'We may thus date the Second Council round 60 A.B. or round 345 B.C.; the dates are very approximate and the precise margin of error incalculable...'

the Prātimokṣa, narratives of the type of the *Vibhaṅga* and much material on the monastic rules, which the Buddha was said to have communicated to his disciples. The collection of Sūtra, which existed on its side, was handed down by a regular machinery of transmission, and we can ascertain a number of texts which belonged to it already in that period.<sup>71</sup>

One Sūtra text which Frauwallner singles out is the *Aṭṭhakavagga* (Sn IV), for the same story mentioning it is preserved in all the extant Vinayas; it probably belonged to the old Sūtra collection. It is therefore possible that much of what is found in the Suttapiṭaka is earlier than c.250 B.C., perhaps even more than 100 years older than this. If some of the material is so old, it might be possible to establish what texts go back to the very beginning of Buddhism, texts which perhaps include the substance of the Buddha's teaching, and in some cases, maybe even his words. I have no intention of going into the important but complex question of what the Buddha did or did not teach. In the following, I will address the two questions posed on p.3, but I will at least attempt to show that some of the details of the Buddha's biography, namely those which record some of his activities as a Bodhisatta, have recorded accurate historical information about events that happened in the fifth century B.C. This will show that a careful use of textual sources is the only way to know anything about Buddhism in the pre-Aśokan period, and will lead to the conclusions that, contrary to what Schopen thinks, some material in the Suttapiṭaka is historically accurate and extremely old.

Various Suttas describe the Buddha's visits to the sages Āḷāra Kālāma and Uddaka Rāmaputta, although the source for the account is probably the *Ariyapariyesana Sutta* (APS, M no.26).<sup>72</sup> Andre Bareau has translated a Chinese Sūtra that corresponds to the APS as well as an account found in the Chinese version of the Dharmaguptaka

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<sup>70</sup> At this point Frauwallner dates the old Skandhaka according to older views about the date of the second council, c.100 years after the Buddha's death. More recent research has modified this date somewhat; I follow Gombrich's date of c.345 B.C. for the second council.

<sup>71</sup> Frauwallner 1956 p.153.

<sup>72</sup> The Suttas including this account are the *Mahā-Saccaka Sutta* (M no.36), the *Bodhi-Rājakumāra Sutta* (M no.85) and the *Saṅgārava Sutta* (M no.100).

Vinaya.<sup>73</sup> There are also versions of the narrative in the Mahāsaṅghika Mahāvastu<sup>74</sup> and the Mūlasarvāstivādin Saṅghabhedavastu.<sup>75</sup> It seems that the account of the training under the two teachers was embedded in the pre-sectarian Buddhist tradition, that is, if one accepts the idea that corresponding parts of the sectarian literature are likely to be pre-sectarian. There is also material on the two teachers scattered throughout the Suttapiṭaka. Scholars have generally accepted Bareau's opinion that the tradition of the two teachers' instruction to the Bodhisatta was a fabrication,<sup>76</sup> but more recently, Zafiropulo has shown that Bareau's arguments are fallacious.<sup>77</sup> If we are to take the tradition seriously, as we must do in the light of Zafiropulo's comments, we must also take into consideration the fragmentary information about the two teachers that is scattered throughout the Suttapiṭaka. I hope to show that a re-evaluation of the data on the two teachers makes two things quite clear. Firstly, some of the information on the two teachers cannot have been shared at a later date – it must reflect a pre-sectarian tradition. And secondly, a peculiar detail in the account of the Bodhisatta's training under the teachers shows that the two men must have existed. They must have been teachers of some repute in the fifth century B.C. in northern India, teachers of meditation who probably taught the Bodhisatta.

To show the former point, I will consider the information found in various sources concerning the location of Uddaka Rāmaputta. Hsüan tsang mentions some legendary evidence that relates Udraka Rāmaputta to Rājagṛha; it seems that this represents the local tradition of the Buddhists living in the area of Rājagṛha.<sup>78</sup> This tradition is confirmed by the account of the Bodhisatta's training in the Mahāvastu, which also

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<sup>73</sup> Bareau pp.14-16.

<sup>74</sup> Mvu II.118.1ff.

<sup>75</sup> SBhV I.97.4ff; Skilling points out that there is a Tibetan translation of this SBhV account, as well as a 'virtually identical' Mūlasarvāstivādin version, preserved in the Tibetan translation of the *Abhiniṣkramaṇa-Sūtra* (Skilling p.101).

<sup>76</sup> Vetter p.xxii, Bronkhorst p.86; Bareau sums up his view as follows: 'Personnages absents, morts même avant que leurs noms ne soient cités, ils sont probablement fictifs. Plus tard, on s'interrogea sur ces deux mystérieux personnages et l'on en déduisit aisément qu'ils n'avaient pu être que les maîtres auprès desquels le jeune Bodhisattva avait étudié.' (pp.20-21).

<sup>77</sup> Zafiropulo pp.22-29.

<sup>78</sup> *Si-Yu-Ki* (Beal, Part II p.139ff).

places Udraka Rāmaputra in Rājagṛha.<sup>79</sup> The coincidence between these two sources might have been reached in the sectarian period. There is, however, similar evidence in the Suttapiṭaka which makes it almost certain that the tradition must be presectarian. In the *Vassakāra Sutta*, the Brahmin Vassakāra, chief minister of Magadha, is said to visit the Buddha in Rājagaha and tell him that the *rājā Eḷeyya* has faith in the *samaṇa* Rāmaputta; the commentary names him as Uddaka Rāmaputta.<sup>80</sup> Vassakāra also appears in the *Mahāparinibbāna Sutta* as the chief minister of King Ajātasattu of Magadha.<sup>81</sup> Vassakāra's connection with Rājagaha and Magadha suggest that the *rājā Eḷeyya* was a local chieftain in Magadha, probably situated somewhere near to Rājagaha. If so, it is likely that Uddaka Rāmaputta was situated in the vicinity of Rājagaha. The coincidence of this different evidence from the Theravādin and Mahāsaṅghika sources, as well as the information of Hsüan tsang, is not to be overlooked. It is inconceivable that this correspondence was produced by a later leveling of texts, for it is entirely coincidental – different source materials, not corresponding Suttas, state or imply the same thing. It is hardly likely that a Mahāsaṅghika monk or nun gained knowledge of obscure Pāli Suttas, from which he deduced that Uddaka Rāmaputta must have been based in Rājagaha, and after which he managed to insert this piece of information into the biographical account in the Mahāvastu. And it is even more unlikely that a Theravādin Buddhist, in the early centuries A.D., studied the Mahāsaṅghika Vinaya, from which he learnt that Udraka Rāmaputra was based in Rājagṛha, following which he fabricated Suttas which contained circumstantial evidence which indirectly related Rāmaputta to Rājagaha. Anyone who believes this version of textual history is living in cloud-cuckooland. It is clear that the information on the geographical situation of Uddaka Rāmaputta must precede the Aśokan missions, and even the schism between Sthavira-s and Mahāsaṅghika-s. This implies that the biographical tradition of the training under the two teachers goes back to the very beginning of Buddhism. It surely means that accurate historical information has been preserved, and suggests that Uddaka Rāmaputta was based in Rājagaha, no doubt as a

<sup>79</sup> Mvu II.119.8.

<sup>80</sup> Mp III.164.23: *samaṇe rāmaputte ti uddake rāmaputte*.

<sup>81</sup> D II.72.9ff = A IV.17.11ff (*Sattakanipāta, anusayavagga, XX*). He also appears in the *Gopakamogallāna Sutta* (M III.7ff), which is set in Rājagaha. At Vin I 228 (= D II 86.31ff, Ud 87), he and Sunīdha are in charge of the construction of Pāṭaligāma's defences.



famous sage of Magadha. Incidentally, it is clear that this material has no normative value whatsoever, and so rebuts Schopen's claim that 'even the most artless formal narrative text' has a normative agenda.

Another detail, found in almost all the sectarian accounts of the training under the two teachers, can hardly have been produced by a later leveling of the Buddhist literature; it occurs in the account of the training under Uddaka Rāmaputta. This account is identical in almost all regards to the description of the training under Āḷāra Kālāma. It tells us that the Bodhisatta first of all mastered the teaching, i.e. he gained an intellectual understanding of it,<sup>82</sup> after which he attained the direct realisation of the sphere of 'neither perception nor non-perception' through understanding (*abhiññā*).<sup>83</sup> But the account of the training under Uddaka Rāmaputta makes it clear that it was not Uddaka Rāmaputta who had attained the sphere of neither perception nor non-perception, but Rāma, the father or spiritual teacher of Uddaka.<sup>84</sup> This is seen in the following exchange. The Bodhisatta is said to have contemplated that Rāma (not Rāmaputta) did not proclaim (*pavedesi*) his attainment through mere faith, but because he dwelt (*vihāsi*) knowing and seeing himself.<sup>85</sup> The corresponding passage in the account of the training under Āḷāra uses the same verbs in the present tense (*pavedeti*, *viharati*), indicating that Āḷāra was living and Rāma was dead, and that Rāmaputta had not attained and realised the *dhamma* he taught.

The same phenomenon is found in the rest of the passage. Thus the Bodhisatta is said to have asked Rāmaputta: 'The venerable Rāma proclaimed (*pavedesī*) [his

<sup>82</sup> M I.165.22ff: *so kho ahaṃ bhikkhave nacirass' eva khippam eva taṃ dhammaṃ pariyāpuṇiṃ. so kho ahaṃ bhikkhave tāvataken' eva oṭṭhapahatamattena lapitalāpanamattena ñāṇavādaṃ ca vadāmi theravādaṃ ca, jānāmi passāmīti ca paṭijānāmi ahañ c' eva aññe ca.*

<sup>83</sup> M I.166.4ff: *...yan nūnāhaṃ yaṃ dhammaṃ Rāmo sayaṃ abhiññā sacchikatvā upasampajja viharāmī ti pavedeti, tassa dhammassa sacchikiriyāya padaheyyan ti? so kho ahaṃ bhikkhave nacirass' eva khippam eva taṃ dhammaṃ sayaṃ abhiññā sacchikatvā vihāsiṃ.*

<sup>84</sup> Skilling discusses this in detail; the point had been made earlier by Thomas p.63 and Ñāṇamoli and Bodhi p.258 n.303.

<sup>85</sup> M I.165.27ff: *na kho rāmo imaṃ dhammaṃ kevalaṃ saddhāmattakena sayaṃ abhiññā sacchikatvā upasampajja viharāmī ti pavedesi, addhā rāmo imaṃ dhammaṃ jānaṃ passaṃ vihāsī ti.*

attainment], having himself realised this *dhamma* to what extent (*kittāvata*)?’<sup>86</sup> The reply, of course, is as far as *nevasaññānāsaññāyatana*. The Bodhisatta is then said to have contemplated that not only did Rāma have faith, energy, mindfulness, concentration and insight, but that he too possesses these virtues. And at the end of the episode, Uddaka Rāmaputta is reported to have said: ‘Thus the *dhamma* that Rāma knew (*aññāsi*), that *dhamma* you [the Bodhisatta] know; the *dhamma* you know, that *dhamma* Rāma knew.’<sup>87</sup> This is different from the corresponding speech that Āḷāra is reported to have made to the Bodhisatta: ‘Thus the *dhamma* I know (*jānāmi*), that *dhamma* you know; the *dhamma* you know, that I know.’<sup>88</sup> And whereas Āḷāra is willing to establish the Bodhisatta as an equal to him (*samasamaṃ*), so that they can lead the ascetic group together (*imaṃ gaṇaṃ pariharāmā ti*),<sup>89</sup> Uddaka acknowledges that the Bodhisatta is equal to Rāma, not himself (*iti yādiso rāmo ahosi tādiso tuvaṃ*), and asks the Buddha to lead the community alone (*imaṃ gaṇaṃ pariharā ti*).<sup>90</sup>

The distinction between Uddaka Rāmaputta and Rāma is also found in the Sarvāstivādin, Dharmaguptaka, and Mahāsāṅghika accounts of the Bodhisattva’s training.<sup>91</sup> Although the Saṅghabhedavastu (plus parallel Tibetan translations) and the Lalitavistara fail to distinguish Rāmaputta from Rāma,<sup>92</sup> this must be because of a later obfuscation of the tradition. Exactly the same mistake has been made by I. B. Horner, the PTS translator of the Majjhima Nikāya, who has been duped, by the repetitive oral style, into believing that the accounts of the training under Āḷāra and Uddaka must be the same

<sup>86</sup> M I.165.32ff: *kittāvata no āvuso rāmo imaṃ dhammaṃ sayamaṃ abhiññā sacchikatvā upasampajja* [VRI: *viharāmīti*] *pavedesī ti?*

<sup>87</sup> M.I.166.22ff: *iti yaṃ dhammaṃ rāmo aññāsi, taṃ tvaṃ dhammaṃ jānāsi; yaṃ tvaṃ dhammaṃ jānāsi, taṃ dhammaṃ rāmo aññāsi.*

I leave *dhammaṃ* untranslated here because it indicates the meditative sphere attained by both Rāma and the Buddha. Before this, the Buddha is said to have mastered the *dhamma* intellectually (165.24 = 164.4-5; see n.68), which can hardly mean a meditative attainment and must refer to an intellectual understanding.

<sup>88</sup> M.I.165.3ff: *iti yāhaṃ dhammaṃ jānāmi, taṃ tvaṃ dhammaṃ jānāsi; yaṃ tvaṃ dhammaṃ jānāsi, tam ahaṃ dhammaṃ jānāmi.*

<sup>89</sup> M I.165.5ff: *iti yādiso ahaṃ tādiso tuvaṃ, yādiso tuvaṃ tādiso ahaṃ. ehi dāni āvuso ubho va santā imaṃ gaṇaṃ pariharāmā ti. iti kho bhikkhave āḷāro kālāmo ācariyo me samāno antevāsiṃ maṃ samānaṃ attano samasamaṃ thapesi, uḷārāya ca maṃ pūjāya pūjesi.*

<sup>90</sup> M I.166.24ff: *iti yādiso rāmo ahosi tādiso tuvaṃ, yādiso tuvaṃ tādiso rāmo ahosi. ehi dāni āvuso tvaṃ imaṃ gaṇaṃ pariharā ti. iti kho bhikkhave udako rāmaputto sabrahmacārī me samāno ācariyaṃ thāne ca maṃ thapesi, uḷārāya ca maṃ pūjāya pūjesi.*

<sup>91</sup> See Skilling, pp.100-102.

apart from the difference between the names of the two men and their meditative attainments.<sup>93</sup>

It hardly needs to be pointed out that there is no need to trouble over these details in an oral tradition where adjacent passages are often composed in exactly the same way, one passage frequently being a verbatim repetition of the previous one with a minor change of one or two words. The tendency for reciters of this autobiographical episode would have been to make the two accounts identical bar the substitution of Uddaka's name for Āḷāra's. A conscious effort has been made to distinguish Uddaka Rāmaputta from Rāma, and not to let the repetitive oral style interfere with this. This effort must surely go back to the beginning of the pre-sectarian tradition of composing biographical Suttas, and the distinction can only be explained if Rāma and Rāmaputta were two different people. Otherwise, it is part of an elaborate hoax, and there is no reason for such a hoax.

Bareau maintained that the correspondence between the two descriptions of the training under each of the teachers proved their artificial (i.e. unhistorical) nature.<sup>94</sup> But repetition is normal in Pāli oral literature. And it seems that the two parallel accounts, having preserved the important distinction between Rāmaputta and Rāma, rather than leaving an impression of 'contrivance', have preserved valuable historical information. The conclusion is that the three men were real.<sup>95</sup> It is hardly likely that Buddhists got together a few hundred years after the Buddha's death and decided to make up the idea that Rāma and not Rāmaputta had attained the state of neither perception nor non-

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<sup>92</sup> Skilling p.101.

<sup>93</sup> Horner pp.209-10. Jones (p.117), translator of the Mahāvastu, preserves the distinction between Rāma and Rāmaputta, but fails to notice that in the Mahāvastu, Rāmaputta does not establish the Bodhisattva as an equal to him: it says that he established the Bodhisattva as the teacher (Mhv II 120.15: *ācāryasthāne sthāpaye*). Jones translates: 'Udraka Rāmaputta ... would make me a teacher on an equal footing with himself' (p.117).

<sup>94</sup> Bareau p.20: 'Mais le parallélisme avec l'épisode suivant, l'ordre trop logique et le choix trop rationnel des points de doctrine d'Ārāḍa Kālāma et d'Udraka Rāmaputta nous laissent un arrière-goût d'artifice qui nous rend ces récits suspects.'

<sup>95</sup> Zafiropulo (p.25) does not point out the difference between Rāma and Rāmaputta, but on the stereotyped description of the training under the two teachers he comments: 'Justement cela nous semblerait plutôt un signe d'ancienneté, caractéristique de la transmission orale primitive par récitations psalmodiées'.

perception, and then had such an influence that the idea found its way into recensions of texts being made in regions as far apart as central Asia and Sri Lanka. The idea must have been in the Buddhist tradition from the beginning, and can only be explained as an attempt to remember an historical fact. There is no other sensible explanation. It is also worth pointing out that if this biographical material is so old and really does represent an attempt to record historical facts, then it means that this portion of the Bodhisatta's biography is most likely to be true. It is likely that the Bodhisatta really was taught by Āḷāra Kālāma and Uddaka Rāmaputta.

## ***Conclusion***

At the beginning of this paper, I argued that no matter how necessary the epigraphical and archaeological evidence is, it has its own limitations, a fact which ought not to be overlooked by exaggerating its worth at the expense of the literary evidence. I attempted to demonstrate this by pointing out what seem to me to be a few flaws in the work of Gregory Schopen, a scholar who pursues exactly this line of thought. I argued that some of the epigraphical sources cited by him show that the Pāli Canon must have been closed at a relatively early date. After that, I considered the arguments put forward by Frauwallner and others about the tradition that there was an expansion of Buddhism during Aśoka's reign. I argued that Lamotte conflated the evidence of RE XIII with that of RE II and V, and confused the activity of the *dhaṇḍma-mahāmattā* with activity of Aśoka's envoys (*dūta-s*) mentioned in RE XIII. After reconsidering the evidence of RE XIII, and the evidence from the eleventh chapter of the Mahāvamsa, I concluded that the tradition of the Buddhist missions in Aśoka's time is relatively accurate. This means that much of the material in the Pāli Canon, especially the Sutta and Vinaya portions, reached Sri Lanka at around 250 B.C. And finally, I attempted to show that some of the information preserved in the literature of the various Buddhist sects shows that historical information about events occurring in the fifth century B.C. has been accurately preserved. The corresponding pieces of textual material found in the canons of the different sects (especially the literature of the Pāli school, which was more isolated than the others) probably go back to pre-sectarian times. It is unlikely that these

correspondences could have been produced by an endeavour undertaken in the sectarian period, for such an endeavour would have required organisation on a scale which was simply inconceivable in the ancient world. We can conclude that a careful examination of the early Buddhist literature can reveal aspects of the pre-Aśokan history of Indian Buddhism. The claim that we cannot know anything about early Indian Buddhism because all the manuscripts are late is vacuous, and made, I assume, by those who have not studied the textual material properly.

#### Appendix: The Sarvāstivādin and Mūlasarvāstivādin sects

The name ‘Mūlasarvāstivādin’ is most peculiar – as far as I know, no other Buddhist sects in India sect prefixed the word *mūla-* to their sect name. There were no ‘Mūla-Dharmaguptaka-s’ or ‘Mūla-pudgalavādin-s’, for example. It is hard to explain why any community would have prefixed the word *mūla* to their sect name: it seems to me that this peculiarity can only have arisen in the context of a sectarian debate, for which there are only two possible scenarios. Either the two communities were originally unrelated: one community who accepted *sarvāstivāda* ideas, and who were probably known as ‘Sarvāstivādin-s’, had an argument with another Sarvāstivādin group. Prefixing the word *mūla-* to their sect name by one of the groups would have been part of a strategy of claiming that their community was the real source of *sarvāstivāda* ideology, part of their argument that they were the original or ‘root’ Sarvāstivādin-s. Alternatively, the Mūlasarvāstivādin-s were an offshoot from the Sarvāstivādin-s, a sort of reforming group who used the prefix *mūla-* for the same reason.

Étienne Lamotte, however, proposed a different solution: he dismissed Frauwallner’s theory about the difference between Sarvāstivādin and Mūlasarvāstivādin, by claiming, without presenting any corroborating evidence, that the Mūlasarvāstivādin Vinaya was simply the version of the Sarvāstivādin Vinaya completed at a later date in Kaśmīr (p.178). This explanation, however, leaves too many questions unanswered. For example, if the Mūlasarvāstivādin Vinaya was a later recension of an earlier

Sarvāstivādin Vinaya, then why is it so different? And why did the sect who revised the work change their name from Sarvāstivādin to ‘Mūlasarvāstivādin’? The only answer to the last objection to Lamotte’s thesis is that the name was changed to ‘Mūlasarvāstivādin’ by the reforming sect who expanded the old Sarvāstivādin Vinaya, and who thus used the prefix *mūla* in order to differentiate themselves from the old Sarvāstivādin-s and create a new sect, i.e. an explanation which corresponds to the second of my proposed solutions above. But there is no clear evidence for the theory that the Mūlasarvāstivādin Vinaya is an expansion of the Sarvāstivādin Vinaya and that the Mūlasarvāstivādin-s were an offshoot of the Sarvāstivādin-s.<sup>96</sup> We must therefore look for a different answer to the problem, an answer along the lines of my proposed first solution above, i.e. that two Sarvāstivādin groups came into contact and had a dispute. Such an explanation would simply be a reworking of Frauwallner’s hypothesis – he proposed two different communities, one from Kaśmīr and one from Mathurā, both of whom came to accept *sarvāstivādin* ideas, but he did not state that they had a dispute.<sup>97</sup> I am proposing that the Mathurā school moved to Kaśmīr and disputed with the existing community there, and I think we can detect such a dispute in the Chinese works mentioned by Lamotte pp.174-75. First of all, however, I will show that these works cited by Lamotte support Frauwallner’s theory.

Lamotte did not offer any explanation of the peculiar fact that Kumārajīva, the fifth century translator of the *Mahāprajñāpāramitopadeśa*, knew of the existence of two different Vinaya-s, one from Mathurā and one from Kaśmīr. According to Kumārajīva, the Vinaya of Kaśmīr contained 10 sections, and we can deduce that this was the Sarvāstivādin Vinaya, for he was himself from Kaśmīr and translated the Sarvāstivādin Vinaya into Chinese in 404-05 A.D. (Lamotte p.168). Moreover, the Sarvāstivādin Vinaya was also known as the ‘Vinaya in ten sections’ (*Daśādhyāya*, Lamotte p.168). This means that it is likely that what he calls the Vinaya of Mathurā in 80 sections was

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<sup>96</sup> According to Frauwallner, ‘[t]he Vinaya of the Sarvāstivādin largely agrees with the Vinaya of the other missionary schools and forms with them a close group, while the Vinaya of the Mūlasarvāstivādin shows considerable differences.’ (1956 p.38).

<sup>97</sup> Frauwallner 1956 p.40: ‘They were at first two independent communities of different origin... Later on both communities grew into one school through their accepting of the theories of the philosophical-dogmatic school; but they never completely lost their individualities.’

the Mūlasarvāstivādin Vinaya, that is, if the information of Kumārajīva and the two other Chinese authors mentioned by Lamotte (p.175) definitely concerns the Sarvāstivādin and Mūlasarvāstivādin traditions of Kaśmīr in the fifth century A.D. All three Chinese works mentioned by Lamotte (pp.174-75) knew that the original Vinaya (of Upāli/Kāśyapa) consisted of 80 sections. Sêng yu and Hui Chao related this Vinaya to the patriarchal lineage ending in Upagupta, the fifth patriarch of the Buddhist community of Mathurā.<sup>98</sup> Kumārajīva went further and stated that the Vinaya in 80 sections was that of the community in Mathurā. So we have good reasons to suppose that two Vinaya-s – and therefore two monastic communities, in some way similar – were known in Kaśmīr, and that one had come from Mathurā. Lamotte's theory simply brushes over this fact, whereas Frauwallner adduces good evidence to show that the Mūlasarvāstivādin sect's connection with Kaśmīr is late, and written onto an earlier church history of Mathurā (1956 pp.26-36).

The dispute between the adherents of these two Vinaya-s is just about detectable in the words of Kumārajīva and Sêng yu. Kumārajīva, taught in the tradition of the Sarvāstivādin Vinaya, tells us that the Vinaya of Kaśmīr (i.e. the Sarvāstivādin Vinaya) had only ten sections, but that it also had a Vibhāṣa consisting of 80 sections. Why did he do this? Why did he state that the Vinaya has a commentary consisting of 80 sections? It might be a redundant statement, but I think that the evidence suggests otherwise. From his words (Lamotte p.174), it seems that he was aware of the claim that the original Vinaya consisted of 80 sections. He was also aware of the fact that the Vinaya of Mathurā, probably the Vinaya of a very old Buddhist community, consisted of 80 sections. Surely his statement is that of an apologist, forced into making it because there were others who criticised the Sarvāstivādin Vinaya of Kaśmīr for lacking the full 80 sections.

We can see the nature of such a critique in Sêng yu's *Ch'u san tsang chi chi*: he states that the Vinaya with 10 sections was a reduction of the Vinaya in 80 sections, undertaken by Upagupta for the sake of those with 'weak faculties'. Sêng yu is blatantly

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<sup>98</sup> Frauwallner 1956 pp.28-31.

polemical, arguing against the worth of the Sarvāstivādin tradition. Thus, Kumārajīva stated that there is nothing amiss with the fact that one Vinaya (his Vinaya) had only 10 sections, whereas Sêng yu said that there was something deficient in it – it was an offshoot of the Vinaya in 80 sections for those with a weaker disposition. Kumārajīva was the apologist, asserting the antiquity of his Kaśmīrian Vinaya in the face of a rival, whereas Sêng yu was the inclusivist, attempting to include the Vinaya of Kaśmīr and its tradition within the tradition of Mathurā. It seems that in fifth century Kaśmīr, there was some quarrel between the adherents of two different communities – one original to Kaśmīr, which defended its position, and the other more recently arrived community which arguing that the Kaśmīr tradition was an offshoot of itself.

Why would two groups have clashed in this way? Surely there would have been no need for one community, when moving to another area, to attempt to usurp the position of the resident community. I suggest that everything makes sense if we accept that the disputed issue concerned the ownership of the *sarvāstivāda* idea: one group accepting *sarvāstivāda* ideas had moved from Mathurā to Kaśmīr, and there encountered another community which at some point adopted a similar *sarvāstivāda* ideology. In response to this, and considering itself to be the original source of the *sarvāstivāda*, it labelled itself the ‘Mūlasarvāstivāda’. If this argument, which I claim can be detected in the words of Kumārajīva and Sêng yu, was still fresh in the fifth century A.D., then it seems that the dispute broke out some time after the two groups had co-existed in the same area: Frauwallner noted that interpolations into the Mūlasarvāstivādin Bhaiṣajyavastu (indicating a relocation from Mathurā to the North West) were probably made between c.150-300 A.D.<sup>99</sup> Therefore, we can posit a period in which *sarvāstivāda* ideas circulated between the two groups. But it is more likely that the sect to whom the idea belonged at the beginning of this contact was the sect from Mathurā: the mission that led to the origination of Majjhantika’s community in Kaśmīr/Gandhāra was probably *vibhajjavādin* in the early period, and we can guess, from the name of the sect itself, as well as from Sêng yu’s aggressive stance and Kumārajīva’s seemingly defensive position, that the Mūlasarvāstivādin community was more irked by the dispute. All this is of

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<sup>99</sup> Frauwallner 1956 p.36.



course highly speculative, but if we are to explain the Mūlasarvāstivādin/Sarvāstivādin distinction as the result of sectarian dispute, then some explanation must be found.

It seems to me as if the Chinese works mentioned by Lamotte support Frauwallner's theory and the reworking of it I suggest here. At the least, they show that there was a problem in the Buddhist traditions of Kaśmīr concerning two different Vinaya-s, whereas Lamotte failed even to notice that this is a historical problem; Frauwallner's theory seems to be the best explanation of the evidence. More recently, Enomoto has recently argued that the Sarvāstivādin sect was no different from the Mūlasarvāstivādin – he argues that the two words mean the same thing, but this does not explain the odd facts: two different Vinaya-s, two similar names, and two explanations of their relationship in fifth century Chinese works. However, Enomoto's argument is flawed: it begins with the late and unreliable evidence of the fanciful etymology of the word *mūlasarvāstivādin* in Śākyaprabha's *Prabhāvatī* (c.8<sup>th</sup> century), as well as equally unreliable evidence in the colophons of this work and others by Śākyaprabha – all are inconsistent in the use of the prefix *mūla*.<sup>100</sup> He then attempts to show that Yi-jing used the words Sarvāstivāda and Mūlasarvāstivāda interchangeably.<sup>101</sup> But it seems to me that the section of Yi-jing translated by him does not support such a view. The important section reads:

(What are treated in) this (work)<sup>102</sup> mostly resemble the *Shi-song-lü*. The three different sects divided from the (Sarv)āsti(vāda) sect – 1. Dharmaguptaka; 2. Mahīśāsaka; 3. Kāśyapīya – are not prevalent in the five parts of India... However, the *Shi-song-lü* does not (belong to) the 'Mūla-(sarv)āsti(vāda)' sect, either.<sup>103</sup>

The proximity of the title *Shi-song-lü* and the (Sarv)āsti(vāda) sect in sentences one and two suggest that the former is the work of the latter, and from the last sentence, we know that this was not a work of the Mūlasarvāstivāda sect. The translation of Enomoto certainly does not say that “[t]he ‘Mūlasarvāstivāda’ sect was

<sup>100</sup> Enomoto pp.240-42.

<sup>101</sup> Enomoto p.243.

<sup>102</sup> This work being the Mūlasarvāstivādin Vinaya.

divided into four sects: Dharmaguptaka, Mahīśāsaka, Kāśyapīya and ‘Mūlasarvāstivāda’ itself”, or that “[w]hat is here called the (Sarv)āsti(vāda)’ sect is the same as the Mūlasarvāstivāda sect.”<sup>104</sup> On the contrary, it relates sects which, I have argued, originated from the same missionary endeavour, and distinguishes them from the Mūlasarvāstivādin-s. Enomoto’s theory does not make sense and it does not explain the difficulties. It seems then that Frauwallner’s explanation of the difference between Sarvāstivādin and Mūlasarvāstivādin explains most of the facts while leaving fewer unresolved problems. Some of the remaining problems I have attempted to solve by showing that the name ‘Mūlasarvāstivādin’ originated in the course of sectarian debate, and this seems to offer the best explanation of the various facts.

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<sup>103</sup> As translated by Enomoto pp.242-43.

<sup>104</sup> Enomoto p.243.

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