

PROBLEMS IN THE WRITING OF AJAṆṬĀ'S HISTORY:  
THE EPIGRAPHIC EVIDENCE

Over the past quarter century a revolution has occurred in the study of the Ajaṇṭā caves. Through the work of Walter M. Spink, the chronology of this Western Indian archaeological site has been condensed from the one hundred and fifty year span proposed by Fergusson and Burgess in 1880<sup>1</sup> to "nineteen separate annual levels,"<sup>2</sup> dating between c. 462 and 480 C.E.<sup>3</sup> Spink's so-called "short chronology" is now the most widely accepted model. Fergusson, Burgess, and Spink, as well as most other historians of Ajaṇṭā, have used the many epigraphical records left by the site's patrons as anchors for their work.<sup>4</sup> Indeed, the only explicit evidence for dating the site is found in Caves 16's and 17's dedicatory inscriptions, which link them to the reign of the fifth century Vākāṭaka ruler Hariṣeṇa.<sup>5</sup> Nevertheless, although Ajaṇṭā's inscriptions have played a central role in reconstructions both of the site's and the surrounding region's histories, the suitability of these records for that task has never been fully appraised.

Since 1963, readings, reconstructions, and translations of Ajaṇṭā's major inscriptions from V. V. Mirashi's *Inscriptions of the Vākāṭakas* have been universally used by scholars of Ajaṇṭā as adequate sources for historical information. But, whereas epigraphers have universally called attention to Ajaṇṭā's epigraphic material as in some areas "fallen off,"<sup>6</sup> or "so damaged as to render satisfactory translations impossible,"<sup>7</sup> or "completely destroyed by rain water,"<sup>8</sup> to my knowledge the site's chronologers have hardly acknowledged that such physical flaws might restrict the inscriptions' utility as historical evidence. The present paper will examine several important but damaged lines from Ajaṇṭā's major dedicatory inscriptions, in order to assess whether they are materially capable of providing a secure base upon which to reconstruct the site's history. Because the study of Ajaṇṭā's chronology is now virtually coterminous with the name and work of Walter Spink, I will focus upon the specific records that are most crucial for his reconstruction.

AJAṆṬĀ'S INSCRIPTIONS AS A BODY OF SOURCES

In 1836, James Prinsep was the first scholar to reproduce Ajaṇṭā's inscriptions in print.<sup>9</sup> Based upon eye-copies and crude ink-and-cloth estampages

provided him by early visitors to the site, Prinsep published records from Cave 2, Cave 17, and Cave 26, albeit without reading or commenting upon their content. Prinsep's work was followed in 1863 by Bhau Daji, who visited Ajañtā himself to read its records *in situ*. While there, Daji transcribed and translated the records Prinsep had only reproduced, as well as introducing many additional inscriptions for scholars' use. Principal among the epigraphs Daji copied "at a great height, and look[ing] down on giddy precipices,"<sup>10</sup> were incised dedicatory records from Caves 16, 17, and 26. These three are long, versified celebrations of the caves' patrons, their genealogies, social positions, and beliefs. Lacking these three documents, Ajañtā's history would be as mute as that surrounding the Buddhist caves at Pithalkhora, Aurangabad, and Bāgh. Following Daji, these important records were variously reread and retranslated by Bhagwanlal Indraji,<sup>11</sup> Georg Bühler,<sup>12</sup> B. Chhabra,<sup>13</sup> and finally V. V. Mirashi. Needless to add, not only did each scholar consider his work an improvement upon that of his prior colleagues, but as each new transcription and translation was added to the corpus, earlier work was set aside. This, despite the fact that the individual scholars' readings often conflict, occasionally with wide and significant variations. Thus, at present, Mirashi is the undisputed source for information about Caves 16 and 17, Chhabra for Cave 26.

Ajañtā boasts a total of ninety-seven inscriptions.<sup>14</sup> Of these, six derive from a period of activity spanning the first centuries B.C.E. to C.E. The remaining inscriptions may be subdivided further into two broad temporal groups, the Vākāṭaka and Rāṣṭrakūṭa.<sup>15</sup> It is the Vākāṭaka inscriptions that are of the greatest concern for dating Buddhist history at Ajañtā. The inscriptions from the Vākāṭaka period run the gamut from simple identification of a donor — *bhadanta-dharmadattasya*, "Belonging to Reverend Dharmadatta"<sup>16</sup> — to variously elaborated formulae describing donors, their gifts, and their motivations for giving; to labels that identify the figures in narrative paintings; to didactic verses from a popular literary text, Ārya Śūra's *Jātakamālā*;<sup>17</sup> to the verse inscriptions from Caves 16, 17, and 26 I introduced above. Based upon Spink's chronology (which I summarize below), Vākāṭaka period records may be broadly analyzed into two divisions: the programmatic and the intrusive. The importance of this dichotomy lies in its ability to segregate the strata of donative activities, synchronically across the entire site and diachronically within a single cave. Lacking this periodization, we would possess no clear vision of the patterns of patronage at Ajañtā.

It is within the Vākāṭaka *programmatic donative* inscriptions that we find

the most suggestive evidence for reconstructing Ajañtā's history. This set is comprised of the long records in verse that were incised on the porches of Caves 16,<sup>18</sup> 17,<sup>19</sup> and 26,<sup>20</sup> a brief, formulaic inscription cut into the stone on the pedestal beneath the main Buddha image in Cave 4,<sup>21</sup> and a very fragmentary record found on the left pilaster on the porch of Cave 20.<sup>22</sup> Although published transcriptions and translations of the Cave 4 and Cave 26 inscriptions leave some room for alternate readings, these two will not be treated in the present study: Cave 4's record offers little historical data and that from Cave 26 is remarkably well preserved. Instead, my discussion will focus upon prior readings and uses of the programmatic donative inscriptions from Caves 16, 17 and 20. These three are even more significant because they come from the section of the site that Ajañtā's chronologers have almost universally taken to be the nucleus of its Vākāṭaka renaissance: Cave 16 was the donation of Varāhadeva, a minister of Vākāṭaka Hariṣeṇa; Cave 17 was the donation of an unnamed king, feudatory of Hariṣeṇa; and Cave 20 is widely regarded as one of a complex of caves — comprised of 17, 18, 19, 20 and 29 — that are all the work of that same Vākāṭaka feudatory.<sup>23</sup>

#### A REVIEW OF SPINK'S CHRONOLOGY

Before turning to the inscriptions themselves, I will treat Spink's study of Ajañtā in brief. As scholars familiar with Spink's work will recognize, his method is a type of motival analysis that fathoms the site's artistic and architectural material in their own depths before setting them within a historical context based upon epigraphic and textual gleanings. Joanna Williams describes the methodology of motival analysis as follows:

Elements are isolated and placed in a convincing sequence of development in their own terms, on the supposition that it is more objective to deal systematically with parts than with the complex whole. That whole may, as a final stage, be dated on the basis of this relative sequence.<sup>24</sup>

Williams' characterization tallies almost directly with Spink's own methodological statement found in the 1991 article, "The Archaeology of Ajañtā." There, Spink envisions Ajañtā as an archaeological 'dig,' which "breaks into a number of distinct levels or strata, reflecting the way that its patronage was affected by political, economic, and other factors."<sup>25</sup> To analyze this dig, Spink begins by distinguishing individual strata, through identifications of the "morphological, iconographic, and technological features spreading

horizontally throughout the caves.”<sup>26</sup> This data provides the superstructure from which Spink’s relative, short chronology is constructed. The next order of business is to “make a vertical . . . survey, studying the way each separate feature emerges, evolves, is changed, or vanishes over time.”<sup>27</sup> By meshing the warp and woof of these horizontal and vertical analyses, Spink “weaves the complex tapestry of the site’s dramatic history.”<sup>28</sup> Finally, Spink proposes that this “mute” history can be brought to life through strategic use of “the written record — the inscriptions and narratives of these times.”<sup>29</sup> For Williams, dating is a final step that may (or may not) be performed by an art historian; but Spink’s ultimate concern is precisely with the site’s absolute chronology, its “many revealing connections with known historical events.”<sup>30</sup>

It may be possible to problematize or even contradict Spink’s reconstructed history without upsetting his short chronology. Indeed, if a challenge to the absolute chronology were to undermine the relative, this could suggest that the relative chronology itself was derived through a faulty methodology. I have spent over half-a-year living at Ajañṭā with Spink, discussing the caves daily, and have no cause to pronounce his relative sequence thus “contaminated.” Moreover, these *in situ* discussions and my own research have satisfied me concerning the argument presented in Spink’s 1975 article concerning Cave 16,<sup>31</sup> which fixes this relative sequence within, at most, the adult lifetime of a single man, that of the Vākāṭaka minister Varāhadeva. Nevertheless, to foreshadow my conclusions, I have found that the inscriptions Spink uses to give historical voice to Ajañṭā’s “abstract developmental sequence”<sup>32</sup> may not tell the same tale as his; Spink’s reconstruction of Ajañṭā’s history may be compromised by his too-strong reliance upon Mirashi’s translations of the Cave 16 and 17 inscriptions, which misread and over-interpret the available, fragmentary data.

Here is a synopsis of Spink’s story based upon a recent article:<sup>33</sup>

- Early 462-Late 468: Situated along a major North-South trade route, the region surrounding Ajañṭā is an object of contention between the Rṣīka dynasty to its North and the Aśmaka to its South. At this time both Rṣīkan and Aśmakan rulers are feudatories of the Vākāṭaka emperor. Some time before 462 the Rṣīkas defeat the Aśmakas in battle. Almost simultaneously, the Vākāṭaka mantle is thrust upon King Hariṣeṇa by his father. The Ajañṭā ghāṭ being at peace, the Rṣīka king, Hariṣeṇa’s prime-minister, a monk named Buddhahadra with ties to Aśmaka, and other patrons of unknown affiliation serially initiate the excavation of cave monasteries for Buddhist monks.

- Early 469-Late 471: Aśmaka recovers from its prior humiliation and attacks Rṣīka. This conflict stops work on all caves except those sponsored by the Rṣīka king (Caves 17, 18, 19, 20 and 29) and Hariṣeṇa himself (Cave 1). Many workmen go to Bāgh several hundred miles to the north.
- Hiatus: Early 472-Late 474: The Rṣīka/Aśmaka conflict heightens and all work at Ajañṭā stops. It is important to note that during this period programmatic work was completely discontinued on the caves, and no intrusive Buddha images, no “graffiti,” were added either. This highlights an important facet of Ajañṭā’s patronage. Namely, as long as a donor maintained an interest in the cave for which he paid, that excavation was treated as his exclusive property, and was not available for alteration or decoration by anybody else. Thus, a clear demarcation can be made between “programmatic” and “intrusive” periods, iconographies, and inscriptions at Ajañṭā.
- Early 475-Late 477: Aśmaka wins this time around. Work resumes in earnest, this time under Aśmaka supervision. This period ends with Hariṣeṇa’s sudden, unexpected, death.
- Early 478-Late 478: The shock of Hariṣeṇa’s death, the evident weakness of his successor, and the recognized ambitions of Aśmaka to destroy the Vākāṭaka empire impels Ajañṭā’s patrons to hurriedly complete and dedicate their caves.
- Early 479-Late 480: Aśmakan machinations devastate the Vākāṭaka polity; Hariṣeṇa’s empire is rent in ‘civil war’ when his many feudatories turn against their overlord. Original patrons give up their control, enabling the monks and artisans still living at Ajañṭā, as well as others who may have visited at this time, to commission images in the caves already excavated. Eventually, the long and bloody war diverts traffic from the Ajañṭā pass. The monks leave Ajañṭā in search of support, for the original patrons of the caves no longer maintain their establishments and sufficient economic support is not available from other sources. The site is abandoned.

In general one will notice three profound moments in Ajañṭā’s history: that of creation, that of hiatus, and that of devastation. In this study, the latter two are of particular import. Spink’s relative chronology highlights two periods of rushed work followed by major changes in motival usages and patronage patterns, the latter followed by a sudden and absolute cessation of work. The first period of rush is explained through appeal to Aśmakan bellicosity; the second is also blamed upon Aśmaka in part, but here the dominant theme is the dissolution of an empire after a great king’s

death. These two events are the preeminent determining factors in Ajaṅṭā's history, and both are reconstructed based upon evidence gleaned from the incised inscriptions on Caves 16 and 17. More specifically, Spink's narrative of Aśmaka aggression stems from a single verse, number 10, in Cave 17; his characterization of the site's final years, and sudden demise, relies upon parallels between information provided in verse 18 from Cave 16 and the eighth-chapter of Daṇḍin's *Daśakumāracarita*, which he treats as a crypto-historical recounting of the Vākāṭakas' downfall. In short, Spink's absolute chronology has been developed by his reading of a relative chronology into the framework of a history derived from these two verses. Let us see how they have been read, and how they might be read.

#### Cave 17: The Aśmaka Connection

According to Spink's relative chronology, motival developments within Caves 17's, 19's, 20's, and 29's decorative programmes stop mid-way through the overall span of Vākāṭaka patronage, although none of these programmes was completed. To explain the premature termination of these caves, Spink develops a novel history for the site based upon Cave 17's dedicatory inscription. In short, he employs Mirashi's translation of one verse to substantiate a history of animosity between Cave 17's donor and the ruler of Aśmaka. Spink proposes (with supporting, "horizontal" evidence of course) that, as this donor confronted and defeated Aśmaka some time shortly before the site was begun, so the hiatus of 472 occurred when the Aśmakan king took revenge, defeating Cave 17's (19's, 20's, and 29's) donor. However, the only discursive evidence that could tie the Aśmakas to Cave 17's premature termination, the only direct evidence for *any* interaction between Cave 17's donor and Aśmaka, is verse 10, from Cave 17's inscription, which Mirashi has translated as follows:<sup>34</sup>

9. [The donor's father] had two sons resembling Pradyumna and Sāmba. . . . The elder of *them* bore the title of king, while the second bore the appellation Ravisāmba.
10. Having subjugated prosperous countries such as Aśmaka . . . [the two princes] whose prowess had become fruitful, shone like the sun and the moon.

However, when one turns from Mirashi's English translation to the Sanskrit, this relationship certainly becomes less clear. Before I present my

own edition of this crucial verse, I will discuss two prior editions, those of Bühler and Mirashi:

..... samaśmakādi[bhiḥ] {}  
 nu tābhyām abhibhūya bhūyasā rarājatuś candradivākarāv iva ||  
 {10 ||} <sup>35</sup>  
 - - - - - [nīyocchrita]m aśmakādi[kam] {}  
 [kr]tārttḥasatvā[v] abhibhūya bhūyasā rarājatuś candradivākarāv  
 iva || {10 ||} <sup>36</sup>

Here, the crucial verb is *abhibhūya*, meaning "having overcome" or "having conquered." As a gerund, *abhibhūya* is governed by the sentence's main verb: if that main verb is active, *abhibhūya* will take a subject in the nominative; if that verb is passive, the logical subject will be instrumental. Verse 10's two editors have severally presented both options. According to Bühler's transcription, the logical subject of this sentence would be the instrumental pronoun *tābhyām* (= by those two), which might then coordinate with the instrumental *aśmakādi[bhiḥ]* (= by/with the Aśmakas and others). However, this reading cannot be correct. The sentence's main verb is *rarājatuḥ*, which is active, requiring that the gerund *abhibhūya* also be active. The lack of coordination between the gerund and the sentence's main verb implied by Bühler's transcribed *tābhyām* would be a breach of classical Sanskrit grammar unparalleled in these major Ajaṅṭā inscriptions. No doubt recognizing this difficulty, Bühler simply ignores the problematic *tābhyām* in his translation. For Bühler, the two royal brothers, perhaps in league with the Aśmakas and others, defeated a foe whose name has been lost.

Now, the *akṣaras* Bühler reads as the instrumental *tābhyām* and the instrumental ending *°bhiḥ* for *aśmakādi°*, are both less clear on his plate than on that of Mirashi. Mirashi understands both the gerund and the main verb to be active. Reconstructing Bühler's *aśmakādi[bhiḥ]* as *aśmakādi[kam]* (an accusative) and Bühler's *tābhyām* as *[kr]tārttḥasatvā[v]* (= those two whose prowess achieved its aim, nominative), Mirashi surmises that the two royal brothers, their prowess realized, defeated the Aśmakas and others in battle. In short, for Bühler the Aśmakas were possibly Cave 17's donor's allies, for Mirashi, definitely their enemies.

While we might dream of a new and clear estampage of this important inscription, at present Bühler's and Mirashi's plates are our sole means for judging whether the relationship between Cave 17's donor and the Aśmakas

was that of friend or foe.<sup>37</sup> Mirashi's reconstruction can be assessed by almost anybody with vision, simply by comparing the morphologies of "representative" *akṣaras*. Obviously a certain bias is introduced into this study through my choice of *akṣaras*. To this charge, I answer that a wider sample of letter forms would surely allow for a more secure assessment of this verse, but such a project is better suited for a journal of paleography. Here my aim is simply to present clear, "text-book" examples of individual *akṣaras*, a task made difficult by the fact that these rock faces are so damaged, rendering estampages doubly unintelligible. The figures I provide here come from Mirashi's plates, reproduced using a Hewlett-Packard flatbed scanner; their colors are inverted to black-on-white; and they have been cleaned up to enable easier identification by non-specialists. Again, this process adds bias, but the undoctored reproduction is readily accessible in Mirashi's monograph.

First let us look at the *akṣara* acknowledged as unclear by Bühler and Mirashi alike, read by the former as *bhi* and the latter as *ka* (fig. 2). Bühler's plate shows a mere smudge and is useless; Mirashi's has an *akṣara* that is generally unclear, but is almost certainly not *ka*. The latter syllable can be recognized typically by the presence of a distinct vertical bar crossed about 1/3 of the way from the top by a horizontal (fig. 3). Bühler's *bhi* is not at all morphologically viable, and we may assume he only suggested this reconstruction by parallel with his *tābhyām*. On Mirashi's plate, this *akṣara* looks most like a *ṣu* (fig. 4), *lu*, or an initial *ā* (fig. 5).

Failing an easy solution based upon morphology, meter and grammar may provide assistance. As for the first, this verse is in Vamśastha meter, each quarter foot of which properly ends with a light syllable followed by a heavy syllable. Both Bühler's and Mirashi's suggestions fit these requirements. Except for *ā*, the reconstructions suggested above break meter. However, at Ajaṇṭā, Vamśastha verses are often imperfect. That is to say, although the final *akṣara* should be heavy, one often finds that it is light. In fact, this exception obtains for the final syllable of verse 10, *va*. In view of this metrical anomaly, there are numerous reasonable possibilities for this *akṣara*,<sup>38</sup> among which Mirashi's estampage comes closest morphologically to *ṣu*. Turning now to grammar, we are fortunate that *ṣu* can allow for a contextually meaningful reconstruction. It is also important to note that the first word of the next half-verse (and perhaps the word preceding *āsmakādi* as well<sup>39</sup>) is clearly in the accusative case. Thus, although the verb *abhi|bhū* can take an object in the locative case, the locative plural *āsmakādi|ṣu* should most likely be read in its locative sense: "in Aśmaka and other [realms]."

We know that Cave 17's royal donor had an enemy or enemies whom he defeated, and we may even know *where* the fight happened, but from this inscription alone we cannot determine *who* that enemy was. The second crucial *akṣara* for our discussion is that read by Bühler as the final *m* of *tābhyām*, and by Mirashi as the bracketed *v* of *\*satvā|v* (fig. 6). Here both estampages almost definitely show *m* (fig. 7). Bluntly put, Mirashi's *\*tvā|v* is a misrepresentation of the estampage: this bracketed *\*v* is very clearly *not* *\*v* (fig. 8). Bühler's *\*m* is certainly a viable alternative. However, on Mirashi's estampage beneath the more visible letter is the hint of a loop that could be a sub-joined *\*v* (as Bühler's plate was filled in for greater clarity, one cannot use it to judge the inscription on this matter). If this sub-joined *\*v* is accepted, then one could read this likely *\*m* as a *\*ṣ* (fig. 9 is an example of an *\*ṣ* isomorphic with *\*m*), and the complete *akṣara* as *\*ṣva*. Thus we would have a locative plural (*|kr|tārthasatvāṣv a*) to coordinate with *āsmakādi|ṣu* from the previous line. However, the *akṣara* preceding this hypothetical *\*ṣva* very clearly has the mark of a medial *ā*, and the retroflex *\*ṣ* will not occur after the vowel *ā*. Since the preceding *akṣara* is definitely conjunct, and the *\*m* under consideration is a case marker, it seems almost certain that this word is a singular accusative from an *ā*-stem.<sup>40</sup> A revised reading of Mirashi's *|kr|tārthā* (which makes little sense here as an accusative) as *|kr|tārddha* is viable since *\*rthā* and *\*rddha* can be regarded as isomorphic on a estampage as uncertain as this. Let me note, if this compound is a feminine accusative singular and the object of the verb *abhibhūya*, then Mirashi's hypothetical *āsmakādi|kam* could not also be that verb's object, and must in any event be a wrong reading.<sup>41</sup> My reconstruction of this verse thus reads:

o - o - - o - o - o - o - o - o - - o - o m āsmakādi|ṣu {||}  
 |kr|tārddhasatvām abhibhūya bhūyasā rarājatuś candradivākarāv  
 iva || {10 ||}

10. . . . after those two [princes] vanquished [an enemy] . . . whose life was cut short . . . [in] Aśmaka and [elsewhere], they shone (or governed) brilliantly like the moon and the sun.

A foe of this cave's donor may have been defeated on several fronts, in Aśmaka and elsewhere; that foe's identity, name, and title remain unknown. However, because the compound noun *|kr|tārddhasatvām* is of the *bahuvrīhi* type — i.e., it coordinates syntactically with the object to which it refers — we can ascertain that the defeated foe would have been known by a name or epithet ending in the feminine singular. It is unlikely the adversary was a

woman; it is possible that that antagonist's name ended in the feminine *śrī*. Nevertheless, I would speculate that this opponent was identified by the name of the country he governed. Aśmaka is only attested as masculine in gender. Possible names of potential enemies in the area included: Avanti, Kuntala, Kosalā, Mekhalā, Mālava, R̥ṣika, Trikūṭa, Lāṭa, Andhra, Mūlaka, Anūpa, Vidarbha, Vanavāsī, Nāsikya, Murala, Koṅkaṇa. Of these, only four (to my knowledge) are attested as feminine: Kosalā and Mekhalā (in the Māṇḍhal plates of Vākāṭaka Pṛthiviṣeṇa II<sup>42</sup>), Vanavāsī, and less likely Vidarbhā.<sup>43</sup> It may be the case, therefore, that the lord of Kosalā, Mekhalā, Vanavāsī, or maybe Vidarbhā, met his demise in Aśmakan territory. However, there is also a distinct possibility that these two refulgent rulers were themselves the sovereigns of domains located "in Aśmaka and elsewhere." Surely this would be the proper interpretation if *aśmakādi[ṣu]* was in the second half-verse. This latter possibility is attractive in that, from our vantage point, the locale under this cave's donor's sway is more important information than where he defeated some now ancient enemies. Nevertheless, because so much is lost of the first half-verse, we can never really be sure exactly why this inscription's author included *aśmakādi[ṣu]*,<sup>44</sup> if indeed this is the proper reconstruction.

#### *Cave 20: A Case of Mistaken Identity*

This revision of a single verse from the Cave 17 dedicatory inscription has greatly problematized Spink's telling of Ajaṅṭā's history. Without Mirashi's reading of this verse, no direct, documentary evidence exists for conflict between Cave 17's donor and Aśmaka. Again at this point I wish to emphasize that insofar as the analytic distinction between relative and absolute chronologies has been suitably maintained in their elaboration, this new reconstruction of Cave 17's verse 10 has no necessary impact upon the length or elaboration of Spink's short chronology as a relative construct.

There is a second problem that might be brought to attention in regard to Cave 17's patronage. This point is ancillary to my paper's broader agenda, but is still worthy of notice. Spink and other scholars have proposed that Caves 19 and 20 are the donations of the same person as that responsible for Cave 17. The Cave 19 attribution is fairly certain, based upon Cave 17's verse 27, wherein Cave 17's donor claims to have "commissioned an extensive *gandhakuṭī* in another section of this [monastic complex], to the West."<sup>45</sup> The *gandhakuṭī*, literally perfumed hall, was originally the name of the Buddha's personal residence at Jetavana, Anāthapiṇḍada's monastery in Śrāvastī, but came to be identified with the central shrine of

any monastery.<sup>46</sup> As such, the Buddha shrine at the rear of this vihāra might well be called its *gandhakuṭī*. This placement for the *gandhakuṭī* would be further supported by the vinaya of the Mūlasarvāstivāda sect of Buddhism,<sup>47</sup> which describes the following basic floor-plan for monasteries of that sect:

The Blessed One said, 'If you have three cells made, a *gandhakuṭī* should be placed in the middle and two cells on the two sides. Thus in a structure with three walls, there are nine cells. In a four walled structure, the *gandhakuṭī* [should] face a door-way in the center [of the wall opposite it]; and two cells [are to be placed] on the sides of the door-way.'<sup>48</sup>

That Cave 17's Buddha shrine is not the *gandhakuṭī* in question, however, is suggested by this verse's stipulation that the *gandhakuṭī* is in another section to the West, as well as by the fact that early in this record, verse 24 mentions a separate *caitya* for the King of Sages excavated in the cave's recesses.<sup>49</sup> A similar disjunctive pairing of *gandhakuṭī* and *tathāgatacaitya* is found in the *Mūlasarvāstivāda vinaya* on several occasions. In one instance, the *gandhakuṭī* and Buddha's hair-and-nail stūpas are named as separate edifices that monks should repair when damaged;<sup>50</sup> in another, the text prescribes that if no other source of revenue is available with which to care for a sick monk, his nurse can sell a parasol, a pennant, flags, an ornament, or scented waters otherwise reserved for the saṅgha to cast upon the *tathāgatacaitya* or the *gandhakuṭī*.<sup>51</sup> This latter passage suggests that although these two structures were physically distinct, they shared similar rituals. Cave 17's inscription also suggests that a *gandhakuṭī* and *muni-rājacaitya* were two distinct elements in its donor's plan. Yet, taking these to be the Cave 19 stūpa and the Cave 17 Buddha shrine respectively, we find that a certain ritual symmetry obtained between the two, for the same two images flank the entrance way to both excavations: on one side of the door is the bodhisattva Sumati<sup>52</sup> paying homage to Buddha Dīpaṅkara, on the other side, Rāhula, the Buddha's son, receiving his 'inheritance.'<sup>53</sup>

That Cave 20 was also the production of Cave 17's donor is less secure. The argument most frequently articulated to support this position is based upon Bühler's reading of Cave 20's programmatic dedicatory inscription, wherein he suggests that "Upendra or Upendra[*gupta*]" was this cave's donor, and that his "father's name may have been Kṛi[*shṇa*] or Kṛi[*shṇadāsa*]."<sup>54</sup> The possible significance of these names vis-à-vis a relationship between this cave and Cave 17 is presented by Chandrashekhar Gupta as follows:

The fragmentary [*sic*] inscription in Cave 20 is also written in box-headed characters identical to those of Caves 16 and 17. It mentions donation of this cave (*mandapa*) by one Upendra(*gupta*) whose father and grandfather were named but got destroyed [*sic*]. An important clue is obtained by the presence of a letter *Kri* in 1.2 succeeding the word

*pautrasya*. It makes clear that here the name of the father of Upendra was mentioned which is rightly reconstructed as Krishna or Krishnadasa by Bühler. Now we know one Krishnadasa from inscription [sic] in Cave 17 whose son got caused number [sic] of *vihāras* and *chaityas*. So there remains no doubt that the name of the feudatory chief who got excavated [sic] Caves 17 to 20 was Upendragupta as there is sufficient space after his name in 1.3 of the second inscription and one Upendragupta is also present in the genealogy [sic] of the first inscription.<sup>55</sup>

There are quite a number of places in Gupta's argument at which the absolute certainty of his claim that "Upendragupta" patronized Caves 17–20 can be rendered not so very absolute. My reconstruction of Cave 20's programmatic donative inscription, based upon all published facsimiles and *in situ* investigation reads:

## TEXT:

- L1: ... yaṃ maṇḍapa ...  
 L2: ... pautrasva<sup>1</sup> kṣ[a]<sup>2</sup> ...  
 L3: ... putrasya upendra ...  
 L4: ... sya dharmma[haga]<sup>3</sup> ...  
 L5: ... trasya jayata ...  
 L6: ... [sya] kulapri ...  
 L7: ... mocāsaka[sya] ...  
 L8: ... tvāmika[syau]<sup>4</sup> ...  
 L9: ... puṇyan tad ...  
 L10: ... gamānā[m] sa ...  
 L11: ... nāvāp ...

## TEXT NOTES:

- <sup>1</sup>Read \*trasya <sup>2</sup>This is the *akṣara* identified by Gupta as *kr* <sup>3</sup>The reading "haga" is dubious  
<sup>4</sup>This might also read \*nvāmika\*

In light of Gupta's analysis, one wants to know what to do with the °*trasya* in line 5, which could be the remnant of *putrasya*, and the genitive singular °*sya* seemingly found in lines 6, 7, and 8, all of which must be shown to be epithets of the so-named Upendragupta of line 3, for otherwise they could quite possibly be the names of other members of the genealogy. More troublesome for Gupta's interpretation of the inscription, however, is that he begins his analysis by appeal to paleographic similarities between the Cave 17 and Cave 20 inscriptions. However, the *kr* of Cave 17's Kṛṣṇadāsa (fig. 11) looks nothing like the so-called *kr* of the Cave 20 record (fig. 10). Instead, the medial vowel read by Gupta as °*r*° at Cave 20, looks either like a sub-joined °*y*° (fig. 12) or a sub-joined °*ṣ*° (fig. 13).<sup>56</sup> Indeed, this Cave 20 *akṣara*, if read *kr*, would be morphologically unique at Ajaṇṭā.<sup>57</sup> Thus, treating Cave 20's inscription in isolation as an integral donative record, one cannot be certain that Upendra, let alone Upendra-

gupta, was the name of the *maṇḍapa*'s donor; nor does it seem possible that someone whose name begins *kr*° was the immediate ancestor of the donor. As these two points are ambiguous in the Cave 20 inscription, it is unsound to map the chronology and patronage of Cave 17 onto Cave 20 based upon epigraphic evidence.

*Cave 16: The King's Many Conquests? The King's Many Wives?*

Above, I introduced Spink's reconstruction of Ajaṇṭā's history as having two linchpins. We have already seen the problems surrounding the first of these, the Aśmaka dynasty's role as the nemesis of Cave 17's donor. The second piece of data — used to explain the site's demise — focuses upon the Vākāṭaka king Hariṣeṇa. Direct information about this Vākāṭaka monarch comes from two sources: the paean to him and his forbears that comprises the first half of Varāhadeva's Cave 16 inscription, and the Thālner grant dated to year 3 of Hariṣeṇa's reign.<sup>58</sup> A third document apposite to Hariṣeṇa is the eighth chapter of Daṇḍin's early seventh-century classic, the *Daśakumāracarita*. This text has come to be used as a source for recovering Hariṣeṇa's legacy and Ajaṇṭā's history based upon V. V. Mirashi's 1945 article, in which he set out to prove that this tale of a King-of-kings destroyed when his many feudatories turn on him<sup>59</sup> retells the events surrounding the Vākāṭaka empire's fall. Although one cannot but be suspicious about treating an ostensibly literary work as crypto-historical, Mirashi made a sufficiently convincing case that his thesis became an *idée reçue* among Ajaṇṭā's chronologers. Mirashi's argument was based upon his observation that the list of treacherous feudatories enumerated in the *Daśakumāracarita* was virtually identical to a list of territories found in verse 18 of the Cave 16 inscription. According to the dominant interpretation of this verse, these lands were conquered by Hariṣeṇa early in his reign, and would have had a natural impulse to regain their individual sovereignty when the opportunity arose. Thus, the *Daśakumāracarita*'s eighth chapter is likened to a docu-drama telling the history of the Vākāṭaka empire after Hariṣeṇa's death. As Spink tells it, "Daṇḍin has changed the names of the players, but the 'playing-field' is the same, for nearly every territory mentioned in the Cave 16 inscription figures in Daṇḍin's account too, as do the domains that had been in Hariṣeṇa's empire from the time of his succession."<sup>60</sup>

There are two principal issues that may be raised in relation to this text. The first, which I will *not* explore, is whether the *Daśakumāracarita*'s eighth chapter *should* be treated as a historical reflex of the events following Hariṣeṇa's death. The viability of the *Daśakumāracarita* as a crypto-

historical work has been contested all around, from rejection,<sup>61</sup> to tempered acceptance,<sup>62</sup> to the full acceptance characteristic of Mirashi and Spink. Nevertheless, although scholars have variously accepted and rejected the *Daśakumāracarita*'s historicity, none has problematized the very presupposition from which that proposition springs: that the Cave 16 inscription's verse 18 enumerates lands conquered or held by Hariṣeṇa. Examining this verse, one discovers that the many lands mentioned therein could have been Vākāṭaka feudatories. However, one also discovers that this standard interpretation is by no means certain. My interest in this verse is not with its reconstruction, but with the use of its translation. Accordingly, I will present Mirashi's transcription, and my own translation:

sa kuntalāvantikaliṅgakosalatrikūṭalāṭāndhra ˘ - ˘ ˘ jān imān {}  
 ˘ - ˘ - - ˘ ˘ [śaurrya]viśrutān api svanirddeśa(guṇāti) - ˘ -  
 {|| 18 ||}<sup>63</sup>

18. [Hariṣeṇa] . . . those . . . of Kuntala, Avanti, Kaliṅga, Kosala, Trikūṭa, Lāṭa, Andhra, [who] celebrated for [heroism] as well as . . . [extremely . . . to the qualities] of self-government.

One will immediately notice from my translation of Mirashi's Sanskrit transcription that, as it now exists, this verse lacks a verb. This problem is akin to that explored in regard to the relationship between Cave 17's donor and the Aśmakas: we know that Hariṣeṇa bore some relation to the countries named Kuntala, Avanti, Kaliṅga, Kosala, Trikūṭa, Lāṭa, and Andhra, or their kings, or their populace, but that relation's content is by no means certain. Bühler and Mirashi alike read this verse as claiming that Hariṣeṇa "[conquered]" these various lands.<sup>64</sup> This is one likely interpretation, perhaps the most likely. Nevertheless, it is a reconstruction without any direct confirmation in the available evidence. This is why I have left the verb out of my translation: however reasonable this interpretation is, the possibility remains that this verse simply delineates the range of territories Hariṣeṇa inherited from his father, or maybe even other countries with which he formed an alliance for some unknown purpose.<sup>65</sup> This is the point of tension that must be recognized, if not resolved. For although Bühler and Mirashi acknowledge the tentativeness of their translations, Mirashi, and following him Spink, have taken the information contained in this verse as a firm basis for reconstructing the events surrounding Ajaṅṭā's last days.

For an example of the dangers involved in a too-uncritical acceptance of this verse as falling into the genre of *digvijaya praśasti* (celebration of

territorial conquests), we can look to a recent article in which Shobana Gokhale fills in the lacuna found in this verse's first line between *˘āndhra* and *˘jān*. Apparently viewing the martial nature of this verse as given, Gokhale boldly states that Hariṣeṇa "was the paramount ruler of the region extending from Malwa in the north to Kuntala in the south;"<sup>66</sup> and she wonders why, as he was such a great conqueror, India's west coast is missing from his *digvijaya praśasti*. Accordingly, she proposes reconstructing the letters "*parānta*" into verse 18's lacuna. An ancient name for the coastal area south of the Tapti River, Aparānta (shortened to *parānta metri causa*) fits into the *Vaṃśastha* meter for the reconstructed reading: *sa kuntalāvanti-kaliṅgakosalatrikūṭalāṭāndhraparāntajānimān*. Gokhale claims that "according to the essentials of the *Vaṃśastha* metre no other name of a country suits between Andhra and Jānimān."<sup>67</sup>

Gokhale's final assertion is incorrect, for *Vidarbha* scans the same metrically. Be that as it may, however, Gokhale has so completely accepted this verse as a *digvijaya praśasti* that she fails to explain what the significance of this compound's final element would be given her suggested emendation. If one accepts Gokhale's *parānta*, there are two possible readings for the line's ending *˘jānimān*. First this can be analyzed into *˘jān* (an accusative plural, meaning the inhabitants or people of) and *imān* (an accusative plural pronoun, "these"). In this reading, Hariṣeṇa has an unknown relationship to "these, the inhabitants of Kuntala, Avanti, Kaliṅga Kosala, Trikūṭa, Lāṭa, Andhra, and Aparānta." And though it would make grammatical sense to read this relationship as one between conqueror and conquered populace, culturally this interpretation seems unlikely. Within the Cave 16 inscription, Hariṣeṇa's ancestors are celebrated for vanquishing their enemies, for having their lotus-like feet kissed by rays from the jewels set in the crowns of other kings, for conquering all armies, and for humbling the lord of Kuntala, among other accomplishments; these other Vākāṭaka kings' glory little derived from their ability to subdue farmers and merchants. I would be surprised if Cave 16's poet celebrated Hariṣeṇa with lesser praise than his forefathers, or described the mere populace of the many realms mentioned as "celebrated for [heroism]" and interested in "self-government."

By reading *˘jānimān* as this longer compound's final element (as Gokhale seems to do), one complicates matters still further: *˘jānimān* is a nominative singular — apposite to the subject of the sentence, presumably Hariṣeṇa — meaning possessed of (*mān*) wives (*jāni*) from these various lands. This latter reading leaves no scope for a claim, based upon this verse, that Hariṣeṇa conquered these lands. To have possessed such a glorious range of



matrimonial alliances may well merit eternal celebration; however, it all but invalidates the possibility of reading this verse as possessing historical information parallel to that in the *Daśakumāracarita*.

If one does not reconstruct this lacuna with the name of yet another country, but instead a word meaning king that ends in \*ja (not *rāja*, as this violates the meter), one retains the possibility of reading this verse within the *digvijaya praśasti* genre. This seems to me a reasonable tactic, albeit a reconstruction of this sort stands outside the scope of my study. With this inscription, as with the previous two, I am not claiming that reconstructions of Ajaṅṭā's history, or that of the Vākātakas, have relied upon necessarily incorrect epigraphic data. Rather, my intention has been to clarify the exceptionally tentative nature of the material from which these histories' "facts" derive — an uncertainty that is not often reflected in the historians' prose. Transcriptions and translations based upon such incomplete and often illegible estampages simply cannot alone bear the interpretive burden required for the declarative certainties found in writings on Ajaṅṭā's history. The "known historical events"<sup>68</sup> Spink invokes to reconstruct an absolute chronology for Ajaṅṭā — an enmity between Cave 17's donor and the ruler of Aśmaka; Hariṣeṇa's martial (not marital) conquests; the dissolution of the Vākāṭaka empire due to Aśmakan perfidy — are not quite so well known.

Rhodes College  
Memphis

#### NOTES

<sup>1</sup> Ajaṅṭā's excavation was placed "between the years 500 and 650, with a very little margin either way before or after these dates" in James Fergusson and James Burgess. *The Cave Temples of India* (Delhi: Oriental Books Reprint, 1969): 299.

<sup>2</sup> Walter M. Spink. "The Archaeology of Ajaṅṭā," *Ars Orientalis*. 21 (1991): 70.

<sup>3</sup> Fergusson and Burgess, Spink and myself are all referring here to the second of two general periods in Ajaṅṭā's history of excavation and decoration. The first period, commonly termed the "Hīnayāna," ranged somewhere between the first century B.C.E. and first century C.E. This second phase of work has been variously identified, but following Spink I will call it the "Vākāṭaka period," named after the dynasty reigning at the time this patronage occurred.

<sup>4</sup> Whereas the most recent tier of scholarship on Ajaṅṭā has been dominated by Spink's meticulous examinations of the site's chronology, post-Burgess chronologers have included, among others, Philippe Stern (*Les Colonnes indiennes d'Ajanta et d'Ellora: évolution et répercussions [styles gupta et post-Gupta]*, [Paris: Presses universitaires de France, 1972]); Wayne Edison Begley (*The Chronology of Mahāyāna Buddhist Architecture and Painting at Ajaṅṭā*, [Ann Arbor, Mich.: University Microfilms, 1966]); Vasudev Vishnu Mirashi (*The Inscriptions of the Vākātakas*, Corpus Inscriptionum Indicarum Vol. V. [Ootcamund: Government Epigraphist for India, 1963]); Sheila Weiner (*Ajaṅṭā: Its Place in Buddhist Art*, [Berke-

ley: University of California Press, 1977]); Karl Khandalawala ("The History and Dating of the Mahayana Caves of Ajanta," *Pathik*. 2.1 [1990]: 18–21). Joanna Williams' excellent summary of several of these scholars' chronologies should be consulted by anyone interested in pursuing the topic ("Ajanta and the Art of the Vakatakas," appendix to J. Williams' *The Art of Gupta India: Empire and Province*, [Princeton: Princeton University Press, 1982]).

Of these various chronologers, Begley relies the most upon epigraphic studies. As Begley explained it, his project was "to evaluate all the pertinent evidence for establishing on a firm basis the chronology of the Mahāyāna phase of artistic activity at Ajaṅṭā" (*Chronology*, 3); this "firm basis" was itself to be supported by "the framework suggested by inscriptional evidence" (*Chronology*, 4), for Begley conceived of epigraphical paleography as a "reliable and scientific tool for dating" (*Chronology*, 30). However, Begley's use of Ajaṅṭā's many inscriptions as chronological anchors compromises his conclusions: paleography is hardly reliable and hardly scientific. K. V. Ramesh, former head of the Archaeological Survey of India's epigraphy branch, warns against paleographic dating ("The [F\*]utility and (F)utility of Paleography in Dating Undated Inscriptions," *Studies in Indian Epigraphy*. 3 [1977]: 156–162); and Spink presents cogent arguments against dating through paleography at Ajaṅṭā in particular ("Ajaṅṭā's Chronology: The Problem of Cave Eleven," *Ars Orientalis*. 7 [1968]: 155–168, esp. 156–7). But let us look at one paleographer's, Shobana Gokhale's, understanding of this domain of inquiry: "the paleographical chronometer may be successfully employed for the purpose of chronology but then the results from such indications should be considered as approximate not precise" ("Epigraphical Evidence for the Chronology of Ajanta," *Journal of Indian History*. 51 [1973]: 482). How approximate, we ask? Gokhale would date the Ajaṅṭā Cave 20 inscription, for example, to somewhere within "a period ranging from A.D. 450 to A.D. 525" ("Epigraphical Evidence," 482); Begley, following Indrajī, Bühler, and Chhabra would place this record in the mid sixth-century (*Chronology*, 51). Combining the two, Gokhale for the near end, Begley for the far, they establish that paleographical dating is reliable in terms of centuries, which is to say hardly reliable at all for setting either a relative or absolute chronology at Ajaṅṭā.

<sup>5</sup> This is not to say that Hariṣeṇa's dates are themselves set with any certainty. Mirashi places Hariṣeṇa in the last quarter of the fifth century, whereas Spink places him in the third quarter. See Spink "Reply to K. Khandalawala, A. Jamkhedkar, B. Deshpande," *Pathik*. 3/4 (1992) for his most recent statement on the Vākāṭaka genealogy. See, additionally, Mirashi. *Inscriptions*, v–vi & xxix–xxxiii, as well as Nisar Ahmed. "A Re-examination of the Genealogy and Chronology of the Vākātakas," *Indian Antiquary*. ser. 3. 4 (1970): 149–164.

<sup>6</sup> Bhau Daji. "Ajanta Inscriptions," *Journal of the Bombay Branch of the Royal Asiatic Society*. 7 (1863): 54.

<sup>7</sup> James Burgess and Pandit Bhagwanlal Indrajī. *Inscriptions from the Cave Temples of Western India with Descriptive Notes, &c.* (Delhi: Indian India, 1976): 67.

<sup>8</sup> Mirashi. *Inscriptions*, 121.

<sup>9</sup> James Prinsep. "Facsimiles of Various Ancient Inscriptions," *Journal of the Asiatic Society of Bengal*. 5 (1836): 348–9, 556–561, plate ix, # 4, plate xxviii, # 9, # 10, # 11.

<sup>10</sup> Daji. "Ajanta Inscriptions," 53.

<sup>11</sup> Burgess and Indrajī. *Inscriptions from the Cave Temples of Western India*, 69–79.

<sup>12</sup> In James Burgess. *Report on the Buddhist Cave Temples and their Inscriptions*. Archaeological Survey of Western India Vol. IV (Varanasi: Bharatiya Publishing House, 1975): 124–136.

<sup>13</sup> In N. P. Chakravarti, and B. Ch. Chhabra. "Notes on the Painted and Incised Inscriptions of Caves XX–XXVI," appendix to Ghulām Yazdani. *Ajanta: The Colour and Monochrome Reproductions of the Ajanta Frescoes Based on Photography*. Volume 4 (London: Oxford University Press, 1955): 114–118.

<sup>14</sup> The number 97 for Ajaṅṭā's epigraphs is derived from my collection of materials based upon published sources and *in situ* investigations; these 97 include both readable and

unreadable records, as well as inscriptions noticed by prior scholars that no longer exist due to time and wear. Along with Ghatotakaca's two epigraphic records, I have re-edited and retranslated all of the legible materials as an appendix to my dissertation written at the University of Michigan, *Setting the Three Jewels: The Complex Culture of Buddhism at the Ajañṭā Caves*. When time permits, I intend to publish this collection separately, with interpretive and historical notes.

<sup>15</sup> A group of six epigraphic records may be dated to the late seventh to early eighth centuries (approximately), when, according to G. H. Malandra, the Rāstrakūṭa dynasty governed the territory surrounding Ajañṭā ("The Date of the Ajañṭā Cave 27 Inscription," *Wiener Zeitschrift für die Kunde Südasiens*. 26 [1982]: 37–46). These post-Vākātaka inscriptions have no specifically Buddhist content; nor do they signal new dedications or donations. Rather, these records seem mostly to be simple graffiti, recording the names and titles of latter-day visitors to the caves. A single exception is a notoriously damaged paean to the Rāstrakūṭa family, located on the wall connecting Cave 26 and Cave 26 Lower Left. None of these later epigraphic records, however, give grounds for inferring that the caves were still functioning as Buddhist vihāras at the time of their writing.

<sup>16</sup> N. P. Chakravarti. "A Note on the Painted Inscriptions in Caves VI–XVII," appendix to Yazdani. *Ajanta*. Volume 3 (London: Oxford University Press, 1946): 95.

<sup>17</sup> The first scholar to make this identification was Heinrich Lüders. "Ārya Sūra's *Jāta-kamālā* and the Frescoes of Ajañṭā," *Indian Antiquary*. Translated by J. Burgess. 32 (1903): 326–28. For transcriptions and translations of these verses see John Allen. "A Note on the Inscriptions of Cave II," appendix to Yazdani. *Ajanta*. Volume 2 (London: Oxford University Press, 1933): 57–63.

<sup>18</sup> This was edited in Daji. "Ajunta Inscriptions," 56–58; Burgess and Indrajī. *Inscriptions*, 69–73; Burgess. *Report*, 124–127; and Mirashi. *Inscriptions*, 103–111.

<sup>19</sup> This was edited in Daji. "Ajunta Inscriptions," 59–61; Burgess and Indrajī. *Inscriptions*, 73–76; Burgess. *Report*, 128–132; and Mirashi. *Inscriptions*, 120–129.

<sup>20</sup> This was edited in Daji. "Ajunta Inscriptions," 61–63; Burgess and Indrajī. *Inscriptions*, 77–79; Burgess. *Report*, 132–136; and Chakravarti and Chhabra. "Notes on the Painted and Incised Inscriptions of Caves XX–XXVI," 111–118.

<sup>21</sup> Dinesh Chandra Sircar. "Inscription in Cave IV at Ajañṭā," *Epigraphia Indica*. 33 (1959–60): 259–262.

<sup>22</sup> This was edited in Daji. "Ajunta Inscriptions;" Burgess and Indrajī. *Inscriptions*, 76–77; Burgess. *Report*, 132; and Chakravarti and Chhabra. "Notes on the Painted and Incised Inscriptions of Caves XX–XXVI," 113–114.

<sup>23</sup> On this point see, for example, Spink. "Archaeology of Ajañṭā," 76; Khandalawala "History and Dating," 21; Chandrashekhar Gupta. "Authorship of Ajanta Caves 17 to 20." in *The Art of Ajanta*. Ed. by Ratan Parimoo, et al. (New Delhi: Books and Books, 1991): 100–104. Spink alone adds Cave 29 to this list, and he has only done so orally.

<sup>24</sup> Williams. *Art of Gupta India*, 6.

<sup>25</sup> Spink. "Archaeology of Ajañṭā," 70.

<sup>26</sup> Spink. "Archaeology of Ajañṭā," 70.

<sup>27</sup> Spink. "Archaeology of Ajañṭā," 70.

<sup>28</sup> Spink. "Archaeology of Ajañṭā," 70.

<sup>29</sup> Spink. "Archaeology of Ajañṭā," 70.

<sup>30</sup> Spink. "Archaeology of Ajañṭā," 70.

<sup>31</sup> Spink. "Ajañṭā's Chronology: The Crucial Cave," *Ars Orientalis*. 10 (1975): 143–169, Plates 1–16.

<sup>32</sup> Spink. "Archaeology of Ajañṭā," 70.

<sup>33</sup> Spink. "Archaeology of Ajañṭā." For a more complete summary of Spink's reconstructed history see appendix D to my dissertation, *Setting the Three Jewels*; for a more complete critique of that reconstructed history, see the dissertation's first chapter.

<sup>34</sup> Mirashi. *Inscriptions*, 128.

<sup>35</sup> Burgess. *Report*, 129.

<sup>36</sup> Mirashi. *Inscriptions*, 125.

<sup>37</sup> I wish to reiterate that this study is based upon readings of the published facsimiles, work I undertook *in situ* (during which I spent several several days standing upon a ladder in front of each inscription, trying to make sense of the individual obscure *aḥṣaras*), and at the Archaeological Survey of India's epigraphical archives in Mysore, where I examined the actual estampages that were reproduced in Mirashi's *Inscriptions of the Vākātakas*. During this latter period, I had the good fortune to receive the assistance of Dr. K. V. Ramesh, probably India's premier living epigraphist.

<sup>38</sup> This range of possibilities includes: \**nā* (instrumental singular), \**bhiḥ* (instrumental plural), \**su* (locative plural), and \**kaḥ* (nominative singular), \**kam* (accusative singular), \**ke* (locative singular), \**kau* (nominative/accusative dual), \**kān* (accusative plural). Note that none of these suggestions include a conjunct constant. This is because, at Ajañṭā, one finds metrical play *only* in the final measure of a *pāda*, and Vamśastha meter requires that the penultimate *aḥṣara* be light.

<sup>39</sup> Here I am referring to the *aḥṣaras* Mirashi reconstructed as "[*nīyocchrita*]*m*," which he translates as "prosperous." Mirashi's square brackets indicate that he himself was not too comfortable with this reading. I simply do not see this word, and have left it out of my transcription.

<sup>40</sup> Possibilities that I have rejected include a dual instrumental from an *a*-stem or *ā*-stem, a dative or ablative from an *ā*-stem, the locative singular form of an *i* or *ū* stem, or finally a feminine *i* or *u* noun. Still another possibility, suggested to me by Gregory Schopen, is that \**satvām* could be a masculine plural accusative, with *m* substituted for the intervocalic nasal *n*. Indeed, this reading would coordinate well with the locative plural *āsmakādiṣu*, allowing several foes to be defeated in several locales, a not unreasonable scenario. Unfortunately, such a substitution of *m* for *n* has no parallel in Ajañṭā's major incised inscriptions. Everywhere in the Cave 16, 17, 26 and Ghatotkaca inscriptions, the masculine plural accusative ends in \**ān*, even when that nasal is intervocalic. While such a substitution of *m* for *n* is not unknown to the Sanskrit literary universe, given the lack of parallel examples from Ajañṭā, and the uncertain context for *āsmakādiṣu*, I think it best to read this as a feminine singular accusative.

<sup>41</sup> It is not clear to me why Mirashi made such a blatant mistake in his edition of this verse. Were he simply misreading the *aḥṣara*, Mirashi would not have bracketed the *v*, for however it is read, this *aḥṣara* is very clear on his estampage; if Mirashi thought there was a scribal error here, he would have placed what he considered the proper reading after it, as he does earlier in the same line, where he suggests reading a scribal \**tv*\* as \**tvā*\*: [*krjārttha-satvā(itvā)*] *v* *ajbhibhūya* (Mirashi. *Inscriptions*, 125). It seems, in this instance, that Mirashi was second guessing the scribe. Perhaps he ignored the clear and evident reading because it did not tally with the history he expected (or desired) to find in this verse.

<sup>42</sup> Ajay Mitra Shastri. "Māñdhāl Plates of Prthivshena II, Years 2 and 10," *Epigraphia Indica* 1975–76. 41 (1989): 177, Line 17.

<sup>43</sup> *Vidarbha* is almost invariably found in the masculine, but Sircar records at least one instance, from the *Skandha-purāna*, in which this territory's name is recorded as the feminine *Vidarbhā*. (c.f. D. C. Sircar. *Studies in the Geography of Ancient and Medieval India*. [Delhi: Motilal Banarsidass, 1971]: 262).

<sup>44</sup> For instance, this could have been one element in a locative absolute clause or the locative could have been used the sense of "in regard to . . ." Both of these interpretations would require additional material for support.

<sup>45</sup> anyāṅgadeśe 'sya diśi prāṭicyām acikarad gandhakuṭim udārām // (Mirashi. *Inscriptions*, 127).

<sup>46</sup> For studies of the Buddha's chamber see, John Strong. "Gandhakuṭi: The Perfumed

Chamber of the Buddha," *History of Religions*. 16 (1977): 390–406, and Gregory Schopen. "The Buddha as an Owner of Property and Permanent Resident in Medieval Indian Monasteries," *Journal of Indian Philosophy*. 18 (1990): 181–217, esp. 193ff.

<sup>47</sup> I utilize this text in particular based upon Dieter Schlingloff's research into textual precedents for Ajaṅṭā's narrative paintings (See *Studies in the Ajanta Paintings* [Delhi: Ajanta Publications, 1988]: passim). In short, Schlingloff has shown that the major painted caves — 1, 2, 16, and 17 — all relied upon the *Mūlasarvāstivāda vinaya* for inspiration. Let me emphasize, I am aware that oral, rather than written, redactions may have served as the basis for Ajaṅṭā's narratives. Nevertheless, Schlingloff's scholarship is sufficient basis upon which to invoke the *Mūlasarvāstivāda vinaya* for ideologies of the *gandhakuṭī* and *tathāgatacāyā* as they may have been current at Ajaṅṭā.

<sup>48</sup> bhagavān āha: yadi trilayanam kārayasi madhye gandhakuṭīḥ kārayitavyā dvayoh pārśvayoh dve layane; evaṃ trisāle nava layanāni; catuḥsāle madhye dvārakoṣṭhakābhimukhaṃ gandhakuṭīḥ; dvārakoṣṭhakapārśvayor dve layane. Raniero Gnoli (ed.). *The Gilgit Manuscript of the Sayanāsānavastu and the Adhikaraṇavastu, Being the 15th and 16th Sections of the Vinaya of the Mūlasarvāstivādin* (Rome: Istituto Italiano per il Medio ed Estremo Oriente, 1978): 10–11.

<sup>49</sup> niveṣītāntarmunirājacāyām ekāsmakam maṇḍaparatanam etat // (Mirashi. *Inscriptions*, 127).

<sup>50</sup> Nalinaksha Dutt (ed.) *Gilgit Manuscripts*. Volume 3, part 2 (Delhi: Sri Satguru, 1984): 143.

<sup>51</sup> yat tathāgatacāyā vā gandhakuṭyām vā chattraṃ vā dhvajam vā patākā vā ābharānakam va samghena pāniyam pātavyam iti / upasthāyikena vikriya. Dutt. *Gilgit Manuscripts*, vol. 3, part 2, 125.

<sup>52</sup> This is the name given the bodhisattva of the *Dīpaṅkara jātaka* within the *Divyāvādāna* collection, which is intimately associated with the *Mūlasarvāstivāda* tradition. In the Pāli *Nidānakathā*, he is called Sumedha; in the *Mahāvastu*, Megha.

<sup>53</sup> On these images see, Suresh Vasant. "Dīpaṅkara Buddha at Ajanta," in *The Age of the Vākātakas*. Ed. by A. M. Shastri (New Delhi: Harman Publishing, 1992): 209–217, and figs. 48–55; Maurizio Taddei. "Appunti sull'iconografia di alcune manifestazioni luminose dei Buddha," in *Gururājamañjarikā. Studi in onore di Giuseppe Tucci*. Volume 2 (Napoli: Istituto Universitario Orientale, 1974); Maurizio Taddei. "The *Dīpaṅkara-jātaka* and Siddhārtha's Meeting with Rāhula: How Are They Linked to the Flaming Buddha?," *Annali di Istituto Universitario Orientale*. 52 (1992): 103–107.

<sup>54</sup> Burgess. *Report*, 132.

<sup>55</sup> Gupta. "Authorship of Ajanta Caves 17 to 20," 102–3.

<sup>56</sup> These figures, like those presented in my prior discussion, were created using a flatbed scanner etc. However, because the estampage found in Burgess's *Report* has served as the basis for all analyses of this inscription, I reproduce that facsimile even though it is "worked up": a process that makes the individual letters more distinct at the price of introducing bias into all subsequent use of the estampage. Moreover, because I use Burgess' "work up" estampage for the Cave 20 record, I have used his estampage of the Cave 17 inscription for parity.

<sup>57</sup> Again, allow me to clear up a possible point of confusion. One may wonder about the basis upon which this *akṣara* was originally read as *kr*. The answer is that *kr* with a right-facing hook is found in India's North at least as early as the Allahabad pillar inscription of Samudragupta (mid-4th century). However, the thesis presented by Gupta, and the point to which I am responding in the body of my essay, is that the Cave 17 and Cave 20 inscriptions have a paleographical kinship. A northern-style, right-hooked *kr* is found neither in the Cave 17 inscription, nor anywhere else at Ajaṅṭā, a southern site paleographically speaking.

Surely, a scribe from India's North could have been responsible for the Cave 20 inscription (to make this case, one is required to explain why that scribe would have used a southern-style *ma* alongside the northern *kr*). However, this latter proposition would not in anyway bolster the argument that Caves 17 and 20 had a shared source of patronage.

<sup>58</sup> See V. V. Mirashi. *Indological Research Papers*. Volume 1. (Nagpur: Vidarbha Samshodhan Mandal, 1982): 78–87; Krishna Mohan Shrimali. *Agrarian Structure in Central India and the Northern Deccan (c. AD 300–500): A Study in Vākātaka Inscriptions* (Delhi: Munshiram Manoharlal, 1987): 82–3; A. M. Shastri "New Vākātaka Inscriptions" in Shastri. *Age of the Vākātakas*, 251–253. While in India in 1992, I heard report that another Vākātaka copperplate inscription had been found in Dhulia, near Thālnr. There was further rumor that this record belonged to Hariṣeṇa himself. Unfortunately, though this rumor was known to several Vākātaka scholars and epigraphists, none had seen the record or could even confirm its existence.

<sup>59</sup> V. V. Mirashi. "Historical Data in Daṇḍin's Daśakumāracarita," *Annals of the Bhandarkar Oriental Research Institute*. 26 (1945): 20–31.

<sup>60</sup> Walter M. Spink. "The Achievement of Ajanta," in Shastri. *Age of the Vākātakas*, 183.

<sup>61</sup> Ajay Mitra Shastri. "The Vākātakas: Original Home and Some Other Problems," in Shastri. *Age of the Vākātakas*, 15.

<sup>62</sup> Brahmanand Deshpande. "Dating of the Ajintha Caves in the Context of Vakataka Decline," *Pathik*. 3.4 (1992): 8–14; Khandalawala. "History and Dating," 18; Williams. *Art of Gupta India*, 182.

<sup>63</sup> Mirashi. *Inscriptions*, 108.

<sup>64</sup> Burgess. *Report*, 127; Mirashi. *Inscriptions*, 110.

<sup>65</sup> I make this latter suggestion, as it is indeterminate whether this verse's initial *sa* is a personal pronoun or the first element in the compound the follows.

<sup>66</sup> Shobhana Gokhale. "Epigraphical Evidence on Hariṣeṇa's Conquest from Kanheri" in Shastri. *Age of the Vākātakas*, 269.

<sup>67</sup> Gokhale, "Epigraphic Evidence," 277.

<sup>68</sup> Spink. "Archaeology of Ajaṅṭā," 70.

## REFERENCES

- Allen, J.: 1933, 'A Note on the Inscriptions of Cave II', appendix to G. Yazdani. *Ajanta*, vol. 2 (Oxford University Press, London), pp. 57–63.
- Begley, W. E.: 1966, *The Chronology of Mahāyāna Buddhist Architecture and Painting at Ajaṅṭā* (University Microfilms, Ann Arbor, MI), pp. 3, 4, 20, 51.
- Burgess, J. and B. Indrajī: 1976, *Inscriptions from the Cave Temples of Western India with Descriptive Notes, &c.* (Indian India, Delhi), pp. 67–88.
- Burgess, J.: 1975, *Report on the Buddhist Cave Temples and their Inscriptions* (Bharatiya Publishing House, Varanasi), pp. 124–138.
- Chakravarti, N. P. and B. Ch. Chhabra.: 1955, 'Note on the Painted and Incised Inscriptions of Caves XX–XXVI', appendix to G. Yazdani. *Ajanta*, vol. 4 (Oxford University Press, London), pp. 113–128.
- Chakravarti, N. P.: 1946, 'A Note on the Painted Inscriptions in Caves VI–XVII', appendix to G. Yazdani. *Ajanta*, vol. 3 (Oxford University Press, London), pp. 91, 95.
- Daji, B.: 1863, 'Ajanta Inscriptions', *Journal of the Bombay Branch of the Royal Asiatic Society* 7, pp. 53–74.
- Deshpande, B.: 1992, 'Dating of the Ajintha Caves in the Context of Vakataka Decline', *Pathik* 3.4, pp. 8–14.

- Dutt, N.: 1984, *Gilgit Manuscripts*. Vol. 3, part 2 (Sri Satguru, Delhi), pp. 125, 143.
- Fergusson, J. and J. Burgess: 1969, *The Cave Temples of India* (Oriental Books Reprint, Delhi), pp. 299.
- Gnoli, R.: 1978, *The Gilgit Manuscript of the Śayanāsanavastu and the Adhikaranavastu, Being the 15th and 16th Sections of the Vinaya of the Mūlasarvāstivādin* (ISMEO, Rome), pp. 10–11.
- Gokhale, S.: 1973, 'Epigraphical Evidence for the Chronology of Ajanta', *Journal of Indian History* 51, pp. 479–483.
- Gokhale, S.: 1992, 'Epigraphical Evidence on Hariṣeṇa's Conquest from Kanheri', in *The Age of the Vākātakas*. Ed. by A. M. Shastri (Harman Publishing, Delhi), pp. 269–278.
- Gupta, C.: 1991, 'Authorship of Ajanta Caves 17 to 20' in *The Art of Ajanta*. Ed. by Ratan Parimoo, et al. (Books and Books, Delhi), pp. 100–104.
- Khandalawala, K.: 1990, 'The History and Dating of the Mahayana Caves of Ajanta', *Pathik* 2.1, pp. 18–21.
- Lüders, H.: 1903, 'Ārya Śūra's Jātakamālā and the Frescoes of Ajaṅṭā', *Indian Antiquary* 32, pp. 326–28.
- Malandra, G. H.: 1982, 'The Date of the Ajaṅṭā Cave 27 Inscription', *Wiener Zeitschrift für die Kunde Sudasiens* 26, pp. 37–46.
- Mirashi, V. V.: 1945, 'Historical Data in Daṇḍin's Daśakumāracarita', *Annals of the Bhandarkar Oriental Research Institute* 26, pp. 20–31.
- Mirashi, V. V.: 1963, *The Inscriptions of the Vākātakas. Corpus Inscriptionum Indicarum* vol. V (Government Epigraphist for India, Ootcamund), pp. v–vi, xxix–xxxiii, 103–111, 120–129.
- Mirashi, V. V.: 1982, *Indological Research Papers*. Vol. 1 (Vidarbha Samshodhan Mandal, Nagpur), pp. 78–87.
- Prinsep, J.: 1836, 'Facsimiles of Various Ancient Inscriptions', *Journal of the Asiatic Society of Bengal*. 5, pp. 348–9, 556–561, plate ix, # 4, plate xxviii, # 9, # 10, # 11.
- Ramesh, K. V.: 1977, 'The [F\*]utility and (F)utility of Paleography in Dating Undated Inscriptions', *Studies in Indian Epigraphy* 3, pp. 156–162.
- Schlingloff, D.: 1988, *Studies in the Ajanta Paintings* (Ajanta Publications, Delhi), passim.
- Schopen, G.: 1990, 'The Buddha as an Owner of Property and Permanent Resident in Medieval Indian Monasteries', *Journal of Indian Philosophy* 18, pp. 181–217.
- Shastri, A. M.: 1989, 'Māṅḍhal Plates of Pṛthivshēṇa II, Years 2 and 10', *Epigraphia Indica* 41, pp. 159–180.
- Shastri, A. M.: 1992, 'The Vākātakas: Original Home and Some Other Problems', in *The Age of the Vākātakas*. Ed. by A. M. Shastri (Harman Publishing, Delhi), pp. 3–20.
- Shastri, A. M.: 1992, 'New Vākāṭaka Inscriptions', in *The Age of the Vākātakas*. Ed. by A. M. Shastri (Harman Publishing, Delhi), pp. 209–269.
- Shrimali, K. M.: 1987, *Agrarian Structure in Central India and the Northern Deccan (c. AD 300–500): A Study in Vākāṭaka Inscriptions* (Munshiram Manoharlal, Delhi), pp. 82–83.
- Sircar, D. C.: 1959–60, 'Inscription in Cave IV at Ajaṅṭā', *Epigraphia Indica* 33, pp. 259–262.
- Sircar, D. C.: 1971, *Studies in the Geography of Ancient and Medieval India* (Motilal Banarsidass, Delhi), pp. 262.
- Spink, W. M.: 1968, 'Ajaṅṭā's Chronology: The Problem of Cave Eleven', *Ars Orientalis* 7, pp. 155–168.
- Spink, W. M.: 1975, 'Ajaṅṭā's Chronology: The Crucial Cave', *Ars Orientalis* 10, pp. 143–169, plates 1–16.
- Spink, W. M.: 1991, 'The Archaeology of Ajaṅṭā', *Ars Orientalis* 21, pp. 67–94.

- Spink, W. M.: 1992, 'Reply to K. Khandalawala, A. Jamkhedkar, B. Deshpande', *Pathik*. 3.4: 16–25.
- Spink, W. M.: 1992, 'The Achievement of Ajanta', in *The Age of the Vākātakas*. Ed. by A. M. Shastri (Harman Publishing, Delhi), pp. 177–202.
- Strong, J.: 1977, 'Gandhakuṭi: The Perfumed Chamber of the Buddha', *History of Religions* 16, pp. 390–406.
- Williams, J.: 1982, *The Art of Gupta India: Empire and Province* (Princeton University Press, Princeton), pp. 6, 182.

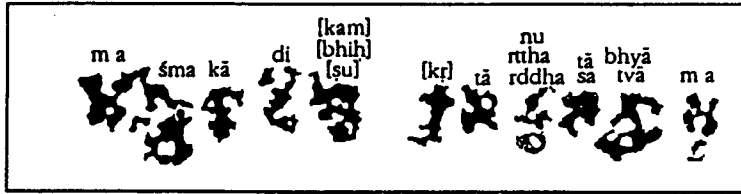


Figure 1: Cave 17's line 10. Note, this figure has been edited for ease of reading.



Figure 2: The akṣara read variously as [ṣu], [kam], or [bhīḥ].



Figure 3: A sample ka, from line 28.



Figure 4: A sample ṣu, from Cave 16, line 13.

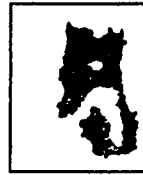


Figure 5: A sample ā, from line 13.



Figure 6: The akṣara read variously as ma and /va.



Figure 7: A sample ma, from line 29.



Figure 8: A sample va, from line 29.

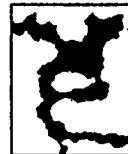


Figure 9: A sample ṣu from line 17.



Figure 10: Cave 20's akṣara, read variously as kr and kṣa.



Figure 11: A sample kr, from Cave 17, line 6.



Figure 12: A sample kṣa, from Cave 17, line 25.



Figure 13: A sample kṣu, from Cave 17, line 5.