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**Translating Vaidya's *Harivaṃśā***

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Gwasg Prifysgol Caerdydd

# **Translating Vaidya's *Harivaṃśa***

Simon Brodbeck

## **Abstract**

This paper is a philological and textological complement to a new translation, produced in Cardiff, of the Sanskrit *Harivaṃśa*, the final part of the *Mahābhārata*. The paper is a project report, providing a detailed behind-the-scenes account of the translation project. It discusses the specific 118-chapter published text that was chosen for translation, and the reasons for that choice; it discusses the emendations that were made to that text, and the reasons for making them; it discusses the method, process, and conventions of the translation, with particular reference to the intended audience, the accompanying apparatus, the format of the translation, its literary and linguistic register, and the treatment of specific words; and it discusses a selection of problematic passages in relation to previous translations. The paper's discussion of the Sanskrit *Harivaṃśa* is wide-ranging, rigorous, unprecedentedly in-depth, and makes significant original contributions in the fields of Sanskrit philology, translation studies, and world literature. The appendix to the paper is a searchable electronic version of the Sanskrit *Harivaṃśa* text that was translated.

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## Preface: The Cardiff *Harivaṃśa* Project

The idea of translating the Sanskrit *Harivaṃśa* was suggested to me by James Hegarty. In 2008, in connection with our project on genealogy in India, I translated the genealogical chapters 8–10 and 20–29, and we had hoped to do more together. A few years later, a project for a full translation was proposed to the British Arts and Humanities Research Council (AHRC) and generously funded by them from 2011 to 2014, led by Will Johnson, with me as the main translator. The proposal to the AHRC emphasised that the audience for the translation would be the general public. After the end of the funded period and the retirement of Will Johnson, I have had the great honour of completing the project, and this document is its valedictory report.

The principal output of the Cardiff *Harivaṃśa* project is the translation, packaged with apparatus appropriate to facilitate its appreciation by its intended audience, and published in New York by Oxford University Press (Brodbeck in press). A subsidiary output is this report, which serves as a technical complement to that published book. Additional outputs so far have been a 5000-word encyclopedia entry on the *Harivaṃśa* for Brill's Encyclopedia of Hinduism (Brodbeck in press b), an annotated bibliography on the *Harivaṃśa* for Oxford Bibliographies Online (Brodbeck 2013), an article on the *Mahābhārata* critical edition with particular reference to the *Harivaṃśa* (Brodbeck 2011), an article on the *Harivaṃśa*'s substories (Brodbeck 2016), and an article about some characters in the *Harivaṃśa*'s solar lineage (Brodbeck 2018). Other articles may follow.

This report contains some of the kinds of material that might have been appropriate in the luxuriant introduction and apparatus of the translation were that translation aimed at a more narrow scholarly audience. But it is also intended as an operational record of what needed to be done, in what order, and what the considerations were, to make the translation. To that degree I hope that some parts of it might be of interest to translators or would-be translators from any language.

The report has three parts. The first part discusses which Sanskrit edition of the text was to be translated, and explains the choice of P. L. Vaidya's critical edition, published in Poona in 1969. The second part discusses the problems that were found with the text as printed in that edition, and the emending remedies that were made in 32 instances to deal with those problems and establish a sensible text to translate. The third part of the report discusses the rendering of the text into English, the fitting of the product to its audience. It discusses considerations for the translation and its presentation and the decisions made, beginning with general principles and ending with a short sequence of specific cases, discussed in detail and arranged in textual order.

Tyler wrote that 'it is the duty of a translator to attend only to the sense and spirit of his original, to make himself perfectly master of his author's ideas, and to communicate them in those expressions which he judges to be best suited to convey them' (Tyler 1907 [1813]: 7). In a similar vein, E. V. Rieu, translator of the *Odyssey* and founder editor of the Penguin Classics series, is credited with advising translators to 'make up your mind as to

what it means and say that as clearly as you can' (quoted in Sandars 1987: 149; cf. Doniger O'Flaherty 1987: 122). In general I have sought to follow this advice, and the division between the second and third parts of this report is intended to mark the approximate distinction between the process of understanding the Sanskrit version and the process of formulating the English version. The report's appendix is an electronic version of the Sanskrit text translated, in the form of several searchable Microsoft Word documents.

Prototype versions of parts of this report have been presented at several conferences and seminars. A version of the first part was presented at the annual Seminar on the Sanskrit Tradition in the Modern World in Manchester on 23 May 2014. A version of the second part was presented at the Seventh Dubrovnik International Conference on the Sanskrit Epics and Purāṇas on 12 August 2014. Versions of the third part were presented at a Cardiff University Department of Religious and Theological Studies Seminar on 7 December 2011 (with Will Johnson), and at the Sixteenth World Sanskrit Conference in Bangkok on 30 June 2015. Summary overviews of the project, or discussions of specific aspects, were also presented to the new Cardiff University Vice-Chancellor during his visit to the School of History, Archaeology and Religion on 8 May 2014; at a Cardiff University Open Day on 28 January 2015; at a 'Public University' event at Chapter Arts Centre, Cardiff, on 6 August 2015; at a University of Bristol Department of Religion and Theology Seminar on 6 December 2016; at a Cardiff University Department of Religious and Theological Studies Research Day on 12 July 2017; and at the Eighth Dubrovnik International Conference on the Sanskrit Epics and Purāṇas on 14 September 2017. This report and/or the translation that it accompanies have been significantly improved as a result of the suggestions kindly made on those occasions, particularly those made by Jackie Hirst.

In terms of the translation project as a whole, thanks to Will Johnson, who was my project partner before his retirement, and who has been helpful and charming throughout. For their encouragement and support thanks also to my other colleagues at Cardiff University, in particular James Hegarty, Josef Lössl, Max Deeg, and Mansur Ali, and to my old colleagues at the University of Edinburgh, John and Mary Brockington and Paul Dundas, and also to André Couture, Alf Hildebeitel, and Adam Bowles, and thanks to the latter also for visiting Mathura and Vrindavan with me in January 2012. For reading and commenting on parts of early drafts of the translation, thanks to Chris Austin, Greg Bailey, Carole Satyamurti, McComas Taylor, and Christophe Vielle, and thanks to the latter also for sending me Dutt's old translation. For advice on specific points of translation, thanks to Mukta Aklujkar, James McHugh, Valerie Roebuck, and Sven Sellmer. For reading bits of the Sanskrit with me, thanks to Michael Delicate, Tim Negus, Molly Robinson, Rebecca Shortland, and David Utton. Thanks to the British Arts and Humanities Research Council for their generous funding, and to Rebecca Blackwell for helping to secure it. Thanks to the team of scholars coordinated by Peter Schreiner, for transliterating Vaidya's edition. Last but not least, thanks to my families old and new, and particularly to Victoria and Llewellyn, for their love and support.

The transmission of text seems to be quite a problem for phenomenology, for, in dealing with it you cannot separate the material (or quasi-material) means, methods, and conditions of the transmission of text from the intentionality of its fixation as an object of consciousness, the transmission of which can be seen as another side of its very being.

(Piatigorsky 1993: 33)

mundus insanit in libros sacros

(Erasmus)



## Part 1. The Edition: *Stemmata Quid Faciunt?*

As Davies notes, ‘One of the problems facing a translator of ... any text where multiforms exist, is to decide what exactly is the “original” text ... Indeed, is there such a thing as an “original” text?’ (Davies 2004: 172). This first part of the report describes and explains the version of the *Harivaṃśa* that was chosen for the translation. It introduces Vaidya’s edition and the method used to produce it, it discusses criticisms that have been advanced against the method and the edition, and it thus explains, in a roundabout way, why Vaidya’s version was chosen, and what it was that was chosen when Vaidya’s version was chosen.

The subtitle, *stemmata quid faciunt*, means ‘What use are pedigrees?’ It is the first sentence of Juvenal’s *Satire* number 8. In that context, the point is explained thus: ‘Though you deck your hall from end to end with ancient waxen images, Virtue is the one and only true nobility’ (trans. Ramsay 1961: 159). The words *stemmata quid faciunt* are also the sometime motto of the Windsor-Clive family, and are found over the fireplace in the front room of that family’s erstwhile pile, St Fagans Castle, which is now part of the Welsh National History Museum and open to the public without charge. Perhaps their original function there was to remind the household and its visitors of Juvenal’s point, and to encourage them to be as virtuous as they were well-bred. A similar formulation, *virtus non stemma*, ‘Virtue, not pedigree’, is the sometime motto of the Duke of Westminster, the Grosvenor Rowing Club, and the Harrow County School for Boys. In response to the question ‘What use are pedigrees?’ placed over this particular fireplace, the modern-day visitor to St Fagans might reflect that genealogical pedigree used to and still does sometimes mean either having or not having a great deal of inherited wealth and property.

The presentation of an ancient work of Sanskrit literature in the form of an accessible book in English might perhaps be similar to the passage of St Fagans Castle and its magnificent grounds from being a private residence to being a public museum. But the main intention behind my iterative redeployment of Juvenal’s phrase as the subtitle of this part of the project report is to remove it, as others have before me (Reeve 1989: 35 says ‘By now the joke has worn rather thin’), into the context of *textual* pedigrees, that is, trees not of human descent but of textual transmission. In this context, the history of philology shows that if a pedigree, tree, or *stemma* that describes a text’s history of transmission can be reconstructed on the basis of the text’s surviving manuscripts, then it can guide decisions as to which of various contrasting readings is the oldest.

### OVERVIEW OF THE POONA CRITICAL EDITION

Vaidya’s critical edition of the *Harivaṃśa*, the final section of the *Mahābhārata*, was published in two parts in 1969 and 1971, as the final stage of the *Mahābhārata* critical-edition project at the Bhandarkar Oriental Research Institute in Poona (now Pune). Thus Vaidya’s *Harivaṃśa* was produced and formatted in the same way as the other volumes of the *Mahābhārata* edition (Sukthankar et al. 1933–71). The Poona *Mahābhārata* project, whose

first editor-in-chief was Vishnu S. Sukthankar (1887–1943), was inaugurated in 1919 with the publication of a prospectus for the *Mahābhārata* edition (Bhandarkar et al. 1919). Such a project had first been mooted by Winternitz more than two decades earlier (see e.g. Winternitz 1899), and a prototype of the first part of the first chapter, based just on manuscripts held in Europe, had been published in the meantime (Lüders 1908, the *Druckprobe einer kritischen Ausgabe des Mahābhārata*). For the positive European response to the Poona project, see the *Proceedings of the Seventeenth International Congress of Orientalists, Oxford, 1929*: 68–69; for a brief overview of the Poona project, see Rocher 1995: 585–86.

The collection and minute comparison of as many manuscript versions as possible was the basis for reconstituting a text imagined to be ‘the ancestor of all extant manuscripts, or, to be precise, of the manuscripts examined and collated for this edition’ (Sukthankar 1933: ciii). Such a text is usually called the ‘archetype’, which Weitzman defines as ‘the oldest text that can be constructed from the extant witnesses’ (Weitzman 1987: 300). That text was then presented verse by verse in the *Mahābhārata* edition, with notes below the main text detailing the variant readings found in the various manuscript versions, as well as the passages found in some manuscripts but not in others, which were assumed to be interpolations (additions) made at various times and places during the long and branching process of textual transmission. Manuscripts did not last long in the monsoon climate (Houben and Rath 2012: 2; Wujastyk 2014: 161), and textual adjustment occurred anew with each new manuscript, created either by copying or by dictation. As Colas has said, ‘Alteration arose almost mechanically from the rapid rate at which texts were recopied’ (Colas 1999: 40).

Figure 1 reproduces a sample page from the edition. This page is from the *Harivaṃśa*, but pages from throughout the *Mahābhārata* edition are presented in the same way. The variants are listed verse by verse and *pāda* by *pāda* (foot by foot, labelled a, b, c, d), and on this page the four-line ‘star passage’ \*529 appears below the line because it is comparatively poorly attested. Where interpolated passages were inconveniently long for presentation as ‘star passages’ below the line, they were presented instead as ‘appendix passages’ at the end of the edition (i.e., for the *Harivaṃśa*, in Vaidya’s second volume, 1971).

Sukthankar stressed that the edition thus presents all the available textual material:

Since all divergent readings of any importance will be given in the critical notes, printed at the foot of the page, this edition will, for the first time, render it possible for the reader to have before him the entire significant manuscript evidence for each individual passage. The value of this method for scientific investigation of the epic is obvious. Another feature of the new edition will be this. Since not even the seemingly most irrelevant line or stanza, actually found in a *Mahābhārata* manuscript collated for the edition, is on any account omitted, this edition of the *Mahābhārata* will be, in a sense, more complete than any previous edition. It will be a veritable thesaurus of the *Mahābhārata* tradition.

(Sukthankar 1933: iii–iv)

89. 19 ]

हरिवंशे

[ हरिवंशपर्व

सोऽग्निं प्राक्सवने दृष्ट्वा ह्यमानं महर्षिभिः ।  
 अवन्दत महातेजाः कृत्वा पौर्वाहिकं विधिम् ॥ १९  
 स ददर्श मखेष्व्वाज्यैरिज्यमानं महर्षिभिः ।  
 भागं यज्ञियमभानं स्वदेहमपरं स्थितम् ॥ २०  
 अभिवाद्याभिवाद्यानामृषीणां ब्रह्मवर्चसाम् ।  
 परिचक्राम सोऽचिन्त्यो ब्रह्मलोकं सनातनम् ॥ २१  
 स ददर्शोच्छ्रितान्युपांशुपालाप्रविभूषितान् ।  
 मखेषु च ब्रह्मर्षिभिः शतशः कृतलक्षणान् ॥ २२  
 आज्यधूमं समाप्राय शृण्वन्वेदान्द्विजेरितान् ।

यज्ञैरिज्यन्तमात्मानं पश्यंस्तत्र चचार ह ॥ २३  
 तमचूर्णययो देवाः सदस्याः सदसि स्थिताः ।  
 अधर्योद्यतभुजाः सर्वे पवित्रान्तरिताननाः ॥ २४  
 स्वागतं ते सुरश्रेष्ठ पद्मनाभ महाद्युते ।  
 इदं यज्ञियमातिथ्यं मन्त्रतः प्रतिगृह्यताम् ॥ २५  
 त्वमस्य यज्ञपूतस्य पात्रं पाद्यस्य पावनः ।  
 अतिथिस्त्वं हि मन्त्रोक्तः स दृष्टः सततं मतः ॥ २६  
 त्वयि योद्धुं गते विष्णो न प्रावर्तन्त नः क्रियाः ।  
 अवैष्णवस्य यज्ञस्य न हि कर्म विधीयते ॥ २७

( for 'नं ). Ds मनीषिभिः ( for महर्षिभिः ). — After 19<sup>ab</sup>,  
 T4 reads 20<sup>ab</sup>, repeating it in its proper place. — ° )  
 Ś1 V3 अविन्दत ( for अव° ). — ° ) G2 पूर्वाहिकं. K N2  
 D1. 2. 4. 5 कृत्वा पूर्वाहिकं ( D1. 2 °हिकं कीं क्रियाम्; N1. 3 V B  
 Dn2 Ds T2 कृत्वा पौर्वाहिकं क्रियाम्; Dn1 G1 कृत्वा  
 पौर्वाहिकां ( G1 °कक्रियाम्; T3 अवस्थानमथाकरोत्.

20 T4 reads 20<sup>ab</sup> for the first time after 19<sup>ab</sup>. — ° )  
 Ś1 [आ]ज्यम्; K1. 3. 4 B3 Ds. 5 M3 [आ]द्यैर्; T4 ( both  
 times) [अ]द्य ( for [आ]ज्यैर् ). — ° ) Ś1 इज्यमानो ( for °मानं ).  
 — ° ) K2 T2 G1-3 M याज्ञि ( M1-3 °ज्ञी ) यम्; G5 याज्ञिकम्  
 ( for याज्ञियम् ). Ds भागं यज्ञार्थमभान्तं; T4 भागक्रियं सम-  
 भानं. — ° ) N3 V1 D1 परमं; T2 पुरतः ( for अपरं ). Ś1  
 K4 N1. 2 Dn D4 स्वदेहप ( D4 °हे प ) रमं स्थितम्; V3 °हं  
 परिसंस्थितम्; B3 ( marg. ) हेन परं स्थितम्.

21 ° ) D1 सोऽभिवाद्य ( for अभि° ). G4 अभिवाद्याभि-  
 वाद्यैव. — ° ) T1. 2 G1. 3-5 यो ( for सो ). D4. 5 सचिन्त्यं  
 ( for °न्त्यो ). — ° ) K2 सनातनः; K3 पुरातनम् ( for सना° ).

22 ° ) G3 चपालाश्रयविष्टितान्. — ° ) D3 यज्ञेषु ( for  
 मखेषु ). K3 M3 मखेषु तव ब्र ( M3 देव ब्र ) हर्षिभिः; Dn T2  
 G1. 5 °षु ब्रह्मर्षिभिः; T3. 4 वंशेषु ब्रह्मर्षिभिः; M1. 2 मखेषु  
 देवो मुनिभिः.

23 ° ) M1-3 मन्त्रान् ( for वेदान् ). — ° ) K2 यज्ञेषु  
 ( for यज्ञैर् ). T1. 4 यज्ञैः समिज्यमानं च. — ° ) D3 ससार  
 ( for चचार ). Ś1 सः ( for ह ).

24 ° ) N1 B Ds T3. 4 ऊचुस्तम् ( by transp. ). — ° )  
 Ś1 K1. 3 T1. 2 G M4 अवोद्य ( Ś1 °व्योदि ) तकराः सर्वे. — ° )  
 N ( except Ś1 N1 ) T G M4 पवित्रान्तरपाणयः. — After 24,  
 K2. 4 N V B D ( except D1; D6 om. ) S read 10-15.

25 Before 25, some Mss. ins. ऋषयः; some देवाः.  
 — ° ) T2 महामते; G3 सुरेश्वर ( for महाद्युते ). — After

25<sup>ab</sup>, Ś1 ins :

529\* नमोऽस्तु ते हृषीकेश मधुकैटभसूदन ।  
 दामोदर नमस्तेऽस्तु पद्मपत्रायतेक्षण ॥  
 प्रभुस्त्वं सर्वदेवानां लोकानां प्रभुरव्ययः ।  
 त्वं यज्ञस्त्वं वषट्कारस्त्वयि सर्वं प्रतिष्ठितम् ॥

— M4 reads 25°-27° after 40. 4. — ° ) V2 Dn1 T1. 2. 4  
 G1-3. 4 M याज्ञि ( G2. 3 M °ज्ञी ) यम् ( for यज्ञि° ). — ° ) T3  
 भवता ( for मन्त्रतः ). N1 V1. 2 B2 ( marg. ) Ds. 4 T2. 4  
 G1. 2. 4. 5 परिः ( for प्रति- ).

26 For the sequence in M4, cf. v. 1. 25. — ° ) Ds1  
 ( sup. lin. ) T1. 2 G1. 3-5 M मन्त्र- ( for यज्ञ- ). K1 N2  
 भूतस्य ( for -यू° ). G2 त्वमत्र मन्त्रभूतस्य. — ° ) T3 सर्वस्य  
 ( for पाद्यस्य ). K4 ( marg. ) वारिणः; D3 G1. 3 M1. 2 पाव-  
 नम्; G2. 4. 5 भावनम्; M3. 4 भाजनम् ( for पावनः ). — ° )  
 G1 आधितस् ( for अतिथिस् ). K4 त्वं च; T3 सर्व- ( for त्वं  
 हि ). D2. 3 नः प्रोक्तः; M4 मन्त्राणां ( for मन्त्रोक्तः ). — ° )  
 K1-3 V1 D2. 3 सुदृष्टः; V3 संदृष्टः; Bom. and Poona eds.  
 संदृष्टः ( for स दृष्टः ). B1 T3. 4 परमो; D5 संततं ( for सत-  
 तम् ). N1 मया; N2 महत् ( for मतः ). N3 सुदृष्टः संततं  
 मतः; Ds D4 सुदृष्टः परमो मतः; T1. 2 G M1. 2. 4 सदस्यः  
 सद आगतः; M3 वेददृष्टेन कर्मणा.

27 For the sequence in M4, cf. v. 1. 25. — ° ) G1. 3  
 अथ ( for त्वयि ). K2. 4 V2 Dn युद्ध-; N1 V1. 3 D1. 3 युद्धं ( for  
 योद्धुं ). Ś1 विप्राः; N1 V2 B1 Ds2 T1. 3 G3 M2 विष्णौ ( for  
 विष्णो ). — ° ) Ś1 K4 D2 सत्क्रियाः ( for न क्रियाः ). — ° )  
 M4 वैष्णवस्य च यज्ञस्य. — D2 transp. 27° and 28°. — ° )  
 B3 प्रवर्तते ( for विधीयते ).

28 D2 transp. 27° and 28°. — ° ) N2 सर्वैष्णवस्य  
 यज्ञस्य. — ° ) K2 V3 D2. 3. 5 M3 त्वत्प्रसूतिः; K3 N1 B2. 3  
 सूतिः; D1 T1. 4 सूत- ( for सूतं ). T2 G5 लभेत् ( for भवेत् ).  
 Ś1 त्वत्प्रसूतिफलं मइत्; N2. 3 V1 B1 Ds D4 त्वत्प्रसूतिः

— 280 —

Figure 1. Sample Page of the *Harivaṃśa* Critical Edition (Vaidya 1969: 280)

In the apparatus, capital letters with subscript numerals denote individual manuscripts (D3, T4, etc.), and asterisks indicate passages found only in some manuscripts (\*529, etc.).

The reconstituted text without apparatus, however, is a very different thing: a hypothetical ancient text, attested only by implication. After the Bhandarkar Oriental Research Institute had published the full *Mahābhārata* critical edition complete with the *Harivaṃśa*, it proceeded to publish the reconstituted text again, without apparatus, in five volumes (Dandekar 1971–76), the last and slimmest volume being the *Harivaṃśa* (Dandekar 1976). That reconstituted *Harivaṃśa* without apparatus is what the Cardiff translation translates.

The next two sections describe the methods used to prepare the *Mahābhārata* edition as a whole, and the *Harivaṃśa* edition in particular. The sections following that are more analytic, discussing criticisms of those methods and of the resulting text.

## METHODOLOGY OF THE MAHĀBHĀRATA EDITION

The method for the *Mahābhārata* critical edition was described in Sukthankar's 'prolegomena' to the first volume (Sukthankar 1933: i–cx). One of the subsections of the prolegomena is entitled 'The Method of Reconstruction Explained' (pp. lxxxvi–xcii), but the whole prolegomena is relevant to that theme. In what follows, I quote frequently from Sukthankar's prolegomena. I refer to the prolegomena in the singular because it is one piece of work, notwithstanding the fact that the word is a plural. Although in the first instance Sukthankar's description of the method concerned just the production of the critical edition of the *Ādiparvan* (Mbh 1), it was programmatic and was applied by the editors of the other *parvans* too.

As wide a range of manuscripts as possible was used, from as many regions and in as many scripts as possible. Also used, in a corroborative function, were testimonia such as passages quoted in commentaries, and old translations, adaptations, and summaries (pp. xxv–xxix). Older manuscripts were selected in preference to newer ones (p. vi), even if they were fragmentary (cf. e.g. p. x on the Ś1 manuscript, and the chart on p. xxiv).<sup>1</sup> The manuscripts were initially classified according to their scripts, on the supposition that because many scribes were familiar with just one script, 'The superficial difference of scripts corresponds ... to deep underlying textual differences' (p. vii; cf. Katre 1941: 29).

The *Mahābhārata*'s critical editors adapted a method that had already been used on many other, primarily European, texts. It is one response, in a print culture where a book can be mass-produced, to a manuscript tradition where no two manuscripts carry exactly the same text. Where several variant readings exist, this method is historically reconstructive in seeking to privilege the older and more original variant.

How can one tell which one out of several acceptable variants is the more original? The Poona editors used the genealogical or stemmatic method that is often associated with

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<sup>1</sup> Brockington lists the oldest manuscript used for each *parvan* of the *Mahābhārata* (Brockington in press: n. 9): these oldest manuscripts date from 1261 to 1679 CE.

the name of Karl Lachmann (1793–1851)<sup>2</sup> and that has been summarised by Paul Maas, amongst others (Maas 1958 [1927]: 2–9; Timpanaro 2005 [1959–60]; Trovato 2017). The editor carefully compares the clear errors (e.g. of transcription or inspiration) or ‘readings of secondary origin’ (West 1973: 32 n. 3) that have entered the manuscript versions. Each manuscript will have peculiar errors of its own, but errors that a subset of manuscripts have in common will have been inherited from an ancestor that they share and others don’t. A genealogical (upside-down) tree of manuscripts (a *stemma codicum*) can thus be drawn, with the extant manuscript versions as twig-tips at the bottom, with lost (or potentially extant) intermediary texts as branching points in the middle (these are called hyparchetypes or subarchetypes), and with the lost singular archetype at the top.<sup>3</sup> Once the *stemma* of the manuscripts is drawn accurately – see Figure 2 for Sukthankar’s *Ādiparvan stemma*, including hyparchetypes N, v, γ, ε, S, and σ – it then allows the editor to decide, in many cases, which of several competing acceptable variants was found in the archetype, on the basis of their distributions within the manuscripts at the bottom.

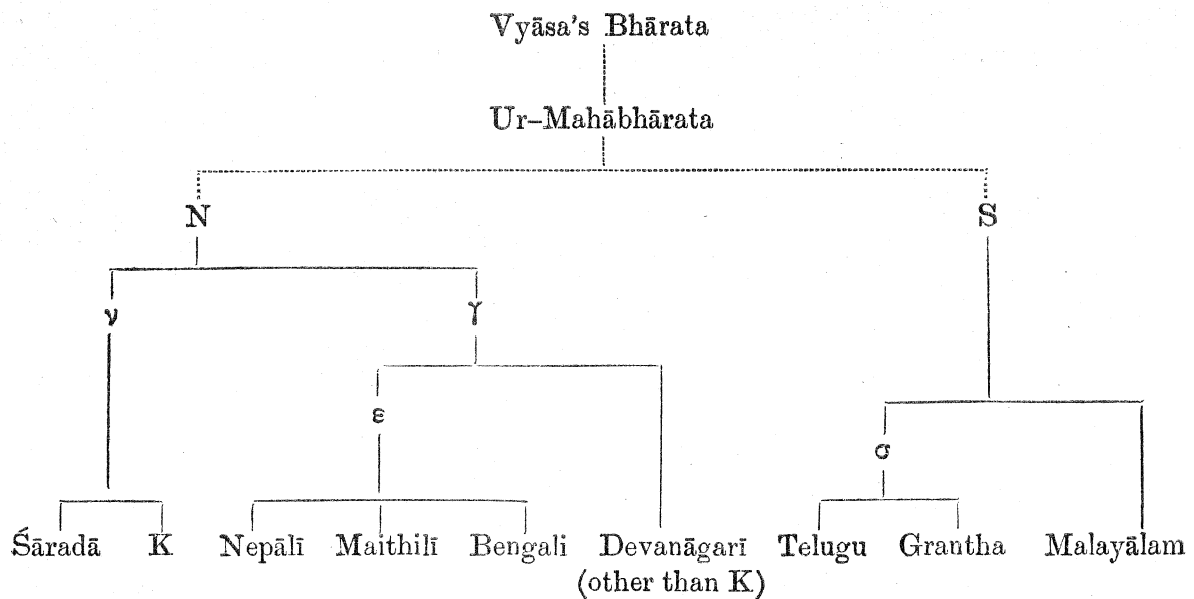


Figure 2. Sukthankar’s *Stemma of Ādiparvan Versions* (Sukthankar 1933: xxx)

The means of stemmatic text-reconstitution, working upwards through the elimination of singular variants, is ingenious and convincing, except where a line of transmission splits into just two branches and there is no majority verdict. In lower levels of the *stemma* one might put a question mark for now, and in the meantime look elsewhere

<sup>2</sup> For details of ‘what really belongs to Lachmann’, see Timpanaro 2005: 115–18; Kenney 1974: 109–12; Schmidt 1988; Hanneder 2017: 134–37.

<sup>3</sup> According to Timpanaro, the first *stemma codicum* was drawn up by Carl Johan Schlyter in 1827 (Timpanaro 2005: 92).

in the diagram; but if the *stemma*'s first split was into two branches (as it seemingly was with the northern and southern recensions of the *Mahābhārata*), then the distribution of variants cannot reveal, when the two first hyparchetypes differ, which of them has the more original reading (cf. points 7 and 19 at Maas 1958: 3, 17–19). This was decided by editorial judgement.

Here Sukthankar used two methods in particular. One was used also as a general method to understand the distribution of variants: some variants were seen as attempts to adjust and make more comprehensible another variant – the ‘more difficult reading’ (*lectio difficilior*) – that was consequently identified as primary (Sukthankar 1933: lv, lxxix, xcii). The general principle of preferring the *lectio difficilior* in textual reconstitution – a principle to which we shall have cause to return – was first formulated by Jean Le Clerc in the early eighteenth century, although it was foreshadowed much earlier (Timpanaro 2005: 68–69).

Sukthankar's other method was as follows:

When the two recensions have alternate readings neither of which can have come from the other and *which have equal intrinsic merit* (N : S), I have, for the sake of consistency and with a view to avoiding unnecessary and indiscriminate fusion of versions, adopted, *as a stop-gap*, the reading of N.

(Sukthankar 1933: xci)

Editorial judgement is still implied here, in identifying parity of ‘intrinsic merit’ (for an instance where the southern-recension variant was preferred, see p. xlvi). But Sukthankar's general preference for the northern recension is not arbitrary. It follows from his judgement that the southern ‘may ... be fitly styled, in relation to the Northern, the *textus ornatior*’ (p. xxxvi), since it shows ‘a tendency to inflation and elaboration’ (p. xlvi). For details of the southern recension's ‘inflation and elaboration’ in individual passages, see Hildebeitel 2011 and 2018.

Sukthankar's identification of the southern-recension variants as generally secondary has been fleshed out in terms of historical migrations of brahmins: see here in particular the work of Mahadevan (2008, 2011, 2013), who hypothesises that a *Mahābhārata* manuscript written in a form of Brahmī script was taken south, early in the Common Era, by Pūrvaśikhā brahmins, and was reworked into the hyparchetype of the southern recension (Sukthankar's ‘S’) during the next few centuries, before the end of the Sangam period and the Kaḷabhra interregnum (Mahadevan 2011: 26; Mahadevan 2013: 64). Conversely, Bagchee, conscious that the southern recension is so called only because on the census date of the Poona edition it dominated the manuscripts written in southern scripts, has observed that the editorial work that led to it need not actually have taken place in the south (Bagchee 2016: 114–15 n. 94; cf. pp. 118–19 n. 100).

Sukthankar's text further emphasises its hypothetical status at various points:

All important elements of the text – lines, phrases, significant words and even word-parts – that are less than certain, are indicated by a wavy *line* printed

below them ... This device is, by nature, hard to apply strictly, and there are bound to be many inconsistencies in its application. I have retained it all the same with the express object of obviating all false sense of security.

(Sukthankar 1933: cii)

If this device is ‘hard to apply strictly’ (cf. Edgerton 1928: 188–89; Sukthankar 1930: 280–81), then it is also hard to interpret strictly. But *Harivaṃśa* 39.26a is a good example (see again Figure 1), where there is no special reason to consider the well-attested southern-recension variant (*mantra*) to be secondary, so the reading *yajña* has been preferred, with a wavy line. Vaidya spells out his own use of the wavy line in the introduction to the Poona edition’s *Karṇaparvan*, giving examples of its use to mark the privileging of the northern-recension variant in cases where the bipartite *stemma* means that the choice between variants cannot be made on stemmatic grounds (Vaidya 1954: xxxvi–xxxix). This is its basic use throughout the *Mahābhārata* edition.

The method described above is concerned to ascertain which of several variant versions of a word, line, or verse is the most original. But perhaps more importantly, the *Mahābhārata*’s editors also had to decide whether each particular line, verse, or passage should find a place, in some form or another, within the reconstituted text at all. Here the assumption was that although scribes sometimes added material to the texts they copied (and made mistakes), they did not deliberately take material away. Sukthankar makes the point repeatedly:

No one in the past found the epic text too long. Far from it. It was perhaps not long enough.

... [O]ne notices above all the anxiety that nothing that was by any chance found in the *Mahābhārata* manuscript should be lost. Everything was carefully preserved, assembled in a picturesque disarray.

There is nothing to suggest that our manuscripts have suffered any serious loss at any time. There never was any lack of manuscripts ... There is no evidence of any break in the tradition ... The probable inference is that *our manuscripts contain all that was there originally to hand down*, and more ...

... Our primary evidence being the manuscripts themselves, we are bound to view with suspicion, as a matter of principle, any part of the text which is found only in one recension, or only in a portion of our critical apparatus. Therefore, the evidence for such passages as are contained only in one manuscript, or a small group of manuscripts or versions, or even in a whole recension must be pronounced to be defective.

(Sukthankar 1933: lii, lxxxi, xcv)

Thus the common ancestor of the extant manuscript versions contains just the lines or verses common to all those versions, and is shorter than they are. It was on this basis that Sukthankar singled out Ś1, his only manuscript in the Kashmiri Śāradā script:

Assuming then [its own self-estimate of 7984 stanzas] to be the length of the Śāradā version, it becomes the *shortest* known version of the Ādi, and may, therefore, appropriately be called the *textus simplicior*.

While it is the shortest extant version, it is a demonstrable fact that it contains relatively little matter that is not found, at the same time, *in all other versions of both recensions*. It is clear, therefore, that it must contain, relatively, *less spurious matter* than any other known version. That is precisely the main reason why it is taken as the norm for this edition.

(Sukthankar 1933: xlvi)

Nonetheless, there are passages found in Ś1 but not elsewhere, which have thus been omitted from the reconstituted text (p. lii). So the method is to include material in the reconstituted text if it is found throughout the tradition, and otherwise to include it as a star or appendix passage (i.e., an interpolation). Sukthankar also gives additional arguments demonstrating the to-be-expected narrative or stylistic superfluity of various poorly attested passages (pp. liii, lvii, lxi, etc.). As Colas has said, ‘Classical India preferred the inclusive to the exclusive text, even if the coherence of the work suffered’ (1999: 34).

Sukthankar taking the Ś1 manuscript as ‘the norm for this edition’ did not mean he necessarily privileged its readings. That would have been something like what Greg called ‘the tyranny of the copy-text’ (Greg 1950: 26). Sukthankar’s criticisms of Sastri’s edition of the *Mahābhārata*’s southern recension (Sukthankar 1933: lxxxiv–lxxxvi, cv–cvi), which privileged the readings of one particular manuscript, show that that would not have been the case here. Elsewhere Sukthankar clarifies that ‘I have taken the Śāradā MS. only as the *norm* for my edition; I have not undertaken to reproduce its text *verbatim*’ (Sukthankar 1935: 97; cf. Adluri and Bagchee 2018: 180). Rather, the choice between variants was to be done case by case, on the basis of their distribution within the tradition, as per the stemmatic method (see, e.g., pp. lv–lvi, xcvi).

While summarising the critical edition’s ‘principles of constitution’, Mehendale suggests that ‘that reading was chosen ... which was attested by the largest number of manuscripts in both the [N and S] versions’ (Mehendale 2009: 7). This is not true; rather, the salient end-unit is the regional *version*, not the individual manuscript (see Sukthankar 1933: xci), and the question of majority only applies at the stemmatic branching-points, where the reading of any two of three independent branches is the hyparchetypal reading of all three. In stemmatics, manuscripts are weighed (for their usefulness to the reconstructive project), not counted; many will be ‘eliminated’ (Timpanaro 2005: 47–48, 99–101, 154–56).

The method of placing all comparatively poorly attested passages outside the reconstituted text makes a particularly obvious difference to the text.



[From the editor’s perspective,] it is easier to notice and control for interpolations, especially the obvious interpolations found in the Mahābhārata tradition, than it is to identify the changes to the reading of the archetype. From the nonspecialist’s perspective, the former appear far more significant.

(Adluri and Bagchee 2018: 174)

Thus, much the most striking difference between the reconstituted *Mahābhārata* and the so-called vulgate established by Nīlakaṇṭha in the seventeenth century (Kinjawadekar 1929–36) is that the former is barely three-quarters of the length of the latter (Brockington 1998: 60). Apart from the difference of length, the other difference between the reconstituted version and the vulgate – a difference which depends quite obviously upon stemmatic analysis<sup>4</sup> – is that the reconstituted verses differ in many small and usually insignificant ways from the same verses in the vulgate. Sukthankar calls Nīlakaṇṭha’s text ‘a smooth and *eclectic* but inferior text, of an inclusive rather than exclusive type, with an inconsiderable amount of Southern element’, and says it ‘has acquired in modern times an importance out of all proportion to its critical value, to the utter neglect of far superior texts’ (1933: lxvi, lxvii; cf. lxxxiv).

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<sup>4</sup> Does the identification of interpolations depend upon stemmatic analysis? Adluri and Bagchee answer this question in the affirmative: ‘That we can identify ... passages as interpolations at all is due to the fact that Sukthankar by following the stemmatic method could successfully reconstruct the text of the archetype’ (Adluri and Bagchee 2018: 178); ‘But how are we to identify interpolations if not for the stemmatic method?’ (p. 306 n. 310); ‘The Venn diagram can be used to represent the distribution of interpolations, but this presumes a prior stemmatic analysis to identify the interpolations as interpolations’ (p. 448). The reason why Adluri and Bagchee answer the question in this way is that the manuscript tradition is a large one, and on the bottom rung of Sukthankar’s *stemma* we find abstracted manuscript versions rather than specific manuscripts (see Figure 2 above). In other words, the choice of which manuscripts should be used to represent the different versions is a choice which has the wider stemmatic project in mind: the manuscripts chosen should nicely demonstrate the version’s salient peculiarities in contrast to the other versions. In terms of interpolations, the editor’s constitution of the versions means that if, for example, an otherwise important witness has lost a passage through *saut du même au même* (where the same series of words appears before and after a passage which is then accidentally omitted in the copy), that witness can nonetheless contribute its readings to a working version which includes that passage, and the passage it omits doesn’t have to be seen as absent from the archetype. The identification of interpolations by their relatively poor attestation across the versions is a stemmatic business even in the case of the *Harivaṃśa* where the bottom rung of the *stemma* lists manuscripts or sets of manuscripts, because those individual manuscripts are nonetheless selected to be representative of the various abstracted versions immediately above them (see Figure 3 below; cf. also Figure 5). The editor’s constitution of versions by choosing specific manuscripts to represent them is thus a procedure that has potentially significant consequences for the *stemma* and for the reconstituted text as a whole, not just in terms of the latter’s readings, but also in terms of its extent. We shall return to this issue in the subsection on selection of manuscripts (pp. 50–52 below).

Sukthankar stressed that he was not in the business of creating a smooth text (1933: lxxxvi–xciv, ciii). He did not feel free to adjust features of the reconstituted text that were mandated by the manuscript evidence, no matter how strange he found them. That is: he tended to avoid emendation (cf. Sukthankar 1930: 260; von Hinüber 1980: 40). He was content to present the archetype warts and all, rather than presenting a conjectural pre-archetypal text and heading towards what is labelled on his *stemma* as ‘Vyāsa’s Bhārata’ (see again Figure 2). The label is problematic in this case, because Vyāsa is in the first place only the author within the text, and because the text-titles *Bhārata* and *Mahābhārata* are used interchangeably in the text (Brodbeck 2011: 235–37), so the suggestion that the *Bhārata* would precede the *Mahābhārata* is loaded. In any case, the purpose of that place on the diagram is to emphasise that the reconstructed archetype is not the original. It is some past text, which may or may not have been a typical or well-known example of its kind at the time, and which has behind it an untraceable textual history that impacts upon the whole tradition beneath it.

#### METHODOLOGY OF THE *HARIVAMŚA* EDITION

The 1919 prospectus for the Poona *Mahābhārata* edition indicated that the *Harivaṃśa* would be included (Bhandarkar et al. 1919: 33–34); the 1937 prospectus, apparently issued at a time of particular financial hardship, indicated that it would not be (Sukthankar et al. 1937: 7); but in the end it was. P. L. Vaidya (1891–1978), who edited the *Harivaṃśa*, was the third editor-in-chief of the Poona *Mahābhārata* critical-edition project, from 1961 to 1972. For the Poona edition he also edited the *Karṇaparvan* (Vaidya 1954). He was also involved in the Baroda *Rāmāyaṇa* critical-edition project, as editor of the *Ayodhyākāṇḍa* and the *Yuddhakāṇḍa* (Vaidya 1962, 1971b). For brief obituaries, see Bapat 1978; Dandekar 1979.

In the preface to his *Harivaṃśa* edition, Vaidya wrote: ‘The text-critical methodology adapted here is essentially similar to the one adopted in the case of the *Mahābhārata*, though a few modifications have to be made in it in view of the special character of this *Khilaparvan*’ (Vaidya 1969: vi; for the word *khila* see below, p. 70). But Vaidya didn’t specify what those modifications were.

One small modification is clear on the first page of the introduction that follows, even if its connection to the *Harivaṃśa*’s ‘special character’ is not: alongside the 37 chosen manuscripts, Vaidya used four printed editions. ‘[T]hey were collated fully as each one of the printed editions in its own way, represented a manuscript-tradition which, I feel, may not have come down to us’ (Vaidya 1969: xv). The apparatus mentions these editions from time to time (see, e.g., the apparatus to 39.26d, in Figure 1), but Vaidya doesn’t highlight any instance where the reconstituted text might have been reconstituted differently had these editions not been used.

Another possible specific intimation in Vaidya’s introduction of his departure from Sukthankar’s methods is this comment: ‘The editor has to see, while eliminating a certain passage [on the basis of its comparatively poor manuscript support], that the Critical Text

arrived at by elimination runs smooth and indicates no break in the narrative’ (Vaidya 1969: xxxvi). This potentially conflicts with the following statement of Sukthankar’s: ‘The reader will find that the constituted text is by no means smooth ... There remain many contradictions and superfluities’ (Sukthankar 1933: ciii). Vaidya’s precise meaning here is slightly unclear, since he gives no examples. We shall return to this issue in due course, in the section on criticisms of the *Harivaṃśa* edition. In the meantime, Vaidya’s comment here can be taken to confirm that when passages are placed in the apparatus because of poor manuscript support, the narrative of the reconstituted text has not thereby suffered.

Like Sukthankar’s *Ādiparvan* edition, Vaidya’s *Harivaṃśa* edition used a single Śāradā manuscript, Ś1, which contained the shortest extant version (Vaidya 1969: xvi–xvii). Vaidya’s Ś1 manuscript might not be a continuation of the Ś1 manuscripts used for the editions of the *Mahābhārata*’s other *parvans*,<sup>5</sup> since (presumably for the sake of convenience) *Mahābhārata* manuscripts tend not to be of the whole text. For this reason, there is really no such thing as the critical edition of the *Mahābhārata*, there are only a series of critical editions of its individual *parvans*.<sup>6</sup> However, ‘Some libraries have catalogued as separate codices manuscripts which are by the same copyist but are of different *parvans*’ (Dunham 1991: 2).

Vaidya notes that the *Harivaṃśa*’s Ś1 version is similar to those of the Nepālī (Newārī) manuscript Ñ1 and three Malayālam manuscripts M1–3, and that ‘This surprisingly striking agreement in contents as well as in readings has weighed with me immensely in fixing my text’ (Vaidya 1969: xxii). Such agreement is significant because it is ‘the agreement of the extremes’ (p. xxiv) – that is, geographical extremes (cf. also pp. xxxii, xxxvi). Here Vaidya, like Sukthankar, makes use of a text-critical idea that Timpanaro traces in the writings of Bentley, Bengel, Lachmann, and Pasquali (Timpanaro 2005: 85–87; a key passage from Pasquali is translated by Trovato 2017: 72–73, point 8).

[T]he agreement of some manuscripts of one family with some of the other has greater value if the manuscripts come from places very distant from one another ... [D]istance is a guarantee against ‘horizontal transmission,’ against contamination.

(Timpanaro 2005 [1959–60]: 85)

The *stemma* that Vaidya provides on the basis of his manuscript survey resembles Sukthankar’s *stemma* of *Ādiparvan* versions. Vaidya’s *stemma*, like Sukthankar’s, is not a true *stemma codicum*, because it does not represent every manuscript version with a separate

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<sup>5</sup> Dunham notes that ‘no Śāradā manuscripts of the *Anuśāsanaparvan*, *Mausalaparvan*, *Mahāprasthānikaparvan*, and *Svargārohaṇaparvan* have been found to date’ (Dunham 1991: 3). Bronkhorst tries to use this circumstance to support the idea that ‘the *Anuśāsanaparvan* was not part of the archetype’, but without success (Bronkhorst 2011: 49).

<sup>6</sup> This point was emphasised by Mislav Ježić during the Eighth Dubrovnik International Conference on the Sanskrit Epics and Purāṇas, September 2017.

twig. However, it is closer to being one than Sukthankar’s is, because at least Ś<sub>1</sub>, Ñ<sub>1</sub>, D<sub>6</sub>, and M<sub>4</sub> have their own twigs. Vaidya’s *stemma*, like Sukthankar’s, has the basic division into northern and southern recensions, but it has fewer instances of bifurcation beyond that, and more of trifurcation (p. xxiv; see Figure 3). Vaidya’s *stemma*, laid out in the same horizontal order as Sukthankar’s (left to right = north to south), places Ś<sub>1</sub> at the extreme left and M<sub>1–3</sub> at the extreme right, with the archetype (‘Ur. Text of Harivaṃśa’) as their only common ancestor, thus accentuating, in diagrammatic fashion, the value of their agreement to his reconstitutive exercise.

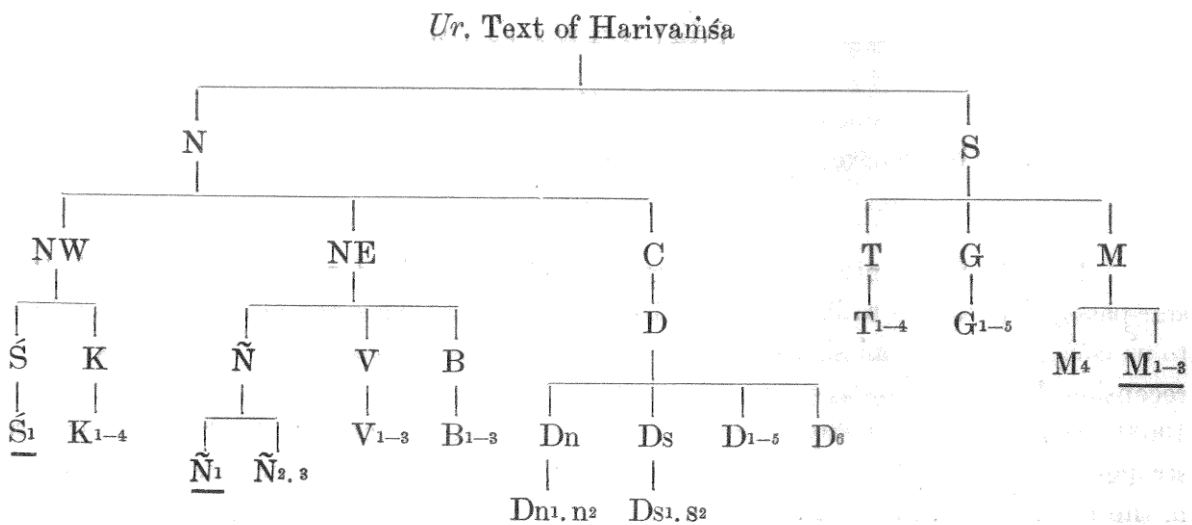


Figure 3. Vaidya’s *Harivaṃśa Stemma* (Vaidya 1969: xxiv)

In Vaidya’s opinion the *Harivaṃśa* was once much shorter than his reconstituted text; but he says he was nonetheless compelled, by the manuscript data, to reconstitute the text as he has (Vaidya 1969: xxx). He thus differentiates the original ‘autograph’ text (the earliest *Harivaṃśa*, not shown on his *stemma*) from the reconstituted archetype. As per Sukthankar’s method, Vaidya placed the material found in all versions in the reconstituted text (p. xxxvi), and the interpolated passages in the apparatus. In the section of his introduction entitled ‘Extent of *Harivaṃśa*’, Vaidya lists the longer passages that were not included in the reconstituted text (i.e., the appendix passages), noting in each case which manuscripts did not include the passage (pp. xxx–xxxii). These data are styled as the ‘reason why [these passages] were not included in my Critical Text’ (p. xxx). Vaidya goes on to give further circumstantial and text-internal reasons for deeming some of these passages to be secondary (pp. xxxii–xxxiv, xl–xliii), as if he thinks that such arguments might constitute a second line of defence against his critics.

Vaidya often stresses the value of Kṣemendra’s *Bhāratamañjarī*, a Kashmiri summary retelling of the *Mahābhārata* and *Harivaṃśa*, in terms of what it does and does not include. ‘Kṣemendra’s date is well-nigh fixed at A. D. 1046, and hence, the “Mañjarī” is very useful in determining the stage of the growth and contents of the various parts of the *Mahābhārata*

and more particularly of *Harivaṃśa*' (Vaidya 1969: xiv). However, to suppose that there was a definite 'stage of the growth' at that time doesn't seem quite right, since we know that the manuscript tradition preserves various versions. Without details of how Kṣemendra himself decided what to include, his *Bhāratamañjarī* can tell us only that certain *Harivaṃśa* episodes were in existence in his day, in a version he knew. Perhaps accordingly, Vaidya uses Kṣemendra only for corroboration (cf. De 1958: xiv–xv).

Here and in some other places, if the reader is convinced to follow Vaidya's lead in taking the manuscript evidence as the sole authority, then – and this is also true to a lesser degree of Sukthankar before him – some of his arguments can be seen as secondary, and potentially as undermining that principle, since they wouldn't be necessary unless its application wasn't justified on its own merits. I suspect that Vaidya was surprised by how much *Harivaṃśa* material ended up in the apparatus (the *Harivaṃśa* edition's entire second volume consists of appendix passages), and that he anticipated his readers would be surprised at this too. So perhaps part of the *Harivaṃśa*'s 'special character', as referred to in Vaidya's preface, is its tendency to accumulate interpolations at a greater rate than other *parvans* of the *Mahābhārata*. However, if that is what Vaidya meant then the comment in his preface is still a bit mysterious, because the methodology of the *Mahābhārata* edition was already well equipped to deal with this circumstance without being modified.

I now survey criticisms of the edition, under three headings: criticisms of the stemmatic method, of the *Mahābhārata* critical edition, and of Vaidya's *Harivaṃśa* edition.

## CRITICISMS OF THE STEMMATIC METHOD

I emphasise that I am very much an amateur in the general field of textual criticism,<sup>7</sup> that I have never collated or edited any old texts, and that what follows are impressions based on too little reading, almost entirely in English. But it seems that the stemmatic text-critical method (Lachmannism), among other methods, is based on a desire for the oldest knowable version of the text. Thus 'the editor is the active repairer of the damages wrought by time' (Tanselle 1995: 16). According to Housman, textual criticism 'is the science of discovering error in texts and the art of removing it' (Housman 1921: 68). In what may be the stemmatic fantasy world, each scribe is imagined to work from a single exemplar, reproducing its peculiarities, most of them inherited from *its* exemplar, and adding new ones.

We can separate criticisms of the stemmatic method initially into criticisms of what the method tries to do (i.e., restore the old text), and criticisms of how well it does it.

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<sup>7</sup> Kenney hazards that the term 'textual criticism' was first used by J. Scott Porter (1848), and says 'It is presumably modelled on German *Textkritik*' (Kenney 1974: 29–30 n. 8).

### The Text-Restorative Project

There is a gulf between objective of the project – the restored text – and the manuscripts that are the subjects of inquiry. It is a gulf between singularity and diversity, and between the hypothetical and the actual. Perhaps ‘philology is the mourning for a text, the patient labor of this mourning ... It is the desire to reduce the troubling image of the other to a primordial sameness’ (Cerquiglini 1999: 34; cf. Parker 2012: 103–04). The ‘other’ – the focus of so-called ‘new philology’ – are the manuscripts, for which variance is constitutive as a primary aesthetic, and none of which are in any sense inauthentic. The stemmatic restorative project reduces manuscripts to bad text-carriers, ignoring other aspects of their artistic and artefactual status within their cultural matrix. Reacting against this reductionism, the ‘new philology’ sought to focus on the manuscript culture (Nichols 1990: 7), studying manuscripts in all their aspects and in their cultural contexts.

Wenzel says ‘we can no longer consider a codex as a mere receptacle that happens to have preserved the text under investigation’ (1990: 14). But even if he may put it like that, still to some intents and purposes we can if we want to. Gumbrecht suggests that ‘the relationship between a neophilological and a Lachmann-style critical edition<sup>8</sup> should be taken as one of incommensurability. They cannot compete with – and they should not be compared to – each other because they depend on incompatible heuristic premises’ (Gumbrecht 2003: 38).

The restorative project can be, as it were, psychoanalysed:

Philology is a bourgeois, paternalist, and hygienist system of thought about the family; it cherishes filiation, tracks down adulterers, and is afraid of contamination. It is thought based on what is wrong (the variant being a form of deviant behavior), and it is the basis for a positive methodology.

(Cerquiglini 1999: 49)

What I call textual eugenics is the practice of creating a text using readings from selected sources (what is generally called eclecticism) and the tendency to attribute to this engineered version a superior value than to the text contained in any of the individual manuscripts.

(Phillips-Rodriguez 2007: 167)<sup>9</sup>

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<sup>8</sup> Neophilological editions would include, for example, facsimile editions of single manuscripts (Pearsall 1985: 105), and, to cope with the volume of material, computer editions (Cerquiglini 1999: 79–82).

<sup>9</sup> In terms of this general meaning of eclecticism – the constitution of a putatively older text by picking and mixing from several surviving ones – Fitzgerald’s ‘for’ is odd when he says that ‘Like Sukthankar’s edition of the *MBh*, Olivelle’s edition of the *Manu* is an eclectic edition, for it was determined that it was not possible to arrive at a rigorously established stemma codicum’ (Fitzgerald 2014: 496, emphasis added). Making an eclectic edition isn’t something an editor would be forced to do, as if as a last resort, were constructing a *stemma* prove to be impossible; it is something a critical editor would want to do with or without a *stemma* (but ideally, with). Perhaps by eclecticism Fitzgerald here has in mind

There are many metaphors here. I suppose value is contextual, and every research project is different. Bailey says:

With its near obsessions with origins philology can run the risk of setting a given text within one particular compositional period and presenting it as a kind of petrified relic of a past age. In part this tendency towards reification – of self as unitary factor of interpretation and text as a bounded artefact of a fixed time – is a product of the self-conscious objectivism of philology ...

(Bailey 2001: 209–10)

Nonetheless, reification facilitates full and precise study; and although it is obviously easy (and perhaps fashionable) to describe textual restoration in derogatory terms, I don't think textual restoration can really be criticised as such. It is a tool for use in a specific historical field of enquiry, which may work to a greater or lesser degree, and the results of whose application will be of more interest to some specialists than to others. For contrasting recent defences of 'philology' by Indologists, see Pollock 2009; Grünendahl 2010.

### Bédier

How successfully can a lost text be reconstituted by stemmatic means? The method was famously attacked by Bédier, who may have been motivated in part by nationalistic considerations (Warren 2013: 120–24), but whose critique is nonetheless crucial (Aarsleff 1985: 105–07; Hult 1991: 117–19; Tarrant 1995: 112–13; Timpanaro 2005: 157–87; Kenney 1974: 133–34). Bédier noted that most *stemmas* constructed by philologists have a primary split into two recensions (hyparchetypes), and suggested that this may be somewhat due to 'la force dichotomique' acting upon the philologists (Bédier 1928: 176), but that in any case it allows them repeatedly to arbitrate between two readings (were there three recensions, the majority reading would be preferred). Bédier's critique would suggest that the stemmatic method is unreliable, the reconstituted text too uncertain, and that perhaps the project should be abandoned, with the editor content to publish a single favoured manuscript, or a selection of them.

Bédier's critique of the frequency of bipartite *stemmas* has been taken seriously and has provoked a lot of thought. Castellani responded in French in a piece entitled 'Was Bédier Right?' (Castellani 1957), which is characterised by Reeve as containing four kinds of argument against Bédier: factual, mathematical, historical, and methodological (Reeve 1986: 58–59). Paul Maas responded briefly in German, with a mathematical argument that was later criticised by Timpanaro. That criticism of Maas was in Timpanaro's oft-rewritten appendix to his book in Italian on *The Genesis of Lachmann's Method*, which was in turn

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something like what Sukthankar called 'indiscriminate fusion of versions'; but when Sukthankar mentions this possibility it is to state explicitly that – and how – he avoided it (Sukthankar 1933: xci, quoted above, p. 13).

criticised by Reeve (Maas 1958: 48–49; Reeve 1986; Timpanaro 2005: 157–87, 207–215, the former pages written before Reeve’s criticism, the latter ones after it).

Despite Reeve’s enumeration, overall I would discern two main types of response to Bédier: those that seek to show that the preponderance of constructed bipartite *stemmas* over multipartite ones is natural and correct, and those that seek to ensure that editors do not construct bipartite *stemmas* unnecessarily. The first of these types of response has borne fruit in the work of Weitzman, whose computer simulations of manuscript birth and death have shown that if the manuscripts surviving at a given time are stemmatised correctly then the *stemma* is very likely to be bipartite (Weitzman 1987: 301–03). If this result is sound, then Bédier’s critique is largely disarmed. Trovato discusses this result clearly without the use of equations, and uses a series of snapshots of manuscript traditions at different times to show how much they can change, how surprisingly recent the archetype of the surviving manuscripts can be, and how small a subsection of the full manuscript tradition (the ‘real tree’) the *stemma* of the surviving manuscripts might sometimes constitute (Trovato 2017: 85–94, and on these latter points also p. 135).

With regard to the second type of response to Bédier – those that seek to ensure that editors do not construct bipartite *stemmas* unnecessarily – generalisations are difficult to formulate, since every manuscript tradition is unique, and so different scholars have to work with different data. Certainly, since Bédier, editors think particularly carefully before constructing a bipartite *stemma*, because if a bipartite *stemma* is used but is incorrect, the choice between variants will sometimes be more dependent on editorial arbitration than it should be. If a bipartite *stemma* is used and is correct, then minimal editorial arbitration is necessary; but that was known long before Bédier.

A third type of response is also possible, by distinguishing two different understandings of a *stemma*: the *stemma* as a tool for the identification of the archetypal variant, and the *stemma* as an accurate representation of the text’s historical transmission. Even if, supposedly in principle, the ability of a *stemma* to work well as a tool requires it to be a functional approximation of a historical process, nonetheless, if we distinguish these two understandings we must admit that for most *stemmas*, the former understanding is more appropriate than the latter, since the history of transmission is touched upon only in order to traverse the space between the extant manuscript versions and the desired archetype. Now, there are cases where the manuscript data are compatible with several possible *stemmas* (Palmer 1968: 67–75), and Grier has argued that ‘the hazards of accepting a false multipartite *stemma* or the arbitrary elimination of a witness far outweigh those of retaining a false bipartite *stemma*’ (Grier 1988: 272), because a false multipartite *stemma* may exclude archetypal readings that an arbitrating editor could have chosen (‘General agreement between any two of the three main streams of tradition creates a presumption of originality’, Edgerton 1944: xlvii; cf. Katre 1941: 50). Grier even says that ‘no matter how likely a multipartite *stemma* appears from the evidence, it must not be accepted’ (Grier 1988: 273). This is a strong claim, but here what to Bédier was a weakness of the method has apparently become a strength. The distinction between a *stemma* as a methodological tool and as a historical representation is nicely made by Phillips-Rodriguez, Howe, and



Windram: they note that ‘even though in many cases the dichotomies presented by the stemmata may not be a matter of reality but of methodological principle, this bifurcating tendency may be beneficial to the editorial process’, and they emphasise that more research is necessary before the historical accuracy of a *stemma* can be ascertained (Phillips-Rodriguez, Howe, and Windram 2010: 34, 41–42).

### Circularity

But how does a *stemma* ‘appear’ from the evidence? Here there is the charge of circularity, credited to Dom Quentin (1872–1935; Kenney 1974: 134–36). Tarrant puts it well:

[H]ow can an editor group manuscripts into families by agreement in error without presuming to know the original reading? ... the readings or other features on which manuscript groupings are to be based must be chosen with particular care ... (Attempts to meet the objection of circularity by replacing *error* with the apparently neutral *variant* ... exchange one logical flaw for a worse one, since *variant* inevitably includes readings of the original, which cannot normally serve as the basis for family groupings.)<sup>10</sup>

(Tarrant 1995: 107)

Kane and Donaldson suggest that

[C]lassification of manuscripts cannot be safely undertaken until the whole text they contain is for practical purposes fixed ... In this situation lodges the ultimate absurdity of recension as an editorial method: to employ it the editor must have a *stemma*; to draw the *stemma* he<sup>11</sup> must first edit his text by other methods. If he has not done this efficiently his *stemma* will be inaccurate or obscure, and his results correspondingly deficient; if he has been a successful editor he does not need a *stemma*, or recension, for his editing.

(Kane and Donaldson 1975: 17–18 incl. n. 10)

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<sup>10</sup> This latter principle is confirmed and discussed by Reeve, who also explores its history, crediting its first systematic application to Gaston Paris in 1872 (Reeve 1988: 450–64). The family of manuscripts that may share an original reading is the family of *all* the manuscripts of that text, rather than any sub-family thereof. As discussed by Adluri and Bagchee, non-inclusion of a particular interpolation cannot therefore be used to establish shared filiation (Adluri and Bagchee 2018: 163–67, 209–47).

<sup>11</sup> This stemmatic editor is, perhaps significantly, male. The historic reach provided by a text-restorative method resembles that of a patriline to a founding ancestor, a common preoccupation in surviving ancient texts (which are androcentric almost without exception), for example the *Mahābhārata* (cf. Brodbeck 2009; Brodbeck 2017: 133–36). Patterson, who argues in support of Kane and Donaldson, calls the restored text ‘the founding father’, and speaks of stemmatics as motivated by ‘*libido*’ (1985: 58, 59).

Kane and Donaldson attempted to reconstitute the ‘B version’ of Langland’s *Piers Plowman* without constructing a *stemma*. *Stemmata quid faciunt?* But their means for arbitrating between variants were comparatively insecure, and Hudson judges that in their edition

Emendation is carried beyond that warranted by manuscript collation or the necessity of sense, to include alterations based on the editors’ view of the total textual history of the poem in all its versions, and on their conception of Langland’s poetic habits.

(Hudson 1977: 42)

Perhaps emendation is inevitably problematic unless kept to a minimum. And without a *stemma* perhaps the editor might as well just publish a few good manuscript versions warts and all, because if the dilemmas that occur during the process of recension (i.e., classifying the manuscripts and grouping them into a *stemma*) cannot be solved, then the only real way around them is no longer to attempt to restore the archetype (Phillips-Rodriguez 2012). But if it is possible to build a *stemma* that works, then the editor may get to the archetype and *then* decide, on a case-by-case basis, whether or not to emend features of the archetype that are identified, by analogy to similar features attested only in segments of the tradition, as scribal alterations (Patterson 1985: 61–64).

Returning to the suggestion of circularity, we note the place in the quotation from Tarrant where he says that ‘the readings or other features on which manuscript groupings are to be based must be chosen with particular care’ (Tarrant 1995: 107). Trovato says that ‘only important monogenetic errors should be used as indicative errors [that is, as errors indicative of stemmatic relations]’ (Trovato 2017: 56).<sup>12</sup> The key to the refutation of the suggestion of circularity is that the features used to group the manuscripts into families and connect them into a *stemma* are not the same features that the *stemma* is then used to arbitrate between. In different terminology: ‘In order to avoid the vicious circle, one must use exclusively manifest errors to build up the *stemma*, and latent errors must under no circumstances be used in this process’ (Eklund 2007: 12; cf. Grier 1988: 272, 278; Katre 1941: 38–39). Eklund’s distinction between manifest and latent errors is crucial, but the problem then is that the more manifest an error is, the more likely it is already to have been corrected somewhere among the descendant manuscripts. Because of this kind of difficulty (which may be a greater or lesser difficulty depending on the specific manuscript tradition), Rau built the *stemma* of the extant *Vākyapadīya* manuscripts on the basis just of their lacunae, thus obviating the need to judge certain variants as manifest errors (Rau 1962: 376; Hanneder 2017: 138–39; cf. Trovato 2017: 55–56, 295). One must also beware of ‘convergent variation’, whereby scribes have made the same alterations independently,

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<sup>12</sup> A nice example of the use of shared errors to establish filiation is the list presented by Zgusta of 17 small errors (misprints, etc.) in Böhtlingk and Roth’s *Sanskrit-Wörterbuch* that were also found in the first edition of Monier-Williams’s *Sanskrit-English Dictionary*, and that thus show beyond doubt that those parts of the latter were copied from the former (Zgusta 1988: 154–55, list B).

leading their versions to look more closely affiliated than they actually are (Patterson 1985: 61–66; Timpanaro 2005: 180).

Trovato points out that a *stemma* drawn up on the basis of manifest errors may be corroborated if the distributions of acceptable variant readings match the distributions of the manifest errors, in which case such acceptable variants (Eklund's latent errors) may serve as 'confirmatory readings' for the *stemma* (Trovato 2017: 116–17).

One of the problems with the computer analysis of manuscript traditions – setting aside the problem of how to feed all that data correctly into a computer – is that a computer allegedly cannot judge which variants are manifest errors. But if it can't, can we? What if errors are manifest to different people to different degrees?

### Contamination

Contamination is also known as conflation (Greg 1927: 56–58), enrichment, and horizontal transmission. It is when scribes use more than one exemplar, so the tradition's branches do not just diverge. To use a fluvial metaphor, 'the streams of tradition are united by canals' (Housman 1938: xxiv). Hanneder states that 'if contamination is present ... every variant, even in a bad, or stemmatically irrelevant manuscript, becomes a viable choice [as the reading of the archetype]' (Hanneder 2017: 66). The extent to which an editor can identify and work around contamination will vary between traditions, the process can be laborious (a computer can help), and the results may be more acceptable to some critics than others, but there is a method for it (West 1973: 31–47; Kenney 1974: 138–42; Mink 2004; Parker 2008: 169–71; Pecchia 2010; Maas 2010; Parker 2012: 80–100).

Tarrant notes that 'Contamination may at times limit or even rule out the use of stemmatics as a reconstructive tool, but the presence of contamination in many traditions does not impugn the validity of the genealogical method' (Tarrant 1995: 109–10). It is thus better to discuss contamination with specific examples than in the context of abstract method. In any case, as Reeve notes, it is principally by applying the stemmatic method and discerning vertical transmission that one would get into a position to ascertain the extent of horizontal contamination (Reeve 1986: 65). By using the stemmatic method, the contamination of some of the descendants of any hyparchetype can be nicely identified as long as there are three or more known branches descending from that hyparchetype, and at least one other known hyparchetype (Adluri and Bagchee 2018: 122–26).

Greg suggests that 'We have ... in the case of conflation, a phenomenon in manuscript genealogy analogous to sexual generation' (Greg 1927: 2 n. 1). Greetham goes further, suggesting that 'contamination ... was the equivalent of introducing females into family trees', and that 'the textualist will be charged with separating the legitimate from the bastardized, the patrilineal descent from the collateral' (1996: 100–101). The metaphor is inviting, but in trying to paint the stemmatic method as patrilineal or patriarchal (see his p. 123 for the latter), Greetham goes too far (cf. Doniger 2009: 18; Palmer 1968: 70–74, with gendered labels). In the absence of contamination, textual reproduction is from a single, ungendered, parent; this differs from human patrilinearity, where the male parent is (or at least can be) privileged precisely because in every generation there are actually two

parents of different genders. When there is contamination, textual reproduction is from more than one parent, but it is not thereby aligned to a sexual model (as Greg and Greetham suppose), since there may be three or more parents, and even if there are just two, they make unequal genetic contributions. Thus it is no more accurate to say that editors who try to evade the problem of contamination are trying to trace the patriline than it would be to say they are trying to trace the matriline. The apparent attractiveness of one such suggestion over the other is due to the sexism often found in the reckoning of human descent (where children may take their father's surname rather than their mother's, etc.). It is not due to any real gendering within the stemmatic model. Paul Maas puts it nicely:

[T]he witnesses are related to the original somewhat as the descendants of a man are related to their ancestor. One might perhaps illustrate the transmission of errors along the same lines by treating all females as sources of error. But the essential point, the aim of reconstructing the original, is not brought out by this comparison.

(Maas 1958: 20)

Perhaps we could imagine text-families as endogamous human groups and contamination as incidents of exogamy, but here too the analogy would be poor, as reproduction is sexual within endogamous groups, and so genetic traits circulate within them – which would be analogous to contamination between closely related branches, rather than to the absence of contamination.

### Scribal Passivity

The stemmatic method is established on a paradigm of scribal activity that hopes for the reproduction of all aspects of the text of one manuscript (the parent) within the text of another (the child), except where the scribe makes mistakes. It is the persistence of the mistakes made by previous generations of scribes within the subsequent generations of texts that allows the construction of the *stemma* on the basis of the manifest errors in the surviving manuscripts, and when acceptable variants arise within the tradition it is their persistence within the descendant texts that allows them to be identified as the reading of a lost hyparchetypal ancestor. However, insofar as we do not actually know, in the first instance, whether scribes were that faithful, there would seem to be a weakness in the method.

In terms of Indian scribal culture, Hanneder presents examples of premodern Indian scribes making various kinds of attempts to edit and correct texts in the process of transmission, and claims that 'In such a transmission the preconditions of the stemmatical method are not met with, since the scholarly scribe is far from mechanically reproducing the text of a single apograph. He applies obvious corrections, he may be aware of and may decide between variants' (Hanneder 2017: 87; cf. Colas 1999: 33–35). Dutta gives a list of anticipated types of correction, adding also 'reckless corrections' (Dutta 1971: 31–32). But it

is a caricature to say that mechanical scribal reproduction is a *precondition* of the stemmatic method (cf. Hanneder 2017: 99, ‘pre-modern Indian textual transmission is in parts far from the scenario *that would be required* for stemmatics to work’, italics added). One might rather say that where the stemmatic method will be most easily successful, the scribes will be non-interventional. But Hanneder’s critique on the basis of scribal creativity is nonetheless significant, and it combines with the critique on the basis of contamination, since in both cases the scribe fails to transmit a single exemplar faithfully.

It is largely by combining these last two critiques that Hanneder argues that the stemmatic method is on the whole not practically applicable to Indian texts (cf. Schwartz 1909: cxxvii for Greek texts). However, we should note that such a judgement is as broad as the judgement of Witzel’s that Hanneder is here arguing against (to the effect that more Indian texts should be stemmatically edited, Witzel 2014: 49–50). Neither generalisation can usefully be applied in advance to any specific manuscript tradition, since each manuscript tradition is unique. It was only after a detailed survey and study of *Mānava Dharmaśāstra* manuscripts, for example, that Olivelle suggested ‘all extant mss. of the *MDh* are at some level hybrid ... It is, therefore, impossible to isolate different recensions of the text’ (Olivelle 2005: 370–71).

The various criticisms outlined and discussed above highlight the uncertainty of the stemmatically reconstituted archetype, and the crucial role of the editor. But ‘uncertainty is the price that must be paid for the possibility of a more accurate text’ (Tanselle 1995: 17). Tanselle admits the role of editorial judgement:

[A]ny rationale of critical editing that seeks to limit (rather than to systematize) the role of judgment is not coherent, since by definition critical editing exists to draw on the strengths of human judgment as a means of correcting the defects of documentary texts ... The tradition we now call Lachmannian has been conducive to a way of thinking that – in the urge to place editing on a foundation of scholarly rigor – exaggerates both the need for and the attainment of objectivity in the reconstruction of texts.

(Tanselle 1995: 19, 20)

However, insofar as the editorial task is not mechanical, ‘every editor adopts roles that are close to those of singers, poets, or authors’ (Gumbrecht 2003: 26), and this can easily seem to be a methodological weakness.

## CRITICISMS OF THE MAHĀBHĀRATA EDITION

As with the criticisms of the stemmatic method, criticisms of the *Mahābhārata* edition can be divided into general criticisms of the project, and more specific criticisms of the method. We start with the first type.

### Status of the Reconstituted Text

Sylvain Lévi's review of Sukthankar's first three fascicules (Lévi 1929) praised the Poona project for assembling so much manuscript data, but suggested that this reconstituted text was simply a new recension (cf. von Hinüber 1980: 40), and that Sukthankar should stop trying to restore the 'Ur-Mahābhārata' and instead reprint Nīlakaṇṭha's vulgate with full apparatus (thus combining all the same manuscript data into one edition, but differently). It is not just a matter of whether or not Sukthankar, given the extant manuscripts, is capable of restoring the archetype; Lévi says that 'The very existence of an "original Mahābhārata" is a singularly obscure and perhaps deceptive question' (Lévi 1929: 348).<sup>13</sup> Imagining a first written *Mahābhārata* in an oral culture where bards varied the text at will, Lévi reasons that 'the written archetype, supposing it existed, would have been unable to annul the variants it hadn't written down' (ibid.).<sup>14</sup> By implication, subsequent written *Mahābhāratas* would not have depended upon that 'archetype' in a straightforward fashion, but would have taken variants freely from parallel oral traditions (cf. Barnett 1929, another review which makes the same suggestion).

Sukthankar explicitly refers to Lévi's review in his prolegomena, in order to refute Lévi's suggestion that the vulgate should have been used as the base text (Sukthankar 1933: lxxxiii–lxxxiv), and in order to clarify that the reconstituted text does not claim to be the 'Ur-Mahābhārata' (p. ciii n.). In his prolegomena Sukthankar also made, without reference to Lévi, some important statements with which Lévi could agree. In Lévi's review of the *Ādiparvan's* final fascicule, which included Sukthankar's prolegomena, Lévi admits to having previously misunderstood Sukthankar, whose claims for his reconstituted text are milder than Lévi had supposed (Lévi 1934: 282). Lévi quotes several of Sukthankar's statements (in French translation) with approval: 'Our objective can only be to reconstruct *the oldest form of the text which it is possible to reach, on the basis of the manuscript material available*'; 'Our objective should ... not be to arrive at an archetype (which practically never existed)'; and '*Ours is a problem in textual dynamics, rather than in textual statics*' (Sukthankar 1933: lxxxvi, cii).

For accounts of Sukthankar's life and work, see Katre 1943, 1945. After growing up in Bombay and completing his first degree in mathematics at the University of Cambridge, Sukthankar trained in Berlin under Lüders, who edited the 1908 *Druckprobe*. In his prolegomena Sukthankar says of Lüders: 'What little merit there may be in the present work is due wholly to that excellent though somewhat rigorous and exacting training in philological methods which I had the benefit of receiving at his hands' (Sukthankar 1933: cx; on this training, see also Morgenroth 1978). Lévi was French. The dispute between Lachmannism and Bédierism was driven by nationalism to some degree:

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<sup>13</sup> 'L'existence même d'un "Mahābhārata primitif" est une question singulièrement obscure, et peut-être décevante.' All translations are mine unless otherwise stated.

<sup>14</sup> 'L'archetype écrit, à supposer qu'il ait existé, ne pouvait annuler les variantes qu'il n'avait pas notées.'

In 1894, Joseph Bédier declared it ‘humiliating’ that Germans were recovering French literature before the French; again in 1913, he complained that it was ‘deplorable’ to have to read the [*Chanson de*] *Roland* in ‘detestable’ German editions. In terms of textual scholarship, then, France needed not only a *Roland* produced by a Frenchman but one produced according to methods both ‘scientific’ and properly ‘French.’

(Warren 2013: 121)<sup>15</sup>

Lévi was impressed by the manuscript research that Sukthankar and his team had done, but his suggestion in the 1929 review, to arrange the apparatus around a good extant version, not a reconstituted archetype, follows in the footsteps of Bédier. But that would have been a different project; *this* project was to arrange the apparatus around the archetype reconstructed using the stemmatic method. Sukthankar is clear about this when he says that his reconstituted text ‘may be regarded, if the editor has done his work properly, [as] the ancestor of all extant manuscripts, or, to be precise, of the manuscripts examined and collated for this edition’ – which is a precise definition of *archetype*. And yet when Sukthankar writes the words that Lévi quotes with approval, and particularly when he writes that because ‘the *Mahābhārata* is not and never was a fixed rigid text ... Our objective should ... not be to arrive at an archetype (which practically never existed)’, he seems to echo Lévi’s concerns about how the existence of a *Mahābhārata* oral tradition might have affected the top of the *stemma*. Thus Sukthankar’s prolegomena does not speak with just one voice; rather, it presents two different views of the reconstituted text, one of which seems to be a critique of the other. Perhaps Sukthankar, who was trained in and operated according to one European national method but was critiqued on the basis of another, sought in his prolegomena somehow to hedge his bets and keep everyone happy. Had Sukthankar written his prolegomena without reading any reviews of the published fascicules, perhaps it might have been more strenuously Lachmannian than it is.

Sukthankar’s conflicted position on the status of the reconstituted text is also clearly connected to his concept of ‘epic’. But before I present the crucial quotation from Sukthankar on this subject, some remarks are in order concerning the reference of the term ‘epic’ as used in *Mahābhārata* studies. The supposed genre-term ‘epic’ is often used to denote the entire *Mahābhārata* or *Rāmāyaṇa*, and in this usage it would thus include elements that Hopkins would have called ‘pseudo-epic’ (Hopkins 1993 [1901]). It is now

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<sup>15</sup> Cf. Housman’s comment on patriotism: ‘in the sphere of intellect it is an unmitigated nuisance’ (Housman 1921: 71). According to Gumbrecht, local circumstances meant that ‘historical thinking came to prevail in Germany as the dominant intellectual style’, in contrast to a synchronic humanities model in France (Gumbrecht 1986: 12). On the German research style in Indology, cf. Goldman 2004: 31–32; Adluri and Bagchee 2014. Von Stietenron, introducing Bailey’s translation of the *Gaṇeśapurāṇa*, says that ‘I myself belong to the German text historical school’, anticipating that his readers will understand this reference without need of further explanation (von Stietenron 1995: x).

fairly common for scholars to use the word in this encapsulatory sense (see e.g. the title of Feller 2004, and of the Dubrovnik conference); and indeed, Sukthankar himself was accustomed to using it in this sense (see the quotations from Sukthankar reproduced on pp. 9 and 14 above). But because of its history, even when used in this sense the word ‘epic’ always seems to encode the old European view which sees, for example, Bhīṣma’s teachings (Tokunaga 2005, 2009) and the *Harivaṃśa* (Oberlies 2003) as late and secondary. Indeed, despite and alongside the encapsulatory sense, the word ‘epic’ is also still sometimes used in a narrower sense to cover only some parts of the *Mahābhārata*, considered the most ancient (see e.g. the titles of McGrath 2004, 2009, 2011, 2013). For these and associated reasons, I try not to use the imported genre-term ‘epic’ to denote the entire *Mahābhārata* or *Rāmāyaṇa*. In terms of simple reference, the titles of those texts will serve perfectly well, and do not leave the reader to wonder what an epic is, or why the writer is so keen to repeatedly state, without explanation, that he or she views this text as such.

Here is the quotation from Sukthankar on the concept of ‘epic’:

As regards fluidity: to conceive of the Epic of the Bhāratas – or for that matter, of any true epic – as a rigid or fixed composition like the dramas or poems of Goethe or Milton, or even of Kālidāsa or Bhavabhūti, would be manifestly grotesque. Such a view can originate only in a fundamental misconception of the origin, growth and function of epic poetry.

(Sukthankar 1933: lxxv)

By this description, my own conception of the *Mahābhārata* could be manifestly grotesque, to say nothing of the *Mahābhārata*’s own conception of itself as having been authored by Vyāsa. Here Sukthankar seems to bully the reader, on pain of being labelled ignorant and twisted, into sharing his particular and highly consequential opinion about ‘epic poetry’. One might imagine that Sukthankar’s aggressive rhetorical tactics would be unnecessary were he simply to explain and provide support for his opinion. He doesn’t do that, though, and so we are at liberty to suspect he can’t. Sukthankar’s tactics might imply that on the issue of ‘epic poetry’ the usual academic role of evidence-based argument could be severely attenuated, or somehow circumvented; and this matches some of my own experience also. Nonetheless, we press on.

The concept ‘epic’ has at various times been compared and contrasted with tragedy (in Aristotle’s *Poetics*; Fyfe 1960, passim; Martin 2005: 11–13), with romance (Ker 1931; cf. Morley 1911), with the novel (Bakhtin 1981: 3–40; Pollock 2006: 539–65), with pseudo-epic (Hopkins 1993 [1901]), with myth (Dumézil 1968; Hildebeitel 1990: 27–59), with praise-poetry (Martin 2005: 17), and with history (Konstan and Raaflaub 2010). The label ‘epic’ has also been applied to certain types of English literature in the colonial period (Quint 1993; Graham 1998; Tucker 2008). Pathak argues that the term’s basic meaning is coextensive with the works of Homer (cf. Nagy 2009), and that its application to certain Sanskrit texts – an application which she traces, in English-language scholarship, to Hopkins himself – is metaphorical, but that it effectively elides its own metaphorical nature, and hence acts ‘to



foreground the features of the Sanskrit works that are analogous to attributes of the Greek ones' (Pathak 2013: 46). Martin discusses various attempts to define 'epic', as a genre of textual product and/or of performance event, but it remains slippery and emerges as 'a notional instead of normative term', a 'super-genre' that is 'hugely ambitious' and 'is on the level of ideology a metonymy for culture itself' (Martin 2005: 11, 17–18). John Smith, discussing the label's application to the *Mahābhārata*, says that a genre term is 'like a bundle of typical characteristics, not all of which need apply in every case' (Smith 2009: lxxv). Smith's characterisation of genre terms makes them similar to terms that denote biological species:

The names of particular species cannot be defined by a set of traits which are severally necessary and jointly sufficient for membership without excluding too many organisms from the evolutionary process. Instead the names of particular species must be defined in terms of properties which are characteristic of the species but which are not universally distributed among its members; that is, a set of 'defining' characteristics is given, but no individual need possess all of them, only a sufficiently high percentage of the most important ones. Some traits are more characteristic of a particular taxon than others, but no one character is strictly necessary. On this view of definition, the sharp distinction between defining characteristics and additional, empirically correlated characteristics breaks down.

(D. L. Hull, quoted in Reeve 1988: 491–92)

In the case of a biological species this wooliness of definition doesn't really matter, because a species is additionally a reproductive community that operates alongside others through long time, whether or not it or any other species notices or names it as such. In comparison, a genre term has no real-world anchor beyond its own appearance in linguistic usage, and so when Todorov calls a genre term a 'codification of discursive properties' (Todorov 1976: 162), we can understand why such codification would be both necessary and temporary. A genre term is nebulous, since it is a matter of opinion what its allegedly 'defining' characteristics would be, or how 'important' each might be in comparison with the others (words taken from Hull, above). Smith is nonetheless happy for the term 'epic' to be used by commentators and comparativists as they see fit. But as Pathak's discussion of 'epic' and the *Mahābhārata* suggests, this must have implications for interpretation of – and thus potentially constrains receptivity to – the text.

Following C. S. Lewis, Fowler distinguishes primary epic from secondary epic and places the two types in a developmental sequence: 'Primary epic is heroic, festal, oral, formulaic, public in delivery, and historical in subject; secondary epic is civilized, literary, private, stylistically elevated, and "sublime"' (Fowler 1982: 160). There can be no doubt that in the above quotation Sukthankar would be speaking of the primary type of epic. Indeed, his allusion to 'true epic' may imply a distinction of this very kind; 'true epic' would perhaps contrast with Hopkins's 'pseudo-epic'. Hopkins's pseudo-epic is posited as

relatively late material, and Winternitz also uses such a chronological separation: what Winternitz calls ‘the Epic’ (the circa 100,000 verses attributed to Vyāsa) is said to comprise a core or nucleus of old heroic bardic-ballad material, to which other materials were then added by being ‘introduced into the epic’ – that is, into the epic that was already there with a small ‘e’ before it grew and became capitalised (Winternitz 1924: 344). The apparent distinction here between epic and Epic is not carried through in Winternitz’s article or in the wider secondary literature, but the idea is common that the expanding text once was and thus still is epic – an idea seemingly shared by Sukthankar (cf. Sukthankar 1936: 75–76).

We might wonder exactly where Sukthankar got his own (allegedly correct) conception of epic poetry. Perhaps during his time in Europe before the Great War he was influenced by notions of epic prevalent there. Writing in the 1820s, Hegel had ‘portrayed the “epic” as the sort of text that seeks to communicate the “national story”’ (Pollock 2006: 544). For Hegel, ‘epics’ were primary facts of national literature, each expressing the original and ongoing spirit and character of a nation and of the *volk* who constitute it. These were Romantic ideas in the style of J. G. Herder (1744–1803), and they were long-lasting and far-reaching. Texts such as the *Nibelungenlied* and the *Chanson de Roland* functioned almost as authentic emblems for particular nation-states (Bédier 1921: 200–288; Cerquiglini 1999: 52–54; Van Hulle and Leerssen 2008). Adluri and Bagchee have written on the nationalist ideology as found later in the nineteenth century in the German writings of the two Holtzmanns, uncle and nephew, on the *Mahābhārata* (Adluri and Bagchee 2014: 40–71), and have drawn particular attention to the insistence on the *Mahābhārata*’s primary orality (pp. 59–62, 145–46), which seems to be coextensive with its identification as an epic. Sukthankar would have seen and been among such discourse in Europe. Perhaps he would have heard that according to Hegel back in the 1820s, ‘The *Mahābhārata* and *Rāmāyaṇa* (which Hegel knew intimately through his association with the Sanskritist Franz Bopp in Berlin) explain to us “the entire outlook of the Indians,” not only “in its whole splendor and magnificence” but also “in its confusion, fantastic flabbiness and lack of real truth”’ (Pollock 2006: 546, quoting Knox’s translation of Hegel). Sukthankar went on to occupy a special position in terms of such discourse.<sup>16</sup>

To return to the issue of fluidity. Sukthankar seems just to *know* that the *Mahābhārata* text was initially and fluidly oral. He says that ‘both recensions [the northern and southern] are, in final analysis, *independent copies of an orally transmitted text*’; ‘it was probably written down *independently* in different epochs and under different

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<sup>16</sup> Pollock (2006: 539–65) explains that the Romantic connection between nation and epic was in later writing (he mentions Lukács and Bakhtin) overlaid or overwritten by a connection between industrial-capitalist nation and novel, where novel was contrasted with epic as the contemporary is contrasted with the past. Pollock’s central point is that such connections and contrasts are made general in view of too little relevant data. He suggests that the living *Mahābhārata* tradition has always been about moral indeterminacy in the present: ‘Indian epics ... have been constantly and earnestly relived ... The novel filled the “sympathy” gap left by the death of the European epic, a gap never experienced in South Asia’ (pp. 554–55). Pollock’s use of the word ‘epic’ to describe the *Mahābhārata* and *Rāmāyaṇa* traditions (for the latter see Pollock 1993) is apparently Hegelian, though his estimation of their texts certainly is not.

circumstances’ (pp. lxxviii, lxxix; cf. Phillips-Rodriguez 2018: 9, repeating the latter claim without reference to Sukthankar). This would explain, Sukthankar says, why ‘Hundreds of thousands of the minor readings are nothing more than mere synonyms or paraphrases, grammatically and semantically equivalent, but graphically totally unrelated’ (Sukthankar 1933: lxxviii), and so don’t seem to be scribal errors. For Sukthankar, they are differences in oral delivery. Hence he claims that the ‘classical model’ of critical editing ‘can be applied to the Mahābhārata with great limitations’; ‘The Mahābhārata problem is a problem *sui generis*. It is useless to think of reconstructing a fluid text in a literally *original* shape, on the basis of an archetype and a *stemma codicum*’ (pp. lxxvii, lxxxvi).

Yet although he remains necessarily agnostic about the original, Sukthankar nonetheless presents a *stemma* and says he reconstitutes ‘the ancestor of all extant manuscripts’ (p. ciii), and so the purport of his *sui generis* claim is unclear. Mahadevan writes as if Sukthankar’s *sui generis* claim was made because of the *Mahābhārata*’s post-archetypal tendency to accumulate interpolated passages, some of them long ones, at a rate unfamiliar from other textual traditions (Mahadevan 2018: 61); but it is not clear to me that this is the only – or even the principal – reason why the claim was made. Reeve says that ‘if it begins to look as though there had been more than one original ... a decision will be needed on what to reconstruct’ (i.e., which of the originals, Reeve 1988: 502); but when Sukthankar says he reconstitutes ‘*the* ancestor of all extant manuscripts’ (rather than just reconstituting N or S, for example; Sukthankar 1933: ciii, italics added), it appears that he had no such decision to make.

Sukthankar’s vacillation concerning the status of the reconstituted text has been noted several times before. Sutherland refers to ‘Sukthankar’s seemingly inconsistent stance’ (Sutherland 1986: 861). Bigger says that Sukthankar ‘is very ambiguous about the existence of an archetype’ (Bigger 2002: 18). Hildebeitel refers to Sukthankar’s ‘somewhat unresolved views’ (Hildebeitel 2011b: 88). Elsewhere, as one would expect, the confusion is ramified. According to Mehta, who wrote a book-length review of the critical edition, Sukthankar imagines a written archetype connected to hyparchetypes N and S only through oral transmission (Mehta 1971: 86). In her review of Mehta’s work, Sutherland notes that ‘Mehta seems to misunderstand Sukthankar’s use of the term archetype’, and says that ‘Sukthankar has not in these passages [adduced by Mehta and discussed by Sutherland] or any others posited a written archetype’ (Sutherland 1986: 861). Brockington, in contrast, says that Sukthankar ‘felt ... that all manuscripts of the *Mahābhārata* basically derive from a single written text’ (Brockington 1998: 59); and this seems also to be the view of Bronkhorst, who envisages the archetype as a descendant of the first written version (Bronkhorst 2011: 43–45, 51–52).

From one point of view, if Sukthankar’s vacillation was born out of a desire to uphold the received position *vis-à-vis* epic poetry and the *Mahābhārata* then it is extremely unfortunate, since it has facilitated the perpetuation of that received position. Most Indologists to whom I have spoken seem also just to know, as Sukthankar apparently did, that the *Mahābhārata* was originally an oral text. The only exceptions to this rule that I have knowingly come across are specialists on the *Mahābhārata*. Meanwhile, writing on textual

criticism in Indology, Witzel, a Vedic specialist, asserts that the *Mahābhārata* edition cannot truly be a critical one because of ‘the problems of oral bardic transmission’, and that ‘we will never reach a true *Mahābhārata* archetype’ (Witzel 2014: 28, 35; cf. 40–41, 55, 63; Adluri and Bagchee 2018: 323–24), and Hanneder asserts that with the *Mahābhārata* edition ‘the claim to reconstruct an “Ur-Text” was not really made’ and that ‘one would not propose a common archetype’ for the text’s different versions (Hanneder 2017: 58 n. 11, 61).

One wonders whether such claims would be made had Sukthankar said only that he was reconstituting the archetype. Perhaps they would. As Sukthankar himself notes on the first page of the prolegomena, when the *Mahābhārata* critical-edition project was first mooted by Winternitz the general opinion of European Sanskritists was that no such thing was possible (Sukthankar 1933: i), and regardless of what has happened in *Mahābhārata* studies in the interim, this opinion is still with us, as the above quotations from Witzel and Hanneder show. Whether it is based on anything much more substantial than the power of the words ‘oral’, ‘epic’, and ‘bard’, I am not yet sure.

Writing on the beginnings of Indian manuscript culture and discussing the question of orality at length, Houben and Rath give an account of two different types of pre-literate textual transmission in ancient India, the Vedic type where the text is kept stable in transmission, and the ‘epic’ type where the text is often modified (Houben and Rath 2012: 16, 21, 23, 32, 43, referring to Staal 1986: 26–27). Such an account is comparatively unobjectionable in itself, but Sukthankar seems to take the further step of hypothetically linking the latter type of transmission with the *Mahābhārata*’s early transmission. The *Mahābhārata* depicts a world where both these types of oral transmission occur, as well as written transmission, and it contains characters who are experts in oral transmission. Indeed, a character in an outer frame of the *Mahābhārata* is the storyteller (*sūta* or *sauti*) Ugraśravas son of Lomaharṣaṇa, who in the story orally narrates a huge text which is said to have been created previously by the sage Vyāsa. So we may say that the *Mahābhārata* (which was probably intended to be received aurally by most of its receivers) contains the idea of its own oral transmission. But it doesn’t contain it in a way that would encourage us to deem it a historical fact. Indeed, we need not take the text’s statements as matters of historical fact. We need not think, for example, that in ancient India there were chariots that flew, pots that gave birth, or dead children that came back to life; and in the case of other statements that don’t seem contrary to the laws of nature, we may simply remain historically agnostic. The fact that it might say something in the *Mahābhārata* is not a reason to believe it. So it is perfectly possible for there to have been a long tradition of oral epic in India before, during, and after the *Mahābhārata*’s composition (which the *Mahābhārata* would naturally comment on and imaginatively include), but for the *Mahābhārata* to have always been a written text. Hildebeitel makes the same point when he says that ‘orality in the *Mahābhārata* is thus a literary trope’ (Hildebeitel 2005: 89; cf. Hildebeitel 2000). But Sukthankar writes as if we are obliged to take the *Mahābhārata*’s presentation of Vyāsa’s, Vaiśampāyana’s, and Ugraśravas’s successive recitations as a quasi-historical account of the *Mahābhārata*’s early transmission (Sukthankar 1933: lxxv; cf. Sukthankar 1936: 70–73). By the same kind of argument, one might imagine that any

written text containing direct speech (So-and-so said ‘Such-and-such’) has an actual oral prehistory.

So are there good reasons to assume the *Mahābhārata*’s primary orality? Sukthankar tries to give a credible reason when he says that ‘Hundreds of thousands of the minor readings are nothing more than mere synonyms or paraphrases, grammatically and semantically equivalent, but graphically totally unrelated’ (p. lxxviii). These apparently oral variants are the only evidence Sukthankar offers to support his idea of the *Mahābhārata*’s *sui generis* oral fluidity, but instead they rather suggest that manuscripts were not always produced on the basis of the scribe’s inspection of an existing manuscript (which brings its own range of ‘scribal errors’, as per Vinaver 1939), but were also produced by the scribe taking down the dictation of an assistant (on this method, cf. e.g. Dagenais 1994: 17; Parker 2008: 154–57; Esposito 2012: 88–89; Sobieroj 2014: 80; De Simini 2016: 103). In the transmission – from the manuscript to the eyes, to the brain, to the tongue, then through the air to some ears, to another brain, and to its fingers – specific types of variants and paraphrases appear (cf. Davis 1998: 101–02). But this has no specific implications for the beginning of the manuscript tradition (cf. Mehta 1971: 87–99, with similar conclusion).

A different suggestion of primary orality is seen in the *Mahābhārata*’s formulaic style, as Brockington has argued (Brockington 1998: 103–16), assisted by famous work on formulaic diction and variant usage in living oral traditions (Lord 1960; Parry 1971). This kind of work is easily co-opted to bolster the old preoccupation in European *Mahābhārata* studies with trying to find the original oral epic *ṣatriya* parts of the text (perhaps based on historical events; see de Jong 1975: 34–37; Witzel 2005: 22–24, esp. n. 4; Fitzgerald 2010: 106–07) – a preoccupation described with some disdain by Adluri and Bagchee (see above, p. 33). But formulaic style is characteristic of any number of literary works for which no oral prehistory is postulated; indeed, formulaic style seems to be a standard trope of the (oral or written) *representation* of orality at least as often as it is a feature of orality itself. Since the *Mahābhārata* represents oral events throughout, it would be expected to manifest an allegedly oral style, whether or not it were an oral text. In the context of Purāṇic studies Bakker points out that

[F]ormularity does not automatically imply oral transmission ... The many different versions of what basically seems to be one and the same epic or Puranic text may have its origin in recomposition during the process of literary transmission rather than in a living oral tradition ... the conclusion that puranic literature as known to us must be a product of oral tradition seems to be based on a fallacy.

(Bakker 1989: 331–32)

Nonetheless, the idea of primary orality is tenacious, and can sometimes seem more akin to an article of faith than a hypothesis to explain a particular state of affairs.

In trying to understand this, we must also acknowledge the influence of Homeric studies. In 1795, long before Sukthankar was born, Wolf had discoursed at length on how

the *Iliad* and the *Odyssey* had subsisted in orally transmitted form before being transformed from loose cycles of tales into written works with a fixed order and structure (Wolf 1985 [1795]: 71–148; cf. now Nagy 2001, with five stations on a historical line from most fluid to most rigid Homeric text). Indeed, the quotation from Sukthankar above (p. 31), in which he mentions Goethe, Milton, Kālidāsa, and Bhavabhūti, recalls Wolf when Wolf says that

[T]he method of those who read Homer and Callimachus and Virgil and Nonnus and Milton in one and the same spirit, and do not strive to weigh in reading and work out what each author's age allows, has not yet entirely been done away with.

(Wolf 1985 [1795]: 72)

Sukthankar effectively applies Wolf's conclusions to the *Mahābhārata*. Lévi discerned this in his 1934 review of the prolegomena:

At root, it is the problem of the Homeric epic that reappears in a new form in connection with the Mahā Bhārata, and Mr Sukthankar, formed at the school of pandits and at the school of German philology, is caught between the indigenous tradition and Wolf.

(Lévi 1934: 282)<sup>17</sup>

After quoting Sukthankar's statements on textual fluidity as quoted on p. 29 above, Lévi says that 'One recognises, from this vocabulary, where Mr Sukthankar's doctrines come from' (Lévi 1934: 283).<sup>18</sup> But on what basis does Sukthankar apply Wolf's conclusions to the *Mahābhārata*? Wolf's conclusions about the originally oral nature of the Homeric material are supported by ancient testimony reaching back as far as Josephus and Cicero (Wolf 1985: 94, 137; Grafton, Most, and Zetzel 1985: 5), but there is nothing comparable in the case of the *Mahābhārata*; there is simply a generic or stylistic resemblance, and it is in the first place a resemblance with *written* Homeric texts that have been reworked in accordance with ancient conventions for the *representation* of orality. Wolf had suggested that comparison with the passage from orality to literacy in other cultures could help to support his theories about the *Iliad* and the *Odyssey* (Wolf 1985: 145–46), but here it might seem that his theories are being used as the basis for *deciding* what happened elsewhere. And it seems that Wolf's theories were to some extent brought forth by the *zeitgeist*:

As the eighteenth century wore on ... literary tastes turned more and more to the primitive and the exotic. The direct, vivid, popular songs of Celts and

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<sup>17</sup> 'Au fond, c'est le problème de l'épopée homérique qui reparait sous une nouvelle forme à propos du Mahā Bhārata, et M. Sukthankar, formé à l'école des pandits et à l'école de la philologie allemande, est tiraillé entre la tradition indigène et Wolf.'

<sup>18</sup> 'On reconnaît, à ce vocabulaire, d'où sortent les doctrines de M. Sukthankar.'

Bedouins were in favor, even if they had to be forged to meet the desires of the public. The only way to keep Homer interesting and to make classical studies look useful in their own right was to claim that up-to-date, comparative research established Homer in the enviable position of the Greek Ossian. Blackwell and Gravina, Wood and Merian labored to knock Homer off his Ionic pedestal, to strip him of his austere classical robes, and to deck him out with the rough staff and furry cloak appropriate to a storyteller at a tribal campfire.

(Grafton, Most, and Zetzel 1985: 11)

Something similar may have happened with Sukthankar and Vyāsa.

To return finally to the status of the reconstituted text, it seems to me that the arguments put forward to support Lévi's and Sukthankar's reservations in this regard are not good enough. So there is no need for us to share Sukthankar's conflicted attitude towards the reconstituted text. We might as well view it – as one part of Sukthankar does – as the stable archetype of the manuscript versions surveyed. That is what the method is designed to reconstruct: 'The postulate of the parent archetype is the sheet-anchor of the science of Text-Criticism' (Mehta 1971: 83); 'to make a critical edition possible at all, one has to presuppose an archetype' (Bigger 2002: 19). And we might as well imagine that archetype as written, because that is normally the case when the method is used, because there is no good reason not to, and because written text is our paradigm of fixed text. Lévi's speculations about the top of the manuscript tradition facilitated his rejection of this view in this instance, but it was the kind of view that it was in any case fashionable for French scholars to reject. Others did not: for example Franklin Edgerton, in his review of the first fascicule of the critical edition, wrote that 'We seem justified in hoping that Sukthankar's methods will give us in time a text which can without much inaccuracy be considered an ancestor of all extant manuscripts' (Edgerton 1928: 188). Edgerton, an American, went on to edit the *Mahābhārata's Sabhāparvan* as part of the Poona team, and he wrote in his introduction to that volume that

[W]ith due allowance for many minor uncertainties, and for errors in editing, [the reconstituted text] is a text – in this case of the *Sabhāparvan* – which once existed, and from which all MSS. of the work known to us are directly descended ... It is not an indefinite 'literature' that we are dealing with, but a definite literary composition.

(Edgerton 1944: xxxvi, xxxvii)

This is effectively the view Sukthankar called 'manifestly grotesque'; but I consider it an honour and a scholarly virtue to be as ignorant as Edgerton was on the topic of epic poetry. We shall have more to say about 'epic' in the third part of this report, when considering the register of the Cardiff *Harivaṃśa* translation.

### Biardeau and Bedekar

In 1968 Biardeau published an article in the Indian journal *Purāṇa* in which she revived her teacher Lévi's critique of the Poona critical edition. She asserted the initial and parallel orality of the text, and the irrelevance of Sukthankar's version in comparison with others – principally Nīlakaṇṭha's vulgate, but also the regional recensions (which she said should be published severally, p. 123). She also questioned the motives of the Poona editors:

In using a seemingly Western scientific method, which was not meant for that kind of literature, they had in mind a purpose which could only superficially be stated in scientific terms; in fact it remained purely religious ... What was to be reached was not so much the original text as the most authoritative one, and it was hoped that the oldest possible version would come closer to the truthful version, having less intermediary transmitting agents to alter it.

(Biardeau 1968: 119)

Van der Veer's argument – that the quasi-religious Indian nationalist project empowered the critical-edition project – is similar (van der Veer 1999; cf. Lothspeich 2009: 73–75; Sukthankar et al. 1937: 8), and can also relate to the democratic plenitude of the critical apparatus. In van der Veer's perspective, the newly engineered singularity of the *Mahābhārata* is overlaid onto the authentic singularity of the independent nation of India, and serves as a common basis against which regional differences can be understood. Biardeau also seems to be saying something more than this, although it is not clear whether she is commenting on the specific issue of reconstituting an ancient version of a revered Hindu text, or more generally on the tendency for older versions of texts to be seen as more authoritative (which was and remains a standard critique of the stemmatic enterprise, as seen above).

Bedekar responded to Biardeau's article, in the same journal, by restating the 'principles of Mahābhārata textual criticism' (Bedekar 1969). Bedekar notes that Sukthankar (who died in 1943) had anticipated most of Biardeau's objections, and that producing good editions of the various regional recensions would require all the work of a single critical edition (cf. Sukthankar 1933: lxxxii–lxxxiii). Biardeau's suggestion that editors should rely on versions known by local pandits is dismissed as unworkable, because there are thousands of pandits, and thus thousands of preferred versions.

Bedekar rejects Biardeau's objection that the critical method is Western and thus unfit for Indian texts. If a text is transmitted in manuscript form, the method applies or does not apply irrespective of geography: 'textual criticism as a science is universal and it is crass obscurantism to say that it is not meant for the Indian Epics and Purāṇas' (Bedekar 1969: 220; cf. Wujastyk 2014: 176, 'The canons of textual criticism ... are equally applicable



to the Indian case').<sup>19</sup> This recurring objection that the method is Western – since revived by Sutherland (1992: 83–86), Sharma (2008), and Doniger (2009: 18) – may be connected with the colonial ideological aura of the term 'Western' (which, since the world is round, has no obvious referential meaning), and with the grouping of the *Mahābhārata* and *Rāmāyaṇa* with the Purāṇas (cf. Hildebeitel 2011b: 88–89). Bakker suggests that restoring an archetype is unrealistic for most Purāṇas, but that 'An exception may be made for some of the early Purāṇas like e.g. the *Viṣṇupurāṇa*' (Bakker 1989: 336 n. 33). In any case, if the method doesn't apply to certain Purāṇas, this wouldn't be because it is a Western method.

Bedekar stresses, as had Winternitz (1934: 159) and Sukthankar (as quoted above, p. 9), that the Poona edition was 'making available in one edition to all readers a panorama of different versions, still leaving them free choice to enjoy their preference for their own particular version which they may be considering as authoritative' (Bedekar 1969: 216). But this doesn't really answer Biardeau's objection to the obscuring of regional versions. The editor's spacial power-plays – placing some variants in the reconstituted text, some in the apparatus – are striking (Warren 2013: 125), and it is hard to use the critical edition to read a regional version, least of all on its own terms (cf. Phillips-Rodriguez 2007: 171): one is always led to think in terms of what a particular branch of the tradition has done with and to an earlier text (e.g. Hildebeitel 2011, 2018). Cerquiglini shows how variants are obscured by the atomistic manner in which the critical apparatus supplies them (Cerquiglini 1999: 73–75), and insists that 'variance is not to be grasped through the word; this must be done, rather, at least at the level of the sentence if not, indeed, at the very heart of the complete utterance, the segment of discourse' (p. 78). With the Purāṇas in mind, and in an attempt to move away from the prioritisation of the oldest reconstructable text, Bonazzoli has devised and demonstrated a new way of presenting differential manuscript data, such that 'The whole tradition is seen, as it were, all together, in a coup-d'oeil': 'The new critical edition makes it possible to easily pick out a single desired version' (Bonazzoli 1985: 382). But it is not at all clear that Bonazzoli's method could work effectively for a manuscript tradition as large and rich as that of the *Mahābhārata*.

Biardeau responded to Bedekar's *Purāṇa* article with a letter to the journal's editor, from which it seems that her position – 'Actually I do not believe in critical editions of epic and purāṇic texts' (Biardeau 1970: 181) – is based primarily within the European debate: 'Biardeau's debate was ... with German philology itself' (Hildebeitel 2011b: 90). This letter was followed by an article in which Biardeau began with something of a retraction: 'I must admit that I started from the wrong end when I criticized the principles governing the so-called critical editions of those texts without mentioning the use that can be made of the different versions of the story' (Biardeau 1970b: 287). The article then argues that in Nīlakaṇṭha's vulgate the two versions of the Kārtavīrya story (in the *Āraṇyakaparvan* and the *Śāntiparvan*) are complementary and both narratively complete, but that in the

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<sup>19</sup> The question of whether the method devised for 'classical' European texts was applicable to medieval European ones had already been raised, and was answered in the affirmative by Gaston Paris (Reeve 1988: 455).

reconstituted text both versions become more obscure: ‘They have lost their intrinsic intelligibility and the mutual relationship that was so clear in the Vulgate has disappeared’ (p. 301). But even if Biardeau points to ways in which the vulgate version might be deemed preferable, that judgement is beside the point of the critical edition, whose guide is general attestation. Moreover, by thinking in terms of the critical editors removing material (and intelligibility) from the vulgate, she loses sight of the intermediate editors who interpolated that material. They did so in order to improve the text (from their point of view), and so one would naturally expect that undoing what they did can seem to impoverish it. Biardeau’s objection has again been anticipated by Sukthankar: ‘the constituted text is by no means smooth’ (Sukthankar 1933: ciii).

The last word on Biardeau’s *Purāṇa* contributions goes to van Buitenen. He no doubt caricatures her position to some degree, but he provides an alternative analysis of the Kārtavīrya stories (including the *Ādiparvan*’s Aurva story as a third token of the type) and spiritedly explains that chronological distinction is not a notion that he is prepared to renounce at her request (van Buitenen 1978: 142–54). In retrospect is it notable that while Biardeau seemed to deny the relevance of chronological distinctions altogether, van Buitenen clearly felt entitled to make them wherever he liked, be that between the reconstituted text and the apparatus materials or between different parts of the reconstituted text, method no object. To my mind, van Buitenen’s and Biardeau’s positions are equally flawed.

#### Non-Application of Higher Criticism

Sukthankar and his team might have sought to make a better text in various ways, had they not been so wedded to the manuscript data. Biardeau thinks their text is too short, but others may think it too long. Mehendale lists contradictions or oddities in the reconstituted text that could potentially be resolved by the application of higher criticism, thus yielding a smoother narrative (Mehendale 2009: 15–21).

‘Higher criticism’ is a term that originated within Biblical studies. It is defined by Katre as ‘separation of the sources utilized by the author’ (Katre 1941: 31), but it can be applied more generally to any attempt to reconstruct a pre-archetypal version of a text. While Mehendale was still young, Ruben had criticised the Poona edition for not attempting this (Ruben 1930: 241; Sukthankar 1930: 259–60). Ruben was presumably one of those of whom Winternitz spoke when he said that

[M]any a Western scholar will be disappointed to find any amount of passages in the constituted text which he was sure could not be genuine and original parts of the Epic. I confess that I myself had hoped that the critical edition would confirm the spuriousness of such passages ... They may be interpolations, nevertheless, but then they must have been added at some earlier period to which our manuscript tradition does not reach back.

(Winternitz 1934: 173–74)

This comment, from one of the project's greatest champions, is telling. It opens the heart of the analytic versus synthetic debate (Brodbeck 2013b: 135–44). The edition seems not to have delivered the intended text. Perhaps it is partly as a result of this that oral epic tradition is so often invoked as a possible erstwhile repository of that intended text. But no one has devised an acceptable method for identifying such 'interpolations' as Winternitz invokes here. In comparison to such methods as have been proposed (see e.g. Ježić 1986; Smith 1992; Brockington 2001: 67 n. 1), Sukthankar's methods of text-constitution seem unproblematic.

Updating the famously (and unnecessarily) pessimistic last sentence in Paul Maas's handbook of textual criticism – 'No specific has yet been discovered against contamination', Maas 1958: 49 – we might say that no specific has yet been discovered against universal non-erroneous attestation. Hence I gladly use the word interpolation for verses or passages identified as such by the Poona method and placed in the apparatus rather than in the constituted text, but when scholars use the word in Winternitz's sense I would put it in inverted commas. The basic claim made about 'interpolations' in inverted commas (i.e., verses in the reconstituted text that some scholars would like to imagine a *Mahābhārata* without) is woefully speculative, because no scholar can argue for what such a possibility would imply – that is, a pre-archetypal line of transmission that persisted for a significant growth-period without branching, or a significant growth-and-branching period eventually traversed by just one smooth snedded branch. The section of Sukthankar's *stemma* in between 'Vyāsa's Bhārata' and the 'Ur-Mahābhārata' archetype might seem to allow for either of these options (see again Figure 2, p. 12 above), but there is no evidence to suggest either of them, and thus no basis even for differentiating them as options. Starting as we do from the end-point of the tradition's development, the hypothetical gap between 'Vyāsa's Bhārata' and the archetype is justified only insofar as the latter, as reconstructed by the method, contains errors that we don't think the *Mahābhārata*'s first writer could possibly have made. Conceptually (and against Austin 2011), such a gap is a world away from what the comparison of surviving manuscript versions reveals (and what everything we know about manuscript traditions would in any case suggest) about the developmental branching of the tradition *subsequent* to the archetype.

When, in the case of the *Mānava Dharmaśāstra*, Olivelle, despite the bane of contamination, arrived at and published a putatively oldest fully attested text, but simultaneously argued higher-critically on structural grounds that some parts of it, which he called excurses, were later additions (see his 'editor's outline', Olivelle 2005: 77–86), Fitzgerald, in his review of the edition, was quite right to spell out what this claim implies about the history of the written transmission. In this respect Fitzgerald's important words, as quoted immediately below, can be compared with those of Adluri and Bagchee when the latter scholars argue against Bigger's idea of a 'normative redaction' of the *Mahābhārata* (Adluri and Bagchee 2018: 45–117).

Olivelle believes these excurses were composed at various times and places subsequent to the completion and dissemination of the putative autograph-

archetype of the *Mānavadharmasāstra* and were then absorbed over time into all copies of some post-archetypal authoritative text of the *śāstra* by way of what must be imagined as a very extensive and homogenizing process of manuscript circulation. This process would consist of any and all corrections and additions that were made to authoritative copies of the text as isolated ‘improvements’ or ‘completions’ becoming more or less knowingly sanctioned or favored by the elite members of the expert *dharmaśāstra* tradition. These improvements would then have made their way into all new normative copies of the text, eventually replacing, in the queues of manuscripts to be copied, all earlier copies that lacked them. Such a process continued, Olivelle believes, until a normative text of the *śāstra* was ‘fixed’ ...

... Such a homogenizing process of circulation is the only way, in general, to view some fragment of a written text that is found in all extant witnesses to be, in fact, a post-archetypal addition to the text ...

... [T]he alternative to Olivelle’s argument about the excurses is that these twenty-six passages obscuring the ‘exquisite structure’ of the text were part of the original archetype of the *Manu Śāstra* ... I think this alternative is more likely.

(Fitzgerald 2014: 501–02)

In the sentences I have elided, Fitzgerald makes attempts to support this judgement with reference to his own imaginary scenario of how the archetype may have come about in this tradition. But that scenario is no less imaginary than the one to which it is opposed, which is the one Olivelle came up with to explain exactly the same textual circumstances. And those two scenarios are thus equally as imaginary as any other as-yet-unstated scenario that might allegedly explain some other exquisite scheme differentiating original material from later excurses. Hence as far as that kind of support is concerned, neither Fitzgerald’s judgement nor any Olivelle-type judgement has a leg to stand on. And so, for that very reason, in this instance Fitzgerald’s judgement is theoretically correct, because since we have no textual grounds for going more than a couple of hair’s-breadths beyond the archetype, we must take it for what it is; all else is noise.

The criticisms just discussed are general criticisms of the *Mahābhārata* critical-edition project. We turn now to specific criticisms of its method.

### Circularity

As we saw above, the modern response to the charge of circularity is to affirm that the witnesses are classified and the *stemma* constructed on the basis of manifest monogenetic errors. Adluri and Bagchee assert that ‘The *Mahābhārata* editors used the *common-error* method’ (Adluri and Bagchee 2018: 47), but nonetheless it would have been reassuring had Sukthankar and his colleagues detailed the specific errors whose distributions suggested the groupings and *stemmas* presented in the introductions to the various volumes of the

Poona edition. Such details are lacking. To my knowledge the only instance of a specific obvious error being mentioned in connection with manuscript classification is in Sukthankar's article 'Dr Ruben on the Critical Edition of the Mahābhārata':

That the three new Malayālam MSS. M6. 7. 8 again go back to the same original follows not merely from the numerous readings these MSS. have in common, but conspicuously from one particular mistake where they *repeat* inconsequentially, at the same point, a fragment of a stanza (1. 85. 25):  
*pūjayantīha loke nāsādhavaḥ.*

(Sukthankar 1930: 265)

In the prolegomena, Sukthankar mentions just underneath his *stemma* (see again Figure 2) that 'y ... contains a considerable number of secondary additions (including repetitions), as also a very large number of verbal alterations *and corruptions*', and that 'σ is the lost archetype of T G, containing a large number of *corruptions and* secondary additions, from which M is free' (Sukthankar 1933: xxx, italics added). In the first instance the juxtaposition of 'corruptions' with verbal alterations makes corruptions seem to be just manifest errors, but in the second instance the word 'corruptions' seems to do duty for the manifest *and* latent errors specific to T and G. In any case, the corruptions are not listed. In the pages that follow thereafter, Sukthankar gives a list of 'readings ... which Ś<sub>1</sub> shares with the K version, Ś<sub>1</sub> and K standing together against all other manuscripts' (Sukthankar 1933: l; these are the readings of v), and also a list of readings in which Ś<sub>1</sub> and/or the K manuscripts agree with the southern-recension manuscripts (p. liv), and so on. There is thus certainly a wealth of what Trovato would call 'confirmatory readings' (confirmatory of the *stemma's* correctness; Trovato 2017: 116–17). But there is no explicit evidence that the *stemma* was not built primarily on the basis of such distributions.

Perhaps in the introductions to their editions Sukthankar and his colleagues did not think it would be of interest to give lists of very obvious errors and their distributions.

Nonetheless, I feel some doubt about the hard distinction between, and the relative ranking of, indicative errors and confirmatory readings. And although Adluri and Bagchee, as mentioned above, identify Sukthankar's method as the common-error method, on several other occasions they write as if Sukthankar proceeded as he generally suggests he did, that is, on the basis of the distributions of different readings *in general* (Adluri and Bagchee 2018: 189–91). For this impression see also Adluri and Bagchee p. 195, approving the statement in Sukthankar's prolegomena that 'the textual peculiarities ... are, in final analysis, the real basis of our classification' (Sukthankar 1933: lxxv), and p. 214 whereby different versions of the text 'demonstrate their true filiation in terms of their minor readings', that is, in terms of what Sukthankar called agreement in 'petty verbal details' (Sukthankar 1933: liv), which bespeak common inheritance and in some cases – the most important ones as far as the eventually reconstituted text is concerned – common inheritance from the archetype.

But in general in a large manuscript tradition, what if plenty of good indicative errors were to indicate groupings and a *stemma* that the mass of other variant readings don't seem to confirm? And what if the variant readings in general were to seem to suggest or optatively confirm a scenario for which no clear-manifest-error patterns are discernable? It is difficult to imagine how the first case could arise, and in the second case one might imagine that the indicative errors have simply been corrected by scribes. The inheriting of a manifest error is exactly the same process as the inheriting of any of a whole host of readings that differ distinctively from those in parallel branches of the tradition, and whose differential diffusion by iterated acts of inheritance cumulatively bespeaks the different branches laid out as the *stemma*. In the process of initial manuscript survey, the more manifest errors would certainly be a natural way for the critical editor to get footholds or armholds into the tree, hopefully from as many different entry-points as possible. But their identification as manifest errors is a feature not of how the tree grows, but of the fact that someone is trying to climb into it.

Recall that the strict definitions of exactly what kind of variants the critical editor may most properly use to build up the *stemma* were made in order to defend the stemmatic method against the charge of circularity (p. 25 above). We might now imagine that the charge of circularity could be refuted somewhat by pointing to the sheer quantitative difference between, on the one hand, any single specific variant that the stemmatic method, at some stage of its process upon the witness material, allows the editor to isolate upon one branch rather than another, and on the other hand the sum total of all the other variants that it likewise at some stage allows the editor to isolate upon one branch rather than another. In the case of a *parvan* of the *Mahābhārata*, as per the examples given by Sukthankar to explain why he has classified his manuscripts into these stemmatic families in particular (Sukthankar 1933: 1 for the hyparchetype v, etc.; see p. 44 above), the difference of quantity between what is in one hand and what is in the other is overwhelming, and the charge of circularity might be ameliorated if each specific act of stemmatic discernment between variants can be justified without reference to itself, but only (only!) with reference to the coincidental, separate, and massive set of complementary other such acts.

To the extent that the distinction between manifest and latent errors is overlooked, other aspects of the general charge of circularity come to the fore. The following several subsections discuss other aspects of that charge.

### Classification of Manuscripts

The general charge that the stemmatic method is circular is made because the reconstructed text seems to be overdetermined by the initial categorisation and grouping of manuscripts. As noted earlier, *Mahābhārata* manuscripts were initially grouped by script, on the assumption that manuscripts written in regional scripts contain distinct regional versions. Sukthankar's explanation of this, which occurs immediately underneath the subtitle 'Classification of Manuscripts', is worth quoting at length:

The manuscript material is divided naturally into recensions by the scripts in which they are written. Corresponding to the two main types of Indian scripts, Northern and Southern, we get two main recensions of the epic. Each of these recensions is again divided into a number of sub-recensions, which I have called ‘versions’, corresponding to the different provincial scripts in which these texts are written. This *principium divisionis* is not as arbitrary as it might at first sight appear. The superficial difference of scripts corresponds, as a matter of fact, to deep underlying textual differences. It is common experience in India that when we have a work handed down in different versions, the script is invariably characteristic of the version. The reason for this concomitance between script and version appears to be that the scribes, being as a rule not conversant with any script other than that of their own particular province, could copy only manuscripts written in their special provincial scripts ...

(Sukthankar 1933: vii)

Before Sukthankar wrote those words, the 1919 prospectus for the Poona edition had announced that ‘a geographical and scriptal principle of classification’ would be applied to the manuscript materials, as suggested by Dr Bhandarkar himself, and that several separate regional versions would be reconstituted as intermediate steps on the way to reconstitution of the text as a whole (Bhandarkar et al. 1919: 28–29). In the context of a country uniting towards independence, the idea that various regional versions shared a common central inheritance would have had an auspicious political resonance. And before that, an initial scriptal classification of *Mahābhārata* manuscripts had been suggested by Lüders and applied in his preliminary *Druckprobe* (Lüders 1901: 3–4; Lüders 1908). The point here is that notwithstanding Sukthankar’s retrospective comments, the initial classification of each *Mahābhārata* manuscript was *not* done on the basis of the text contained within it.

As it turned out, the assumption that manuscripts written in different scripts contain distinct versions was subsequently denied for some of the manuscripts.

The Devanāgarī manuscripts were split into two subcategories, K and D. The K manuscripts (K for Kashmiri-type; K is not a script) were those identified as copies of Śāradā originals; these manuscripts shared many features with the Ś<sub>1</sub> manuscript, and were classified accordingly (Sukthankar 1933: xlix; Dunham 1991: 4–6; see again Figure 2). The other Devanāgarī manuscripts (subcategory D) were found often to be contaminated: ‘A Devanāgarī manuscript of the Mahābhārata may, in fact, contain practically any version or combination of versions’ (Sukthankar 1933: lxii; cf. Hanneder 2017: 53–54). This was also true of the Telugu ones (Sukthankar 1933: lxxii). So, for text-reconstructive purposes, ‘the Devanāgarī manuscripts, which are by far the most numerous of Mahābhārata manuscripts, are, at the same time, the *least important* of them, with the possible exception of those of the adjoining version, Telugu’ (p. lxiii).

During his manuscript survey for the *Ādiparvan*, Sukthankar identified several Śāradā manuscripts from the library of the Raghunath Temple (in Jammu) as containing

Nīlakaṇṭha's text (Dn), and declined to select them for use in the edition, relying instead on just one manuscript, Ś1, to represent the Śāradā version (Sukthankar 1933: v n. 1). Edgerton, during his manuscript survey for the *Sabhāparvan*, identified three of his four Telugu manuscripts as containing a north-eastern text, and declined to select those three for use in the edition, relying instead for his Telugu version on just one manuscript which he said was 'on the whole a normal Southern MS., agreeing with G and M, especially closely with G' (Edgerton 1944: xxi). Edgerton also came across a Nepālī manuscript containing the Maithilī version (V2) and two Devanāgarī manuscripts containing the Grantha version (DG1 and DG2), but he declined to select them for use in the edition, thus relying on just one manuscript to represent the Maithilī version (pp. xviii, xxii). Raghu Vira, during his manuscript survey for the *Virāṭaparvan*, identified two of his three Śāradā manuscripts – in the Raghunath Temple library – as containing 'recent copies of Nīlakaṇṭha's text' (Dn), and declined to select those two for use in the edition, relying instead for his Śāradā version on just one manuscript, Ś1 (Raghu Vira 1936: ix–x). De, during his manuscript survey for the *Udyogaparvan*, identified two Devanāgarī manuscripts as containing a G text, and a further Devanāgarī manuscript as containing an S text, and declined to select them for use in the edition (De 1940: xii, manuscript nos 5–7). Belvalkar, during his manuscript survey for the *Bhīṣmaparvan*, identified one of his Bengali manuscripts as containing a K-version text and classified and selected it accordingly, 'the first time that a MS. written in Bengali characters has been classified with K, which normally designates Devanāgarī copies made from a Śāradā original' (Belvalkar 1947: xxix). Belvalkar also identified one of his Devanāgarī manuscripts as containing a K-version text in its first part and an S text in its second part, and he sometimes mentioned the K part in the notes to the edition (p. lv). Finally Vaidya, during his manuscript survey for the *Harivaṃśa*, identified his only Nandināgarī manuscript as containing a T-version text and declined to select it for use in the edition (Vaidya 1969: x). He also observed that one of his three Śāradā manuscripts was 'incomplete, having neither beginning nor end, and represents nearly one-third of the bulk of the original', and that one contained a Dn text, and he thus constituted his Ś on the basis of just one manuscript, Ś1 (p. xvii).

All these examples of documented departure from the principle that script maps version are nicely collected and tabulated by Adluri and Bagchee (2018: 205–06, Table 5). There may have been other similar departures that were not documented.

We have seen that in principle, manuscript versions are to be classified by their manifest and indicative errors, and that as a first step the Poona editors classified them by script. Does the fact that the general classification by script was discovered to be erroneous and was thus modified *in some cases* mean that the classification of the basis of script was *totally and generally* overtaken by a classification on the basis of actual textual features? Adluri and Bagchee think so. See Adluri and Bagchee 2018: 182, 'The Mahābhārata manuscripts were classified not by their script, but by their text ... If the editors had merely looked to the script, they could neither have drawn up a stemma nor reconstructed the reading of the archetype' (continuous italics removed); see also p. 185, 'the true basis for the classification ... is the text itself (as it must be, if we are to reconstruct the archetype)',



and p. 189, ‘there is no justification for thinking that script was the criterion of classification ... the manuscripts themselves, or, rather, their contents will tell [the editor] whether they belong together’. Adluri and Bagchee’s view is supported by Sukthankar’s statement that ‘textual peculiarities ... are, in final analysis, the real basis of our classification’ (Sukthankar 1933: lxxv). But how well supported is this statement of Sukthankar’s? If the initial classification was by script, one would imagine that such classification was only suspended when it had to be. And the logical problem here is that if a general assumption has to be suspended in specific cases, then one ought to investigate the possibility of doing without the general assumption, since it may be distorting one’s operations even in cases where one does not suspend it.

The contamination evident in the Devanāgarī and Telugu manuscripts implies that the assumption that different scripts house different versions works best in peripheral regions. But is it ever a good assumption? Phillips-Rodriguez emphasises that the evolution of Indian scripts and the evolution of *Mahābhārata* versions are ‘two independent speciation processes’ (Phillips-Rodriguez 2018). She provides an overview of the development of Indian scripts (from Brahmī as the common source, pp. 4–5), and on this basis she observes that ‘the paleographical isolation that is attributed to Mbh manuscripts took place relatively recently in the process of textual transmission’ (p. 9). She thus argues that ‘It may have been only after regional scripts started being clearly differentiated that local variants started to coagulate inside the text, giving the impression of local versions’ (ibid.); but the *Mahābhārata* text had been developing long before that. Nonetheless, she does point out that ‘the first two paleographical branching events (the division between northern and southern scripts, and the early branching out of the Śāradā from the north-eastern scripts) are old enough to have affected the transmission of the text in a radical way’ (p. 10), which seems to support Sukthankar’s valuation of the Śāradā version (now found in two different scripts). Elsewhere, Phillips-Rodriguez presents phylograms and cladograms that group manuscripts into multi-script families, and argues on the basis of them that ‘the correlation between script and version appears to be too weak to be supportable’ (Phillips-Rodriguez 2005: 188). Similar arguments have been made on the basis of the data in specific manuscripts (Grünendahl 1993; with respect to the *Rāmāyaṇa* critical edition, which is in many ways comparable, cf. Brockington 2010: 18).

So one wonders: had they all been in the same script, could or would the manuscripts have been grouped, on the basis just of the textual data they contain, in the way the critical editors actually grouped them? Again, Adluri and Bagchee think so: ‘unless an editor had evidence to the contrary, he would treat all manuscripts as unique exemplars in his stemma, entering a unique siglum for them’; ‘we could convert all our witnesses to a standard script and still undertake the reconstruction’ (Adluri and Bagchee 2018: 196, 291 n. 182). But what if the *Mahābhārata* tradition were like that of the *Viṣṇudharma* critically edited by Grünendahl (Grünendahl 1983)? Adluri and Bagchee observe that with this text ‘[Grünendahl’s] assignment of manuscripts to the three groups evidently follows their script, for he notes that the manuscripts are so conflated that it is impossible to distinguish them otherwise’ (Adluri and Bagchee 2018: 289 n. 167). In this respect perhaps it is just the

quantity of manuscripts (and consequently the quantity of less conflated ones) that differentiates the *Mahābhārata* tradition from that of the *Viṣṇudharma*.

Differentiation by script seems to avert the general charge of circularity by providing an external basis for classification, but this is only helpful if that classification also accurately maps the contents of the manuscripts. However, there are the exceptions detailed above, and in many cases ‘It is ... a question fundamentally of more or less, when we undertake to decide which “recension” a particular manuscript belongs to. And having once decided, we must be prepared for all sorts of qualifications of our answer’ (Edgerton 1944: xxxiii; cf. Dunham 1991: 12). And yet the stemmatic restoration of the archetype depends upon the accurate genealogical grouping of the manuscripts.

On this issue I keep an open mind. It may be that despite the initial scriptal classification, the critical editors of the *Mahābhārata* constituted their versions in such a way as to construct their *stemmas* as accurately as possible and reconstitute the archetype as accurately as possible, given the manuscript material available. For this happy circumstance to be realised, since different manuscripts were in fact in different scripts it is not in principle necessary for the editors to have been able to do those things had all the manuscripts been in the same script (as per the claim of Adluri and Bagchee 2018: 291 n. 182, quoted above). The fact of different scripts could have enabled the discovery of the true *stemma* in a way that might have been impossible had there been just one script, and at the same time, in the process of editorial analysis, the initial scriptal classification could have been entirely superseded by purely textual considerations in every case.

Nonetheless, it is natural for criticism of the edition to develop around the scriptal criterion that is not really a criterion. In his 1993 article, Grünendahl, following Ruben (1930: 242) and apparently misunderstanding the full details of Sukthankar’s procedure (in which the scriptal classification was merely a first step), criticised Sukthankar for using the scriptal criterion. Adluri and Bagchee have exposed Grünendahl’s misunderstanding of Sukthankar, and have traced its influence on subsequent research (Adluri and Bagchee 2018: 269–73). They detail how, even though Sukthankar had voiced, addressed, and transcended all such concerns about the *Mahābhārata* edition many years before, Witzel has ‘commended’ Grünendahl for ‘going beyond the facile division into various script traditions’ (Witzel 2014: 34 n. 136; cf. p. 26 n. 88); and von Simson has written that ‘the criterion of script is by no means reliable’, citing Grünendahl for support (von Simson 2011: 688, trans. Adluri and Bagchee); and Slaje has written ‘I agree with Grünendahl’s (1993) criticism of the “Schriftartenprämissen”’ (Slaje 1996: 210 n. 7); and Bronkhorst has written that ‘Grünendahl (1993) has criticized Sukthankar’s editorial principles, most notably his attempt to associate a number of manuscripts (those covered by the letter K) with the Śāradā manuscripts’ (Bronkhorst 2011: 47); and von Hinüber ‘erroneously repeats Grünendahl’s view that the *Mahābhārata* critical edition is “based ... on the idea developed by H. Lüders, of the coincidence of the scribe and the recension, something that, as R. Grünendahl has shown, cannot be upheld”’ (Adluri and Bagchee 2018: 270, citing and translating von Hinüber 2000: 21 n. 19); and Fitzgerald has written in one place that ‘Grünendahl’s study identified a number of problems and inconsistencies in Sukthankar’s

editorial approach’ (Fitzgerald 2004: xvi), and in another place has mentioned what he calls Edgerton’s and Grünendahl’s ‘searching’ criticisms of Sukthankar’s editorial principles, including the criticism that ‘the script-premise is, as a general description of the working of the manuscript tradition, not warranted’ (Fitzgerald 2010b: 77); and Pollock has noted that ‘doubts about the “writing-system premise” that underlies epic text-criticism and the reality of regional versions have recently been raised by Grünendahl 1993’, and that ‘Grünendahl 1993 importantly reconsiders the logic and reality of “regional versions” of the *MBh*’ (Pollock 2003: 111 n. 155; Pollock 2006: 229 n. 11); and, finally, Hildebeitel has written that ‘it is necessary to bear in mind Grünendahl’s important reservations about V. S. Sukthankar’s *Schriftartprämissen*: the latter’s grounding premise as the first Critical Edition General Editor “that a kind of script constitutes a ‘version’” (Grünendahl 1997: 30)’ (Hildebeitel 2006: 231). So although Sukthankar stated that a ‘textual peculiarities ... are, in final analysis, the real basis of our classification’ (Sukthankar 1933: lxxv), nonetheless the initial classification by script, whether based on rule of thumb, prejudice, or political sensitivity, has cast a long shadow.

#### Selection of Manuscripts

Although the survey of *Mahābhārata* manuscripts undertaken by the Poona editors was unprecedented, only a fraction of the extant manuscripts were collated for the edition. For the *Ādiparvan* ‘there are known to be about 235 manuscripts ... about 60 were actually utilized’ (Sukthankar 1933: v). Dunham notes that the alleged script-group versions of the text (B, G, T, etc.) are internally heterogeneous, and suggests that

To some extent this heterogeneity is a deliberate result of editorial policy, where manuscripts were selected for the critical apparatus because they were thought to reveal the extremes of variation within the version [see Sukthankar 1933: vi]. But by adopting this criterion of selection without a complete survey of all extant manuscripts, it has been impossible to assert with complete certainty that the manuscripts chosen are fully representative, or for that matter, of the way in which they are representative.

(Dunham 1991: 16)

Bigger suspects that ‘the division between the two recensions [northern and southern] may have been made a little more clear-cut by the principles according to which the editors of the Critical Edition selected their mss.’ (Bigger 2002: 29); and such a suspicion – or indeed the contrary suspicion – may also legitimately be entertained with regard to any and all of the divisions within the editors’ presented *stemmas*. All parties seem to agree that ‘A collation of all known mss would not have been manageable’ (p. 29 n. 68; cf. Sukthankar 1930: 260–61; Winternitz 1934: 160–61; Katre 1941: 35–36; Tarrant 1995: 110, 114); but nonetheless it is quite possible that ‘If a different collection of manuscripts had been chosen, the reconstituted text would have been different’ (Phillips-Rodríguez 2011: 213), and so without complete documentation, the *stemma* – and hence the reconstituted text – is

moot. Adluri and Bagchee defend the editorial practice of using only selected manuscripts on the grounds that ‘*eliminatio codicum descriptorum* is well understood in textual criticism. There is no reason to include all manuscripts known if the majority can be eliminated as *codices descripti*’ (i.e., as not containing any stemmatically relevant variants not also present in the retained manuscripts; Adluri and Bagchee 2018: 434–35; cf. Reeve 1988: 456; Reeve 1989). This is true, but nonetheless without reexamining the eliminated manuscripts we cannot know that their elimination would follow only from this principle. Interested readers may attempt to confirm or dispute the *stemma* and the reconstituted text on the basis of the critical apparatus, but that apparatus only gives details of the specific manuscripts that the editors chose for the edition. In fact the work of the critical editors cannot properly be checked without reassembling and rescruinising the entire body of evidence, including all the eliminated manuscripts – something that is not currently feasible and probably never will be.

In addition to the question of whether the manuscripts chosen to represent the regional scriptal versions were representative of those alleged versions in all significant respects, there is also the problem that the editors might not actually have included all such versions. Sukthankar mentions that ‘The only important scripts unrepresented in our critical apparatus are: Kannaḍa, Uriyā and Nandināgarī’ (Sukthankar 1933: vi; Brockington 1998: 59–60), but as Dunham notes, Sukthankar doesn’t explain this omission, so it is difficult for us to know how consequential it might be (Dunham 1991: 2). Dunham thus emphasises the necessity for further investigations of the extent of the *Mahābhārata* manuscript materials (p. 18). In the same vein, in his review of Sukthankar’s first fascicules many decades earlier, Edgerton had hoped that future manuscript discoveries might allow greater clarity concerning the Śāradā and Mathilī versions, and wrote that ‘It is possible that some new unknown recensions may come to light’ (Edgerton 1928: 187). Hardly any *Mahābhārata* manuscript discoveries have been claimed as particularly significant since then, and none have been agreed to be so;<sup>20</sup> but it is hard to say whether this would constitute support for the existing edition and its reconstituted text. Still, it is worth noting Reeve’s comment that ‘Few manuscripts, I suspect, are indispensable’ (Reeve 1989: 5); to reconstruct any particular archetypal feature one will need only a fraction of the extant manuscripts, as long as one can tell which ones.

If the reconstituted text is sensitive to the choice of extant manuscripts used for the edition, it is undoubtedly sensitive to the range of manuscripts, and thus text-families, that have survived at all. It may be that the most significant decisions about which manuscripts to use for the edition have been made not by the editors, but by happenstance. Trovato’s

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<sup>20</sup> A Nepālī manuscript dubbed ‘the oldest extant MS. of the *Ādiparvan*’ came to light after the production of the *Ādiparvan* edition. Sukthankar showed that it constitutes no threat to his reconstituted text (Sukthankar 1939), and Bigger’s subsequent argument about it has been refuted by Adluri and Bagchee (2018: 75–88). Brinkhaus discusses the colophons of two *Harivaṃśa* manuscripts not used for the critical edition (Ñx and Dx), but draws no clear implications for the reconstituted text (Brinkhaus 2002: 164–75).

discussions of ‘real trees’ of manuscript traditions (i.e., trees including all the manuscripts of the text that ever existed, not just the ones whose descendants have survived, Trovato 2017: 92–93, 135–36, 144–46), and Timpanaro’s discussions of ‘extra-stemmatic contamination’ (where there is horizontal transmission from a part of the real tree that is not part of the discernable *stemma*, Timpanaro 2005: 179–80), remind us that for all we know there may once for example have been, in addition to the northern and southern recensions of the *Mahābhārata* (Sukthankar’s N and S), a number of other recensions, now unknown except insofar as they may have successfully affected the descendants of the surviving N and S recensions. Here the word contamination would seem too negative, since that process might supply archetypal readings not otherwise available.

### Accuracy of *Stemma*

What of Bédier’s suggestion that ‘la force dichotomique’ leads to the construction of unnecessarily bipartite *stemmas*? I noted earlier that Vaidya’s *Harivaṃśa stemma* has more trifurcations than Sukthankar’s *Ādiparvan stemma*; but both *stemmas* show the same primary bifurcation into N and S (see Figures 2 and 3). Edgerton’s analysis of the *Sabhāparvan* manuscripts, however, led to a different result: ‘For this book ... I do not regard “N” as a historic reality. I do not believe that W and E [i.e., NW and NE, Sukthankar’s  $\nu$  and  $\gamma$  in Figure 2] are descended from a common secondary archetype’ (Edgerton 1944: xlvi; for the general argument, see pp. xlv–xlix). Edgerton didn’t present a visual *stemma*, but if he had it would have differed from Sukthankar’s in omitting N and in elevating  $\nu$  and  $\gamma$  to stand alongside S as two of the now three main recensions (cf. Mehta’s *stemma*, Figure 4 below).

Edgerton was the only *Mahābhārata* editor to postulate three recensions (Dunham 1991: 15 n. 40).<sup>21</sup> But why? Is the difference in the *Sabhāparvan* data, or in the eye of the beholder? Edgerton admits that ‘It may be ... that the relations of W and E in Book 1 are closer than in Book 2’ (Edgerton 1944: xlix), though he seems to doubt it. Without a systematic reevaluation of the apparatus data (and given the comments above, perhaps even after such a reevaluation), it would be hard for us to comment.

One would expect fewer wavy lines in Edgerton’s text, because editorial arbitration between northern-recension and southern-recension variants is not required. However, in order to make the *Sabhāparvan* volume ‘consistent’ with the others, Edgerton has still put the wavy line ‘under readings in which S unanimously differs from an agreement of W and E, unless there are *intrinsic* reasons for feeling more confidence in the W-E readings as adopted’ (Edgerton 1944: xlix). As Edgerton himself points out, Sukthankar’s tendency, in what for him were stemmatically unresolvable cases, was to privilege the northern variant (NW + NE or  $\nu$  +  $\gamma$ ) over the southern, and so in fact Edgerton’s adherence to a three-

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<sup>21</sup> A full list of pictorial *stemmas* in the introductions to the Poona *Mahābhārata* volumes is as follows: Sukthankar 1933: xxx (*Ādiparvan*); Sukthankar 1942: xiii (*Āraṇyakaparvan*); Raghu Vira 1936: xvi (*Virāṭaparvan*); De 1940: xxiv (*Udyogaparvan*); Belvalkar 1947: cxv (*Bhīṣmaparvan*); Vaidya 1969: xxiv (*Harivaṃśa*). Belvalkar’s *stemma* for the *Bhīṣmaparvan* (Figure 5 below) includes diagonal dotted lines indicating contamination. All these *stemmas* are reproduced in Adluri and Bagchee 2018: 199, 394–96.

recension model would not have affected his reconstituted text (*ibid.*). There is consistency across the edition on this, because the other editors, who assumed a bipartite *stemma*, shared Sukthankar's policy of routinely privileging the northern variant over the southern (thus Vaidya has *yajña* not *mantra* at 39.26a in Figure 1, etc.).

Despite the fact that Edgerton's differently imagined *stemma* apparently has no implications for the reconstituted text, it is nonetheless curious. From one perspective, it reminds us that, as pointed out above (p. 18), the critical edition of the *Mahābhārata* is a series of critical editions of separate *parvans*, and thus the *Mahābhārata* textual tradition is actually a group of separate textual traditions. 'Different parts of the MBh may have different histories of transmission' (Phillips-Rodriguez, Howe, and Windram 2010: 40 n. 12), and those different histories would be represented by different *stemmas*. From another perspective, Edgerton's view also reminds us that different *parvans* had different editors, no doubt with different ranges of skills, and that even if the editorial method was supposed to be the same for every *parvan* regardless of the data, its application must have admitted of some diversity. These two factors – the variety of the data and the variety of the editors – may cause some discomfort when weighed against the canonical status enjoyed by Sukthankar's methods, developed as they were on the basis of the *Ādiparvan* data alone. The exalted reputation that Sukthankar achieved on the basis of his text-critical writings is not shared by the other Poona editors, and yet the judgements that they made when selecting and grouping manuscripts were just as significant as his were. After he learned of Sukthankar's death, Edgerton wrote that 'the loss to scholarship is immeasurable ... I am appalled at the thought that it will now be necessary to entrust the *Mahābhārata* edition to others' (Edgerton 1943: 136). The obvious implication is that the quality of the edition might be compromised as a result. Most of the editors did not draw up *stemmas* for their individual *parvans*; Adluri and Bagchee say of Sukthankar's *Ādiparvan stemma* that 'Its influence on all subsequent editors (with the possible exception of Edgerton) is palpable' (Adluri and Bagchee 2018: 393), but one might have preferred their reconstructions to have been derived independently from their independent data. Vaidya, the project's last general editor, wrote that 'The Parvan-editors were paid at the rate of Rs. 5/- per printed page of demi quarto size ... this rate, in my opinion as well as in actual experience, worked out to be not more than *one rupee a day*' (Vaidya 1966: iii). Being paid by the page might conceivably have encouraged editors to take short-cuts.

#### The Principle of Scribal Non-Omission

In his critical review of the Poona edition, Mehta agrees with Edgerton about the tripartite nature of the *stemma*, apparently not just for the *Sabhāparvan* but for the whole *Mahābhārata*. See Figure 4 for Mehta's *stemma*. However, in Mehta's case the tripartition *would* affect the reconstituted text, because it is accompanied by the claim that sometimes NW (Sukthankar's v) and S both independently omitted an archetypal passage, which must thus be restored from the central NE version (Mehta 1971: 110–15). Mehta provides circumstantial arguments to explain why just those two recensions, which he calls peripheral, would have omitted just those passages (Mehta 1972). In terms of proposing

that certain passages have been omitted from certain manuscripts and recensions, we can compare Pandey 1978; Bigger 1998: 164 (on which see also Adluri and Bagchee 2018: 75–88); Szczurek 2017; and others.

**Main Archetype**

**Recensional sub-archetypes**

**Versional sub-archetypes**

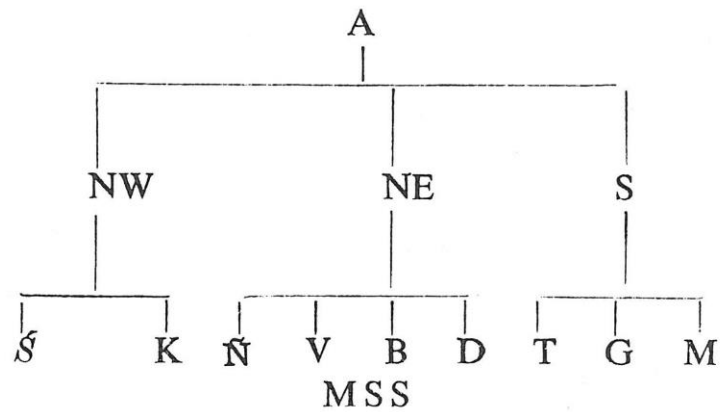


Figure 4. Mehta’s Tripartite *Mahābhārata Stemma* (Mehta 1971: 109)

The Poona principle of non-omission affects not so much the form in which the reconstituted verses appear, but which ones appear in the reconstituted text at all. If the assumption that scribes did not omit material is not to serve for all cases, then one might have to reexamine any and every case. Regarding passages found only in some versions (and thus placed within the apparatus), the assumption that they are not archetypal might or might not be correct. How would we know? But if it is not, it won’t help for the editor to have behaved as if it is correct, except in some exceptional cases. Rather, that editor should do without the assumption, and think again from scratch about how and whether the text might be reconstituted. The difficulty of case-by-case procedure on this question explains why the principle of scribal non-omission was so often stressed by Sukthankar (see again the quotations on p. 14 above) and was also stressed by Winternitz (1934: 162–63), by Lüders (cited in Sukthankar 1933: xcvi n. 2), and with particular intensity by Edgerton (1944: xxxiv–xxxv). As mentioned earlier, because versions differ widely in the passages they contain, this principle can seem to be more vital to the critical edition than the wider stemmatic principle is, and it is hard to question it without simply debunking the edition (cf. Biardeau 1970b: 303).

One may question the critical edition’s reconstruction of particular verses (Winternitz 1934: 164–69; for further references, see his p. 164 n. 2), and/or one may argue that in particular instances the editors have not followed their own principles (Brockington 1998: 293 n. 142; Hildebeitel 2006). But I think the criticisms of the edition discussed above are the most important ones in terms of their potential implications for the reconstituted text as a whole. Their effect has been variously assessed. Dunham says that ‘The text of the *Mahābhārata* as it appears in the C.E. cannot be regarded as a version known in any part of India at any time in the past’ (Dunham 1991: 17; cf. von Hinüber 2000: 22, trans. Adluri and

Bagchee 2018: 31 n. 22), whereas Mehendale says the reconstituted text ‘is not a fiction, but a text which really once existed’ (Mehendale 2009: 10), and Hildebeitel says ‘it is a largely successful reconstitution of the *Mahābhārata* as a work of written literature’ (Hildebeitel 2011b: 87). It is important to note that despite appearances, Dunham and Hildebeitel could both be right on this. Bigger is reassuringly agnostic: ‘we have no means of ascertaining how accurately the Critical Edition represents the text of the normative redaction [i.e., the archetype]’ (Bigger 2002: 24 n. 40).<sup>22</sup> If Bigger is right, then opinions on this matter should be irrelevant, and perhaps expressing them is potentially wasteful. Why make something up when there are so many other important things to do? Elsewhere I have suggested that the reconstituted text is particularly credible where we can identify structural symmetries within it (Brodbeck 2006). My ring-compositional analysis of an eight-chapter section of the *Ādiparvan* has been noted by those whose view of the reconstituted text its results might seem to support (see e.g. Hildebeitel 2012: 157 n. 1), but this approach has not yet been taken further, and is necessarily limited to just some aspects of the text. As far as I know, there haven’t been any other suggestions of how to test the reconstituted text. So Bigger’s judgement seems to be the most realistic one.

## CRITICISMS OF THE *HARIVAṂŚĀ* EDITION

Because Vaidya’s *Harivaṁśā* edition is part of the Poona *Mahābhārata* edition and shares its general method, the criticisms of the *Mahābhārata* edition that were discussed above apply equally to it. Here I discuss criticisms of Vaidya’s *Harivaṁśā* in particular. By far the most significant of these criticisms concern the extent of the reconstituted text.

### Extent of the Reconstituted Text

As Brockington notes, ‘The text [of the reconstituted *Harivaṁśā*] is radically shorter than that of the vulgate, at around a third of its length (118 *adhyāyas* containing 6073 *śloka* units against 318 *adhyāyas* and around 18,000 *ślokas*)’ (Brockington 1998: 319; on the figure 18,000, see Brinkhaus 2002: 157 n. 5). Judging from conversations with colleagues, Vaidya’s reconstituted text is often devalued *because* it is so much shorter than the vulgate. Couture reports this too: ‘I know perfectly well that Indianists don’t agree about P. L. Vaidya’s critical edition – far from it’ (Couture 1995–96: 81).<sup>23</sup>

I think that here Couture and I are commenting just on the reconstituted *Harivaṁśā*’s reception by the scholarly community. But the question of its wider reception

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<sup>22</sup> What Bigger refers to as the ‘normative redaction’ is the archetype, although Adluri and Bagchee have shown that Bigger’s particular title for it encodes some unfortunate assumptions about the real tree in relation to the surviving manuscripts and their *stemma* and archetype. Bigger imagines the latter as a deliberately bottleneck text. Cf. Bronkhorst 2011; see Adluri and Bagchee 2018: 45–117 (a nicely titled chapter, ‘[Here we Refute] Arguments for a Hyperarchetypal Inference’); cf. also Reeve 2007: 335–38 for brief discussion of the hourglass shape as applied to real trees and/or knowable sections thereof.

<sup>23</sup> ‘Je sais parfaitement que l’édition critique de P.L. Vaidya est loin de faire l’accord des indianistes.’



is very much to the point of the Cardiff *Harivaṃśa* project, and it is conceivable that some faithful Hindus might have objections to the presentation of a text that omits so much of the *Harivaṃśa* that they might know. Ricoeur begins an interesting essay by saying ‘I am intrigued by the question of the response of the community whose text has been “critically edited”’ (Ricoeur 1979: 271); but the essay doesn’t really address that question. Sutton has made related comments on the religio-political implications of the historical stratification of the *Mahābhārata*, suggesting that ‘we in the academic community are not the owners of this text’ (Sutton 2006: 81). The idea of textual ownership is slightly mysterious except within the closely defined remit of law (nine tenths of which is said to be possession), and would in any case be of a special kind for the *Mahābhārata*, which anticipates an audience containing both genders and all four *varṇas*, at the very least (Black 2007: 54–56). As it happens, the kind of historical stratification that Sutton here suggests is potentially insensitive is higher-critical stratification within the reconstituted text, not the stratification that the critical edition effects between the reconstituted text and the apparatus materials. So perhaps the text that Sutton considers to be owned by those beyond the ‘academic community’ is the reconstituted text. If so, then it should not be considered politically insensitive for the Cardiff project to make available a translation of the reconstituted *Harivaṃśa*. I don’t know of any Hindu political opposition to the text that Vaidya reconstituted. In this connection it might be recalled that almost all members of the Poona editorial team were Hindus.

To return to the question of *scholarly* reception. What should we make of the disparity in extent between Vaidya’s text and the vulgate? We have seen above (pp. 41–42) that the reconstituted *Mahābhārata* isn’t as short as some scholars had hoped it would be, and Vaidya’s introduction shows him to be disappointed with his reconstituted *Harivaṃśa* in the same way. In Vaidya’s view – a view the manuscript data cannot support – his *adhyāyas* 99–118 were not in the original *Harivaṃśa* (Vaidya 1969: xxxvii; cf. Brinkhaus 2002: 159–64, whose original *Harivaṃśa* is much smaller even than that). But conversely, perhaps the reconstituted *Harivaṃśa* isn’t as long as some scholars had expected it would be. However, as mentioned elsewhere (Brodbeck 2011: 231), I don’t think the edition should be judged on the basis of whether or not it delivered the expected text. And regardless of any expectations, a reconstituted text is more interesting the more it differs from the vulgate. So it is very significant that the reconstituted *Harivaṃśa* is so short. We can certainly say that the application of the Poona reconstitutive method to the *Harivaṃśa* was *effective*. Views and assessments of the *Harivaṃśa* need to be reconsidered in light of the Poona *Harivaṃśa* project, including those concerning its relationship with the rest of the *Mahābhārata*.

Moreover, as Trovato says, ‘more accurate identification of the archetype is not merely of interest to textual scholars ... It is necessary and helpful for language historians, for students of “scribal cultures”, and for historians *tout court*’ (Trovato 2017: 206; cf. Parker 2008: 182–83). An edition that purports to reconstitute a text that may be more than a millenium older than most of its extant manuscript versions is naturally of interest to historians of many different kinds, and the *Harivaṃśa* is of particular interest in the context

of religious history. If the project of reconstituting the oldest knowable version of the *Harivaṃśa* is a worthwhile project, then its results will be useful whatever they might happen to be.

Some of the criticisms of the brevity of Vaidya's reconstituted text are due to the fact that several omitted passages were present in almost all of the utilised manuscripts. Sometimes the passage was present in all manuscripts but one. Appendix passages 18 (*Punar Jarāsaṃdhena Mathuroparodhaḥ*, 1104 lines), 42 (*Varāhaprādurbhāvaḥ*, 661 lines), 42A (*Nārasimhaprādurbhāvaḥ*, 583 lines), and 42B (*Vāmanaprādurbhāvaḥ*, 3071 lines) were absent only from Ś1, and appendix passages 40 (*Mahābhārataśravaṇavidhiḥ*, 173 lines) and 43 (*Tripuravadhaḥ*, 172 lines) were absent only from Ñ1 (Vaidya 1969: xxx–xxxi).<sup>24</sup> Additionally, appendix passages 24, 33, and 41 were absent only from Ś1 and Ñ1, and appendix passage 8 was absent only from M1–3.

It is interesting to wonder whether omitting these passages from the reconstituted text would be less questionable if there were more manuscripts that did not contain them. I suppose it depends on what one thinks of the assumption of scribal non-omission. But what was Vaidya to do? If he had included a few passages in the reconstituted text that were absent in an important manuscript, he would have been making specific and consequential exceptions to a basic principle that governs the whole edition. If there are critics of Vaidya's editorial decisions here, one might ask such critics how many more manuscripts not containing a passage they would want to find before they would omit it from the reconstituted text, and one might then ask them why they have chosen that specific number. In the absence of new manuscript data, it is hard to imagine sensible answers to these questions. Then one might simply restate the principles of the restorative method, and the logic behind them.

The difference between Vaidya's reconstituted text and the vulgate is most striking in the *Bhaviṣya(t)parvan*, which comprises just five *adhyāyas* in the former (*Harivaṃśa* 114–18), but 135 *adhyāyas* in the latter. On the face of it, this circumstance might suggest that the *Bhaviṣyaparvan* was a particularly useful place to store materials that were interpolated into the *Mahābhārata* manuscript tradition. In relation to the *Bhaviṣyaparvan*, Brinkhaus undertakes to 'consider once again whether or not (or, at least, to what extent) the documentary evidence necessitates such a radical shortening of the HV text' (Brinkhaus 2002: 169). Brinkhaus concludes that

Vaidya's decision gives at least the impression of being a bit arbitrary. Nevertheless it does not seem to be easy to find a fairly satisfactory solution to the editorial problems raised here. Perhaps one would have preferred the inclusion of at least the well-attested appendices I,42–42B [absent from Ś1];

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<sup>24</sup> In Vaidya's list of appendix passages there is a mistake with passage 40, which Vaidya says is absent only from Ś1 (Vaidya 1969: xxxi). In fact it is present in Ś1, but earlier in the text than it is found in most manuscripts (Vaidya 1971: 501; Brockington 2010b: 76). However it is absent from Ñ1, as confirmed by Brinkhaus's chart (Brinkhaus 2002: 171). So this looks like a typo in Vaidya.

appendix I,40 [absent from Ñ1] should be accepted as well; but what about appendix I,41 [absent from both Ś1 and Ñ1]? Here I hesitate to give a definite answer. In short, one can criticize Vaidya's decisions to relegate appendices I,40–45 as too arbitrary; but it is not that easy to argue conclusively in favour of any other solution.

(Brinkhaus 2002: 175)

Brockington seems to agree with Brinkhaus about appendix passages 42, 42A, and 42B (for translations of these three passages, see Saindon 2009):

There is an argument that the passages on the three *prādurbhāvas* Varāha, Narasiṃha and Vāmana (Nos. 42 and 42A–B), which are supported by the *Bhāratamañjarī*, should find a place in the text, since their appearance already along with other *prādurbhāvas* in the text at *adhyāya* 31 is not a strong reason for dropping them here.

(Brockington 1998: 320)

Brockington's allusion is to this comment by Vaidya:

The three *Prādurbhāvas* of Varāha, Narasiṃha and Vāmana, along with many other *prādurbhāvas* of Viṣṇu, are already described in *adhy.* 31 of my Critical Text, and hence I do not see any reason why only these three should have been selected for a further detailed treatment in *Harivaṃśa*.

(Vaidya 1969: xxxiv).

Brockington is right that this argument is weak, since it seems to rely on a literary judgement. However, Vaidya's comments here are superfluous, since he has already identified the absence of these passages from Ś1 as the reason for their omission from the reconstituted text (p. xxxi).

Brinkhaus's fondness for appendix passage 40 is connected to the fact that he views it as part of the 'kernel' of the *Harivaṃśa*, which allegedly grew into the *Harivaṃśa* as we know it. Brinkhaus's kernel consists of Hv 1–10, Hv 20–23.121, Hv 114, Hv appendix 40 'which originally followed immediately after *adhyāya* 114', and Hv 115.1 (Brinkhaus 2002: 160–61). But although the subject matter of appendix passage 40 would certainly fit a location right at the end of the *Mahābhārata*, Brinkhaus doesn't advance any more substantial argument for its antiquity, and doesn't explain how it could be absent from Ñ1 and part of the oldest text. By implication, the principle of scribal non-omission is breached here, with all the theoretical implications discussed above.

Vaidya's decisions not to include such passages accord with the method of the *Mahābhārata* edition as a whole (cf. Vaidya 1954: xxxv), and accordingly Brinkhaus's characterisation of those decisions as 'arbitrary' is misplaced. Brinkhaus all but admits that in fact it would be arbitrary to suspend that method in particular cases.

The discussion above was in respect of long passages not included in the reconstituted text (i.e, appendix passages), but the same kind of comments could be made in respect of many of the star passages printed below the line in Vaidya's first volume. For example, in *adhyāyas* 111–13, the conclusion of the Bāṇa story, the star passages are very numerous and many of them are present in most of the manuscripts. The more obviously the reconstructive method does its work by condensing the text, the more it will inevitably be a talking point.

#### Conclusion of the Reconstituted Text

Brinkhaus discusses the question of where exactly the critically reconstituted text should end. Two of the Malayālam manuscripts were said by Vaidya to be incomplete. Vaidya says of M1, 'incomplete (text ends with adhy. 113 of the Critical Edition)', and of M3, 'incomplete (up to adhy. 113 of the Critical Edition)' (Vaidya 1969: xii). Brinkhaus takes issue with the characterisation of these manuscripts as incomplete:

Both of them end with the colophon of *adhyāya* 113, and M3 adds after that a final remark that possible mistakes of the scribe should be excused, etc. – thus it seems to be meant as a proper conclusion of the HV text.

(Brinkhaus 2002: 171)

It is not clear to me that a *Harivaṃśa* ending with Hv 113 would accord with any of Brinkhaus's postulated stages of development, because in his view the Kṛṣṇacarita was inserted between Hv 23.121 and Hv 114 (p. 162); but an ending at Hv 113 could perhaps be that of the Kṛṣṇacarita before its alleged insertion. Now, if on the basis of M1 and M3 Vaidya had deemed the Malayālam version to end with Hv 113, this presumably would have led him to omit the *Bhaviṣyaparvan* from the restored text for lack of complete attestation; but guided by the fuller testimony of M2, he deems manuscripts M1 and M3 'incomplete' and avoids this step. M3's 'final remark'<sup>25</sup> may not be as conclusive as Brinkhaus thinks (perhaps M3's exemplar was damaged or incomplete). The manuscript data require interpretation here, but I agree with Vaidya's interpretation.

Also, the end of manuscript Ś1 is damaged. This case is particularly ambiguous since Ś1 is the only Śāradā manuscript. 'The last page of this Ms. is damaged, and hence no date or scribe's name is available' (Vaidya 1969: xvi). Brinkhaus notes that 'the last entry for the manuscript Ś1 in Vaidya's critical apparatus refers to the śloka 118.1 ... Thus we do not even have a single plain testimony for the conclusion of the HV at the end of *adhyāya* 118' (Brinkhaus 2002: 172; all the manuscripts apart from Ś1 include appendix passages 41 or 42 after Hv 118). Vaidya leans heavily upon the absence of appendix passages 42–42B from Ś1, so it would be nice to be sure that these were actually absent from Ś1! What was it that

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<sup>25</sup> *bhagnaprṣṭhakatiḡrīvaḥ stabdhadrṣtir adhomukhaḥ |*  
*kaṣṭena likhito grantho yatnena paripālayet ||*  
*karakṣtam aparādham kṣantum arhanti santaḥ |* (Vaidya 1969: 759).

made Vaidya think that only ‘the last page of this Ms.’ was damaged, and not the last however many pages? Unless he is dissimulating, something must have; and there is a precedent (i.e., appendix passage 18) for Ś1 not including widely attested material; and notwithstanding Brinkhaus’s comment about the end of Hv 113 in M3, the end of Hv 118 is the best candidate for being the end of the text. The last 13 verses (Hv 118.39–51, spoken by the *sūta*) are 13 consecutive verses in longer metre, exactly half of the reconstituted *Harivaṃśa*’s total of 26 longer-metre verses, which never occur elsewhere in strings of more than two;<sup>26</sup> and within these verses there are elaborate *phalaśrutis* not just for *adhyāya* 118 but for the great seer’s great poem (*idaṃ mahākāvyaṃ ṛṣer mahātmanaḥ*, 118.43a) – that is, by implication, the *Mahābhārata* as a whole.<sup>27</sup> Again, the manuscript data require interpretation here, but I agree with Vaidya’s interpretation.

### The Marginal Wavy Line

At one point in the *Harivaṃśa*, Vaidya prints 49 verses (Hv 87.29–77) with a wavy line in the margin. This convenience is explained in his *Karṇaparvan* introduction:

In some cases, when the Northern and Southern recensions run parallel and passages are rather lengthy, the Editor was forced to put the wavy line in the margin ... ; for, the wavy line below the text continuously for many verses would have been an eye-sore.

(Vaidya 1954: xxxvii–xxxviii)

In the present case, the matter is explained by the notes in the apparatus. The second volume of Vaidya’s *Harivaṃśa* edition explains that all but one of the southern manuscripts substitute appendix passage 22 (81 lines) for 87.29–48 (Vaidya 1971: 182), and that all but one of the southern manuscripts and also one northern manuscript substitute appendix passage 22A (159 lines) for 87.49–77 (p. 184).<sup>28</sup> So it seems that the passage marked with the marginal wavy line is attested by the southern recension, but in an alternate form, and that rather than excluding both versions from the reconstituted text on the grounds of their

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<sup>26</sup> The *Harivaṃśa*’s longer-metre verses are: 30.19–20; 31.16, 120, 153; 33.32; 85.67; 90.17, 19; 110.73; 111.11; 115.7; 117.51; and 118.39–51.

<sup>27</sup> The sense would perhaps be improved had Hv 118.43 and 44 occurred the other way around, since the *adhyāya*’s *phalaśruti* at 118.44 seems to interrupt the larger text’s *phalaśruti* that began at 118.43. See p. 120 below.

<sup>28</sup> The situation seems to be imperfectly stated in the apparatus to Vaidya’s first volume, which say ‘For [87.]29–59, D6 (also) T1.2 G1.3–5 M subst. a passage given in App. I (No. 21)’ (Vaidya 1969: 560); but the second volume presents appendix passage 21 (197 lines) as inserted (not substituted) after 87.28 by D6 and S (except G2) (Vaidya 1971: 177). The only mention of appendix passage 22 in the apparatus to Vaidya’s first volume is under v. 48, where it is said to be inserted by D6 (Vaidya 1969: 562); and I can’t see that appendix passage 22A is mentioned in the first volume at all. Put simply, the notes in the second volume succeed in explaining the marginal wavy line as printed, but the notes in the first volume don’t.

only partial attestation, Vaidya has included the northern-recension version, with the wavy line in the margin and the southern-recension version in the apparatus. The wavy line is thus performing its usual function, but with a longer passage, rather than with an individual word, *pāda*, or line.

The textual circumstance of parallel passages that we see here in the *Harivaṃśa* is occasionally evident elsewhere in the *Mahābhārata*, particularly in the *Virāṭaparvan* and the *Karṇaparvan*. As stated by Raghu Vira in the introduction to the *Virāṭaparvan*, ‘when the versions begin to run parallel, smooth in their own channels, without coinciding, the reconstruction becomes only partial ... the manuscripts do not permit a clear view of the common source’ (Raghu Vira 1936: xxvi). Adluri and Bagchee write that ‘a competent editor will often be able to identify the true reading (the one exception was the *Virāṭaparvan*, a book that appears to have been shifting until a very late date)’ (Adluri and Bagchee 2018: 454 n. 8), but is not clear to me what exactly is envisaged here as happening to the *Virāṭaparvan* until a very late date, or how late that would be (why would it have to be any later than the date of S?). Pollock says that ‘some sections [of the *Mahābhārata*] were transmitted orally, others entirely in written form, and yet others in a mixture of the two, presumably in relation to their performativity’ (Pollock 2008: 54; cf. Pollock 2006: 224); here I can only think Pollock must be referring to parallel passages such as these, but I don’t see how he can conclude, without discussion or argument, that their transmission was oral. In any case, if these particular passages were viewed in isolation perhaps there would be some merit in Hanneder’s judgement that ‘one would not propose a common archetype’ (Hanneder 2017: 61), but they are in a small minority and are surrounded by text that closely coincides in both recensions. One might think again of Mahadevan’s account of the beginnings of the southern recension (p. 13 above), and wonder whether some accident could have befallen specific portions of the written text that was taken south – portions which were then recomposed.

#### Quibbles about Individual Verses

Couture has questioned whether 108.98, absent from K2 Ñ1 T1.2 G1.3 M1.3 (and with significant variants elsewhere), is really well enough attested to merit a place in the reconstituted text (Couture 2003: 603 n. 4). On the face of it, it seems not. A similar case is presented by the three verses 109.68–70, which are absent from Ś1 Ñ1 T1.2 G1.3 M1–3 (Vaidya 1969: 698–99) and so could reasonably have been omitted from the reconstituted text. Perhaps there are other similar instances. Here we return to the following statement in Vaidya’s *Harivaṃśa* introduction: ‘The editor has to see, while eliminating a certain passage, that the Critical Text arrived at by elimination runs smooth and indicates no break in the narrative’ (Vaidya 1969: xxxvi). My guess is that Vaidya would have wanted to omit these verses on the grounds of relatively poor attestation, but nonetheless decided to include them because he didn’t believe that a version without them could be more original. Perhaps this too helps to answer the puzzle of Vaidya’s ‘few modifications’ that had to be made to the Poona method ‘in view of the special character of this *Khilaparvan*’ (Vaidya 1969: vi, quoted more fully above, p. 17).

Looking at the verses in their narrative contexts can help to explain Vaidya's decisions. 108.98 rounds off the scene between Bāṇa and Aniruddha in a way that seems to be dramatically necessary. After Bāṇa gives the order to kill Aniruddha at 108.88, Kumbhāṇḍa addresses him in 108.89–97 and advises against this. The next chapter switches the scene to Dvārakā, and if chapter 108 were to end with Kumbhāṇḍa's speech there would be a 'break in the narrative', for we would have to wait several chapters before finding out whether or not Bāṇa took Kumbhāṇḍa's advice. With 108.98 included, we know that 'Bāṇa listened to Kumbhāṇḍa's speech, and then he left Aniruddha in the palace to fight to his utmost against the snakes, and retired to his own quarters.' As for 109.68–70, these verses occur between Nārada asking the Vṛṣṇis why they are so gloomy (109.67) and Nārada telling Kṛṣṇa the news about Aniruddha's battle against Bāṇa (109.71–74). Without the intervening verses, Nārada's question would be rhetorical. Admittedly he does already know the answer to it, and his visit to Dvārakā is in order to tell the Vṛṣṇis the news they are seeking, so perhaps the text would just about work without 109.68–70; but it is certainly smoother when these verses are included, since they allow Kṛṣṇa to answer Nārada's question, and this answer then serves as a perfect prompt for him to release his news. Here too, then, the omission of the verses would involve a 'break in the narrative', since Nārada's speech would suddenly change tack after one verse. Vaidya's method here is a relatively unhappy one, since it depends upon literary judgements; but it is certainly explicable.

Couture has also implied that since star-passage \*1259 is 'found in all the key recensions', it should find a place in the reconstituted text (Couture 2003: 603 n. 4). But this implication seems unfounded: \*1259 is present in parts of both recensions, but since it is absent from, for example, Ś1, Ñ1, and M, its omission from the reconstituted text is consistent with the method. Couture concludes that 'the logic underlying the reconstruction of the HV is not always evident' (*ibid.*), but if Vaidya's gnomic comments can explain the problematic verses 108.98 and 109.68–70 as described above, I have found very few examples indeed to support this conclusion; see only the case of Hv 15.44, discussed on pp. 85–86 below.

It must be recalled that the case of individual lines and verses differs from the case of longer passages. Longer passages may be omitted from the reconstituted text on grounds of comparatively poor attestation more easily than individual verses, because there is little chance of a manuscript missing them out by accident. So to take the key manuscript Ś1 as an example, its non-inclusion of appendix passages 18 and 42–42B is taken as grounds for their non-inclusion in the reconstituted text, but in contrast it omits a number of lines and verses (e.g., Hv 23.44; 28.43d–44c; 31.11cd; 81.104cd; 83.19c–20d; 105.5–6; 111.2cd; 112.19c–22b) without this being taken as grounds for their non-inclusion in the reconstituted text. My list of lines and verses missing in Ś1 could no doubt be extended, and could be complemented by similar lists of lines and verses missing in other important manuscripts. Vaidya's practice in such cases allows for the mistakes of individual scribes to be taken into account without impoverishing the reconstituted text as a whole.

In these and other specific instances, the reconstituted text is in principle transparent and corrigible on the basis of apparatus data. But I haven't added or removed any verses to or from Vaidya's reconstituted text while translating it.

There are surely other criticisms that might be made of Vaidya's *Harivaṃśa* edition, but that haven't yet been made because very few scholars have studied it. I myself haven't studied it. I've just studied the reconstituted text (often I haven't even looked at the variants), and then only enough to translate it, while wondering, along the way, what it is.

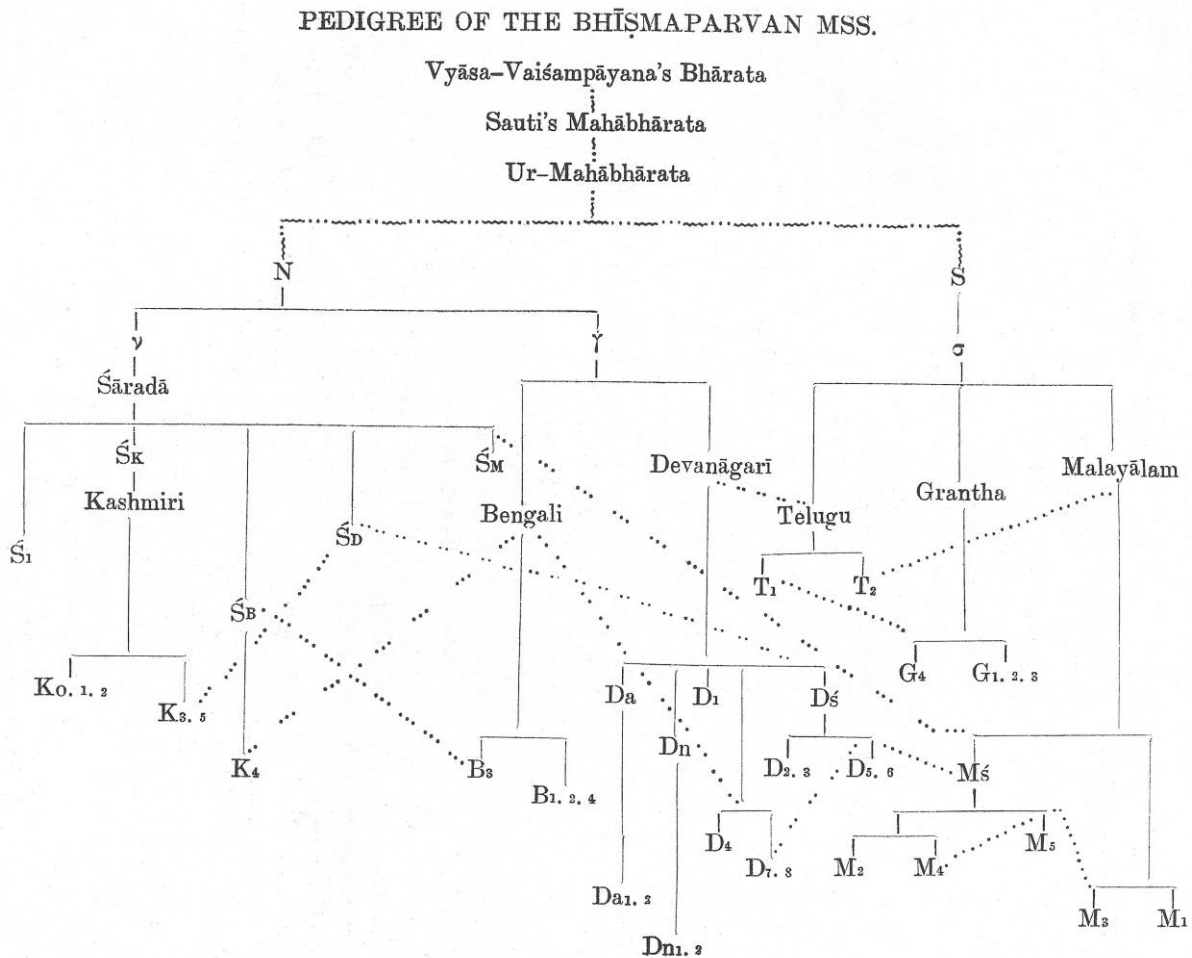


Figure 5. Belvalkar's *Bhīṣmaparvan Stemma* (Belvalkar 1947: cxv)

We return finally to the three underlined manuscripts in Vaidya's *stemma*. Vaidya says: 'my Critical Text is based upon the agreement of the extremes, viz., NW (represented by Ś1), NE (represented by Ñ1) and S (represented by M1-3)' (Vaidya 1969: xxiv; see above, pp. 18–19). Looking at the *stemma*, agreement between NW, NE, and S would determine the archetype whenever it occurs; but that must only be some of the time. As for the suggestion that 'distance is a guarantee against "horizontal transmission," against contamination' (Timpanaro 2005: 85), it may be worth noting by way of comparison that in Belvalkar's



‘Pedigree of the Bhīṣmaparvan MSS.’ (Figure 5), which indicates evident instances of contamination, the Ś and M versions seem at first glance to be the freest from contamination. There are no Nepālī manuscripts for the *Bhīṣmaparvan*.

With respect to the M of the *Ādiparvan*, Sukthankar noted that ‘This is the version of Malabar, the Southernmost extremity of India. It is, in my opinion, *the best Southern version*. It is not only largely free from the interpolations of σ (= T G), but appears to be also *less influenced by N* than σ, wherein lies its importance for us’ (Sukthankar 1933: lxxiii, quoted also in Adluri and Bagchee 2018: 291 n. 180; cf. Edgerton 1932: 253).

Against the idea that distance is a guarantee against contamination, Dutta writes that ‘In a country like India where means of transport were notoriously meagre and slow, the promptness with which manuscripts were written and carried to the distant parts of this country, is an interesting fact to note’, and gives examples (Dutta 1971: 20–21).

The question of geographical extremities goes together with the question of script as a criterion for classification, because both of these questions concern hard contingent facts of the *Mahābhārata*’s terrestrial case, and are thus potentially at cross-purposes with an abstract perspective of theory, whereby all manuscripts might be able to have the privilege of anonymity at the point of delivery of the texts they carry, which are all the system would need in order to configure them into the best *stemma* and derive the best oldest text (cf. Adluri and Bagchee 2018: 191–92; cf. also Fitzgerald 2014: 495, commenting on Olivelle’s edition of the *Mānava Dharmaśāstra* that ‘perhaps the sigla of the manuscripts themselves would be better if they reflected their classification rather than the details of their locations and their scripts’). Another hard contingent fact of the *Mahābhārata*’s terrestrial case is the temporal location of the manuscript survey beginning in the first half of the twentieth century, coinciding with the fight for freedom from British rule in the subcontinent, which culminated in the formation of India and Pakistan in 1947. The circumstances that enabled the Poona project were unique and miraculous, and for all we know, in future the only surviving manuscript versions may be those detailed in the text and apparatus of the Poona edition.

I append Adluri and Bagchee’s general comments with the *Harivaṃśa* in mind:

[C]ontamination of the less promiscuous manuscripts, if present, is likely to be restricted to the insertion of passages or occasional comparisons, leading to the recording of variants as marginal glosses ... Contamination is thus less of a problem in practice than it appears in the abstract. An editor can for the most part account for it, the more so as contamination in the *Mahābhārata* tradition was restricted rather to the tendency to inflation than to combination of readings. Even highly inflated versions such as the Grantha demonstrate their true filiation in terms of their minor readings ... [O]ccasional contamination is doubtless present, transmitted mainly via the comparison of manuscripts, but it is largely restricted to the central manuscript groups and T and G. It ought not deceive us as to the real nature of the relationships in our stemma.

(Adluri and Bagchee 2018: 213–15)

## WHY TRANSLATE VAIDYA'S RECONSTITUTED *HARIVAṂŚA*?

In the introduction to his abridged *Mahābhārata* translation, John Smith said: 'The text translated here is, of course, that of the critical edition' (Smith 2009: lxviii). In her review of Smith's translation, Doniger said: 'there's no "of course" about it' (Doniger 2009: 18). I will argue that despite the criticisms Doniger and others have directed at the critical edition, there is an 'of course' about it.

The principal reason for translating Vaidya's critically reconstituted version of the *Harivaṁśa* rather than some other version is practical, and would be decisive almost irrespective of any other factors. It is that almost all *Mahābhārata* and *Harivaṁśa* scholarship in the world is now done in reference to the numbered chapters, verses, and apparatus passages of the critical edition. The facilitation of cumulative collaborative scholarship is a sufficient justification for the critical edition regardless of the restorative project, and would most probably have resulted no matter what text the editors had arranged the apparatus around. The critical edition is unique and definitive because it purports to include all readings from all versions, and so scholars, whatever their particular interest, can and do refer to it. Given that the critical edition is a definitive edition, it would be perverse to produce a translation with the chapter and verse numbers of some other edition. As all *Mahābhārata* students know to their annoyance, negotiating the post-critical-edition scholarly literature while having to refer to Ganguli's translation of the vulgate (because there is, or at least used to be, no full translation of the critically reconstituted version) is a laborious process involving the frequent intermediate use of a concordance (e.g. Brodbeck 2009: 269–74). The Clay Sanskrit Library *Mahābhārata* volumes (Wilmot et al. 2005–09) translate the vulgate and so require this as well, if they are to be used alongside the scholarly literature.<sup>29</sup> So although for the *Harivaṁśa* we already have Dutt's fine translation of the vulgate (Dutt 1897), there is a demand for a good translation of the critically reconstituted text in particular, which Dutt's translation, even were it to be reprinted, cannot meet.

I think this practical reason is decisive regardless of which version of the text one might, all other things being equal, prefer. But it is also worth thinking about what comes along with Vaidya's version. Vaidya's is a minimal or exclusive version, whereas the manuscript versions or regional versions are inclusive versions to a greater or lesser degree. If, hypothetically, one were to translate a version other than Vaidya's, one would have to choose which, and any rationale for that choice would be somewhat arbitrary. One could choose Nīlakaṇṭha's vulgate, as Lévi and Biardeau would have, and as the Clay Sanskrit Library would have. But why? I suppose because it is a very inclusive text, and

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<sup>29</sup> The Clay Sanskrit Library published 15 out of a projected 32 *Mahābhārata* volumes. For a review of the first six volumes, see Brodbeck 2007. Details of why the Clay Sanskrit Library chose Nīlakaṇṭha's version are not published.

because, perhaps partly as a result, it is very well-known. But Nīlakaṇṭha's method and purpose were idiosyncratic; he simply compiled the text that seemed best to him (Minkowski 2005: 231–32; Minkowski 2010, esp. n. 8; Sukthankar 1933: lxx–lxix). Choosing Nīlakaṇṭha's version might seem to suggest that he or his time or his project or its success is a more immediate subject of interest than the *Harivaṃśa* itself. At this moment of choice, wouldn't one want to think about the *Harivaṃśa* on its own terms, and weigh up one's options accordingly? When Biardeau expressed her preference for the vulgate she didn't do so on the basis of a thorough survey of available versions. And if she had, it would still just have been a personal preference. If her preference was for a very inclusive version, she could perhaps have transferred her preference to the critical edition inclusive of apparatus materials, which is a *maximally* inclusive *Mahābhārata*. Someone else whose preference was for a short version might incline towards the critically reconstituted text without apparatus. After all, even if one is uninterested in or unconvinced by the historically reconstructive aspect of the Poona project, even if one thinks that Vaidya's edition is simply a new recension to stand alongside the others, as Lévi suggested (Lévi 1929: 347), still one might as legitimately prefer it as prefer any other.

Let us stay with Biardeau for a moment more. If she were hypothetically to transfer her preference from the vulgate to the critical-edition-with-apparatus on the grounds of its maximal inclusivity, she would surely not be satisfied by a translation just of the reconstituted text. As Warren points out, 'The proportionality of "apparatus" to "text" can physically signal the power plays of editorial practice: when the apparatus ... [is] reduced to a bare minimum or even eliminated, the silence signals the text's self-sufficiency, naturalizing editorial intervention' (Warren 2013: 125). In this respect, two French translations, one by Biardeau's student and one by *his* student, embody different compromises: Saindon's translation of the *Pitr̥kalpa* (*Harivaṃśa* 11–19) includes translated apparatus passages within the translated reconstituted text, in a smaller font size (see Saindon 1998: 111–12 for discussion); and Couture's translation of the reconstituted text of 'l'enfance de Krishna' (*Harivaṃśa* 30–78) is followed by translation or summary of associated apparatus passages (see Couture 1991: 71–72 for discussion, 327–91 for the apparatus passages).

Saindon's insertion of apparatus passages seems designed to give the manuscript versions their due, but the reconstituted text that now surrounds and links those passages is not any manuscript's version. As for the reconstituted text, its aspiration to approximate a once-extant text evaporates when apparatus passages are placed within it, particularly since these may never all have appeared in the same manuscript version. Couture's method is slightly less odd, but it is nonetheless still odd, for a reason that applies to Saindon's method too: because if apparatus passages are important, then variant words and phrases are equally so. Couture seems to appreciate this, but he says that 'the [reconstituted text's] differences from the Vulgate are (at least for the passages it retains) ordinarily minor' (Couture 1991: 72).<sup>30</sup> That assessment is hopeful; the 'ordinarily' is significant, and in any

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<sup>30</sup> '... les différences avec la Vulgate (au moins pour le passage retenu) sont ordinairement mineures ...'

case Couture is only dealing with a certain section of the text. The kind of compromise made by Saindon and Couture is also made by the available electronic versions of the *Mahābhārata* and *Harivaṃśa*, which supply the reconstituted text and the apparatus passages, but not the variants (Smith 1999; Schreiner et al. 2005). But some critics of such a compromise might argue that the variants of the material in the reconstituted text are more important than the apparatus passages. Thus the method of these translators is an unhappy compromise. Both translators seem to be interested in the oldest available text, and simultaneously interested in the changes gradually made to it; but to do justice to both elements one would have to translate the entire critical edition, variants and all. As James Hegarty has suggested to me, this, or at least such a translation of any smaller part of the critical edition, would be an interesting project and a very useful one for students, but it might run into particular difficulties with the variants, since they are usually presented atomistically at the level of the word, but their translation would require the context. Sometimes different variants could be translated in identical fashion, and so the variance of the variants supplied by the edition could be lost in translation.

Ultimately the decision of what to translate will depend on the translation's purpose. Saindon and Couture want to impart something of the text's reception history, and the apparatus passages supply data about this (for further discussion of the importance of reception history, see Jauss 1970). But a basic assumption behind a *reconstitutive* textual project, with this text as with others, is that there is value in having access – insofar as the method and the manuscript materials permit this – to the text as it was *very early* in its reception history. If one shares this assumption, then it might legitimately guide one's preference of which version an edition should arrange its apparatus around, and also one's preference of which version should be chosen for translation. And if it does, then the apparatus might become comparatively insignificant.

As stated above, we chose Vaidya's *Harivaṃśa* for the practical reason of its ubiquity in contemporary scholarship, which is largely a corollary of the inclusivity of its apparatus, which in turn is a matter independent of the particular version of the text around which that apparatus is arranged. But Vaidya's text gives us something more. Discussing the choice between the stemmatic method and the 'traditional' editorial method (by which he means publishing a corrected 'best manuscript version' with apparatus), Eklund says:

The stemmatic method yields exactly the same information as the traditional method, namely the texts of certain MSS., in this case the texts of those MSS., which are described in the apparatus. Beyond this, the stemmatic method provides a qualified guess about the reading of the original, based on the stemma technique.

(Eklund 2007: 14)

By 'original' here Eklund may mean the archetype, which is effectively the original of the known manuscript versions, or he may mean the autograph, which the reconstituted archetype would be the best available approximation of.

What is the value of such a qualified guess? It could be historical value of various kinds (as mentioned on pp. 56–57 above), based on the fact that the reconstituted text is allegedly much older than the oldest extant manuscript version. Or it could be literary value. And in terms of literary value, the difference between earlier and later versions can seem to be qualitative as well as chronological. For example, discussing the *lectio difficilior*, Patterson says:

[W]hen a scribe substitutes an easy reading for a hard one, he ... transmits ... a text that has been pre-read, smoothing the way for the reader but denying him the opportunity to arrive at his own meaning. The task of the editors, then, is to reverse this process, to return the text to a readable, but as yet unread, condition. The assumption that governs this procedure is that *the poem aspires to a condition of maximum meaningfulness* ... [A]mbiguity or polysemy is a source of difficulty for the scribes, who offer a substitution that expresses one meaning but effaces the others.

(Patterson 1985: 72, italics added)

It is thus clear, as Tom Davis shows,

how important textual criticism is to literary criticism: how completely and utterly literary critics are at the mercies of the vagaries of textual transmission ... It is a distinguishing characteristic of the way that literary texts are read that immense importance can suddenly be invested in a single word, letter, or even comma, out of any that compose it unpredictably ... With literary texts ... you can never tell which word may be seized upon and installed as the core of an argument.

(Davis 1998: 97)

One corollary of this is that ‘The degree of knowledge attainable by textual critics, at those points – the cruces – where their art or science is truly tested, is not commensurate to the importance placed by literary critics on its results’ (p. 100).

What can one do? Davis argues that ‘if the evidence will not deliver what the literary criticism needs ... then it is not for the textual critics to do anything other than point this out’ (Davis 1998: 110). Hence scholars discuss the methods used to reconstitute the text, and their particular problems, and attempt to decide whether or not the evidence will deliver what literary criticism ‘needs’. But given the potentially total nature of literary criticism as Davis describes it, it might seem *a priori* that in almost all cases, and particularly in the cases of ancient texts transmitted in written form, the evidence will *not* deliver this. Even if one leaves both possibilities open in principle, still the threshold at which confidence in the text becomes such as to make literary criticism appropriate will be reached at different points by different people. If one thinks crudely in terms of two non-overlapping types of specialists, one might say that no literary critic can be expected to

take much notice of a textual critic who suggests that the reconstituted text is not certain enough to permit literary criticism, because a literary critic would already suspect that in an important general sense a textual critic thinks textual criticism is more appropriate than literary criticism, and so the textual critic's judgement might seem to be simply an expression of opinion about the relative value of the two types of criticism. Consequently, these words by Brinkhaus seem destined to fall on deaf ears:

[C]ritical remarks ... should not be addressed exclusively to the editors of such Critical Editions, since part of the problem is also their users. The recipients, easily seduced by the label of a 'Critical' Edition, tend to commit themselves purely to the critical text version, as if the critical work had already been done once and for all by the editor.

(Brinkhaus 2002: 175)

As noted above (pp. 57–58), Brinkhaus doesn't think he has good enough grounds to argue for changes to Vaidya's reconstituted *Harivaṃśa* text; but it seems nonetheless that he thinks literary criticism is somewhat inappropriate for that text (cf. Bisschop 2018: 188–89, who may seem to imply that once a critical edition has been produced, the next step is to begin work on another critical edition of the same text). However, the literary critic has not necessarily been 'seduced' by the reconstituted text or its labels; rather, literary criticism cannot take place without a text, and a significant reason for the development of textual criticism was in order to provide a good one. From the perspective of literary criticism, then, the decision to translate the reconstituted *Harivaṃśa*, without variants or apparatus, mirrors rather precisely an important intention of the text-critical method. Even admitting that we cannot tell how closely the reconstituted text approximates the archetype, it should do so as closely as the extant materials allow, and so it is naturally the best text available to the literary critic.

The issue is an important and serious one, because the *Mahābhārata* is one of the finest works of literature that the world has produced, but it is not as widely known or studied as it ought to be. Part of the problem is the long hangover from Macaulay's famous colonial judgement:

I am quite ready to take the Oriental learning at the valuation of the Orientalists themselves. I have never found one among them who could deny that a single shelf of a good European library was worth the whole native literature of India and Arabia.

(Macaulay 1952 [1835]: 722)

In my view, Britain has barely begun to come to terms with its colonial past and is generally unwilling to stand on a level playing-field with the 'other', and in the meantime opinions such as Macaulay's may be more widely shared than they should be. Part of the problem is the *Mahābhārata*'s length, and the fact that it exists in many versions, but that at

least until very recently none of them have been fully available in contemporary English. And part of the problem is that the *Mahābhārata*'s dimensions are routinely misapprehended. When Smith stated, as quoted above, that 'The [*Mahābhārata*] text translated here is, of course, that of the critical edition', he neglected to mention that he had elided that text's final part – the *Harivaṃśa*. By implication, Smith views the *Harivaṃśa* not as part of the *Mahābhārata* but as a 'later mythological text' (Smith 2009: 760 n. 3). This elision is shared by the Ganguli translation (Ganguli 2000 [1883–96]) and seemingly also by the University of Chicago Press translation, which plans to translate the reconstituted *Mahābhārata* minus the *Harivaṃśa* (van Buitenen 1978: ix; Fitzgerald 2004: xv). At a 2010 panel on the *Mahābhārata*'s endings, no panelist discussed the *Harivaṃśa* (Sullivan et al. 2011); and in Shalom's more recent book on the *Mahābhārata*'s ending, scant consideration is given to the *Harivaṃśa* (Shalom 2017). Werner has identified the *Harivaṃśa* as 'the last book' of the *Mahābhārata*, but there seem to be different attitudes on this issue (Werner 2002: 431; cf. Brodbeck 2011: 228–39; Brodbeck 2016: 393–95; Brodbeck in press b; Harindranath and Purushothaman 2015; Sattar 2016).

Eliding the *Harivaṃśa* has interpretive repercussions (this could presumably be a reason for doing it), but it gets no warrant from the text. The *Mahābhārata* says that the (minor) *parvans* constituting the *Harivaṃśa* – the *khilas* (plural not dual) – are among the *Mahābhārata*'s one hundred (minor) *parvans* (Mbh 1.2.69), and that when the *Mahābhārata* was later rearranged into 18 (major) *parvans* none of which included the (minor) *parvans* constituting the *Harivaṃśa*, those (minor) *parvans* were nonetheless still part of the *Mahābhārata* (Mbh 1.2.70–71, 233–34). The word *khila*, which describes the *Harivaṃśa*'s (minor) *parvans* (Couture 1996), can potentially be explained by the position of those *parvans* in relation to the 18 (major) *parvans*: when the 18 (major) *parvans* and the story of the Pāṇḍavas have been completed, they are the *Mahābhārata*'s 'remainder'. But they are part of the *Mahābhārata* as per the critically reconstituted text, and also as per the Spitzer manuscript, the 'oldest extant *parvan*-list of the *Mahābhārata*', which includes the *khilas* (plural not dual; Schlingloff 1969; Brockington 2010b; Shalom 2017: 125–26).

So although the choice to translate the critically reconstituted version of the *Harivaṃśa* rather than any other version is sufficiently justified on practical grounds, there is also a literary aspect to the intention behind the Cardiff project, and to its choice of version. There is reason to suspect that if the *Mahābhārata* is read without the *Harivaṃśa* it will be misunderstood, just as any other literary text would be liable to be misunderstood if one were to omit its final part. Here I do not go into the reasons why the *Harivaṃśa* has so often been removed from the *Mahābhārata*. The point is to supply the *Harivaṃśa* accessibly in English, so that the whole *Mahābhārata* may be properly appreciated. But then it becomes important to supply the reconstituted *Harivaṃśa*, because a literary approach requires the most authorial text available, and because in the case of the *Harivaṃśa* in particular, the reconstituted text is so different from the vulgate version. Just as reading the *Mahābhārata* without the *Harivaṃśa* will give a distorted view of it, likewise reading the *Mahābhārata* with the vulgate *Harivaṃśa* would give a distorted view.

This line of thinking can be brought to bear on the question of whether the Cardiff project should perhaps abridge the critically reconstituted text slightly, in order to facilitate a more accessible product. After all, what might the reader make of the *Harivaṃśa*'s preliminary materials? The subject and the story keeps changing, it is all very weird, and there are lists after lists of one kind or another, and a million names. The main character isn't even born until chapter 48, by which point many readers will already have fallen by the wayside. On the dangers of placing excessive demands upon the reader, see Doniger O'Flaherty 1987: 123–25.

There is a long history of abridgements. An abridgement can sometimes do some of the work required in literary history to keep the source text alive as a cultural object. In this respect an abridgement is an auxiliary text that functions as support for another text. Dubischar, who discusses the term 'auxiliary text', notes that

[T]exts can be too long, too rich or too diverse in their contents, too widely scattered or inaccessible, or they may present serious semantic or exegetical difficulties. Any one of these features ... can become problems and threats to a text (or corpus). Such texts will become increasingly burdensome to read, or irrelevant, or incomprehensible. In short, they will become increasingly unattractive. Chances are that they will become neglected, forgotten, perhaps even irretrievably lost.

(Dubischar 2010: 42)

Auxiliary texts, by solving these problems in varying degrees, help to keep the primary text alive, and to manage its reception. A complete translation would also be an auxiliary text in this same respect, even though Dubischar's catalogue of auxiliary texts doesn't include translations. Using slightly different terminology, Lefevere differentiates five categories of 'rewriting' that can 'manipulate literary fame': translation, historiography, anthology, criticism, and editing (Lefevere 2017 [1992]). Mülke, writing on the genre of *epitome* (condensed versions) in European antiquity, notes that 'the broader public often preferred shorter, easily accessible books ... a short and eventually – in content and/or style – simplified version could serve as some sort of introduction to the complete and more complicated original' (Mülke 2010: 69).

An abridgement can be a real shame, though, and a missed opportunity, especially from the literary perspective outlined above. Alan Williams, before translating the *Masnavi*, decided in his 'creative act' of translation to be 'guided by several principles', first of which was that 'All of the text had to be translated – I did not reshape the work by editing out verses' (Williams 2011: 427). Dick Davis, translating a one-volume *Shahnameh* for Penguin Classics, wrote in his introduction that he had been forced to make some cuts in the interests of space, and also said in relation to some of the cut materials that 'I did not feel I could try the patience of the general reader'. But then he said, of the 'most substantial omission', that the episode omitted is, on one view, 'the heart of the poem' (Davis 2007: xxxv; for discussion of Davis's editorial cuts in his translation of *The Conference of the Birds*,



see Williams 2008: 23). Without any disrespect for the difficult choices Davis made, the problem here is that abridgement inevitably limits the ways in which the presented text can be interpreted, and as such it can be seen somehow to damage the text as a literary object. In a similar way, ‘ancient authors such as Galen and Iamblichus are often suspicious of epitomisation, due to the fact that it could potentially lead to the distortion or falsification of the original’ (Oikonomopoulou 2012). Likewise Nemeč, in his call for more translation work to be done in the field of Indian religions, specifies that this should be ‘unbroken translation’ or ‘complete translation, beginning to end’ (Nemeč 2009: 765, 767). One might say that, because of the openness of future interpretation, when a text is abridged it is damaged to some degree, irrespective of how many or which particular sections have been omitted. (A similar process of damaging also occurs, in a more systematic but less drastic way, in translation.)

The Cardiff team decided to present the whole *Harivaṃśa*, hoping that enough navigational assistance can be provided in the introduction and apparatus. Why present the whole? Because as long as a publisher can be found, it would, in accordance with the above considerations, be rude not to; and because it would be too difficult not to, as one would have to decide what to leave out (one would have to become, excessively, the author of the text); and because, quite apart from the general public, I want to use it for myself, and I want it all.

In this first part of the report we have considered the theoretical and practical problems that affected the Poona team’s text-restorative project and the assessment thereof, many of which are problems of stemmatic text-restoration in particular. Because of the nature of those problems, realistically there is no scope for estimating how accurately the Poona reconstituted *Harivaṃśa* text matches the archetype of the extant manuscripts. Nonetheless, in accord with Sukthankar’s plan, it may for all we know match it rather closely, and it surely does so far better than any other existing version of the text, simply because no other version makes the attempt. The Poona reconstituted text of the *Mahābhārata-with-Harivaṃśa* is a brilliant and awe-inspiring piece of work, a labour of immense love, skill, and coordination, and it would be foolish to downplay it on the basis of the inevitable methodological limitations that it was faithfully produced in spite of.

## Part 2. Emendation Conundrums

My procedure in producing the translation has been to work on a word-processing document with two columns: the Sanskrit text in the left-hand column, and the emerging translation alongside it in the right-hand column.

The initial basis for the left-hand column was the electronic version of the *Harivaṃśa* critical edition that was prepared by a team of 22 scholars in association with the Dubrovnik International Conference on the Sanskrit Epics and Purāṇas. Each scholar took specific portions of the text and transliterated the printed Devanāgarī into electronic Roman script in accordance with a predecided format, including the star passages in indented form within the text, and ignoring the variant readings of specific words and *pādas*. The appendix passages in Vaidya's second volume were also transliterated, and were included separately. Peter Schreiner collated the resulting gobbets. The full electronic text was ceremonially presented to Horst Brinkhaus at the fourth Dubrovnik conference in 2005, in recognition of his affection for, and his work on, the *Harivaṃśa*. The electronic text (Schreiner et al. 2005) was subsequently made available for download from the website of Zürich University, where I obtained it and modified it to form the basis of my working document, carefully excluding the star and appendix passages.

Alongside this working document I have had Vaidya's first volume open on my desk at all times to read from, and verse by verse I have checked the Romanised Sanskrit on the computer screen and corrected any errors within it. The Dubrovnik scholars keying in the text did a very good job on the whole, but of course there are errors. I haven't kept a record of the errors I've corrected, and so I can't be of assistance in the production of any corrected edition of Schreiner's text. But see the appendix to this report, which makes my corrected electronic version of Vaidya's reconstituted text – my left-hand column, the Sanskrit that I translated – available for the use of future researchers.

### PECULIARITIES OF THE *HARIVAMŚA*'S SANSKRIT

When reading Vaidya's text in order to understand and translate it, I have been aided throughout by particular tools. Quine has established the indeterminacy of translation, noting that 'Indeterminacy means not that there is no acceptable translation, but that there are many' (Quine 1987: 9). So I have used the online Monier-Williams dictionary almost all the time (Monier-Williams 1899). I have often referred to Stenzler's Sanskrit primer, as translated into English by my Sanskrit teacher Renate Söhnen-Thieme (Stenzler 1997 [1868]), and I have referred to my old Sanskrit class and revision notes, and to Whitney's book of Sanskrit verb-forms (Whitney 1991 [1885]), and I have used the conjugation engine online on the *Sanskrit Heritage Site*. I have sometimes referred to Speijer's book on Sanskrit syntax (Speijer 2009 [1886]). I have also occasionally used Macdonell's grammar and dictionary (Macdonell 1975 [1901], 2009 [1893]), and Monier Williams's English-to-Sanskrit dictionary (Williams 1976 [1851]), and the St Petersburg

*Sanskrit Wörterbuch* on which Monier-Williams’s Sanskrit-to-English dictionary is based (Böhrtlingk and Roth 1990 [1855–75]; for details of the relationship, see Zgusta 1988).

When Vaidya’s text has puzzled me I have often consulted Nīlakaṇṭha’s commentary (Kinjawadekar 1936), often fruitlessly because his text is different, his coverage patchy, and his interpretations often unhelpful. In order to understand his commentarial style I have found Tubb and Boose’s book on ‘scholastic Sanskrit’ indispensable (Tubb and Boose 2007). Bailey comments that

[C]ommentaries are always composed after the event of the initial composition of the texts being commented upon. In this sense they provide the foundation for the Indian tradition’s interpretation of its own texts. Obviously these kind of hermeneutical canons ... are of a quite different interpretive nature than the more traditional search for the complete historical context of the basic text itself at the time of its own composition.

(Bailey 2001: 189)

I have tried to understand the text in its own time, in terms of its own historical context.

I have been through the whole text twice from beginning to end with close reference to the Sanskrit. The second draft differed markedly from the first, and was much better than it. As a result of the first draft I was far better prepared for the second in terms of the text’s language and in terms of its narrative and conceptual content. Time pressures prevented a third full and in-depth draft.

As Cardona notes, ‘it is customary to recognize a separate genre of “epic Sanskrit” represented in the *Mahābhārata* and *Rāmāyaṇa*’ (Cardona 2003: 107), and thus with regard to the specifics of the *Harivaṃśa*’s language I have made particular use of Thomas Oberlies’s *Grammar of Epic Sanskrit* (Oberlies 2003), which describes the *Harivaṃśa*’s typical departures from the Sanskrit grammatical paradigms presented in Stenzler (cf. Brockington 1998: 82–98; Salomon 1989: 276). This is so despite the fact that Oberlies’s book apparently doesn’t include the *Harivaṃśa* in its titular category of ‘epic’, and gives exhaustive examples from the *Rāmāyaṇa* and from *Mahābhārata* 1–18, but none from the *Harivaṃśa*.

In this section I give a brief overview, in the spirit of Oberlies’s book, of some of the ways in which the *Harivaṃśa*’s language might surprise a junior Sanskritist, illustrated by examples taken from the *Harivaṃśa*. This partial overview is organised according to the pattern and order set by Oberlies’s book, and page references are to that book unless stated otherwise. The examples and references that are given are indicative, not exhaustive.

### Gender

At 11.1d the word *vidha* is masculine or neuter, though when standing alone (rather than as the last member of a compound) it should normally be feminine *vidhā*. At 93.48 and 50 the words *vāsa* and *nivāsa* (indicating the palace residences of Kṛṣṇa’s wives) seem to be neuters, though usually they would be masculine gender (as at 35.57a; 84.7b; 91.22c; 93.66c). At 108.13d the word *vyatikrama* (Uṣā’s ‘transgression’) is neuter, though it is masculine

when its gender is evident elsewhere (10.17; 75.18; 99.13; 107.33). Perhaps in these last two cases the feminine possessor has a neutering effect upon the masculine. For change of gender in other cases, see Oberlies pp. xxxix–xl.

At 37.12a the stem *dos-* is apparently declined in the feminine gender, not the neuter as one would expect. This may be *metri causa*, as it supplies an extra syllable, because it means the accompanying adjective is *āyatapīnābhiḥ* instead of *āyatapīnaiḥ*. At 96.57b the occurrence of *maharddhimat* rather than *maharddhimantam* means that *rājamañca* must be neuter rather than masculine (*mañca* is definitely masculine at 74.5d, 9a, 12a, and 75.41b), but the change of gender protects the metre. At 114.12 the word *pārśva* (‘flank’) is neuter as expected in the first line of the verse, but masculine in the second line. This could likewise potentially be for metrical reasons (cf. Oberlies pp. xxxviii–xl), since if the masculine duals in the second line were changed to neuter duals, then *tāv ubhāv api* would rather be *te ubhe ’pi*, which is one syllable too few.

### *Sandhi*

There is absence of internal *sandhi*, 35.26a (*brahmaṣayo* appears, not *brahmaṣayo*; cf. Oberlies pp. 11–12). There is irregular *sandhi* involving elision of initial long *ā-*, thus with final *-e* we find *te ’tmā*, 45.29a, and with final *-o* we find for example *so ’jñāpayata*, 47.1a; 71.5b; 74.38a; 102.12a (pp. 24–28; *-e + ā-* and *-aḥ + ā-* and *-o + ā-* should normally = *-a ā-* with hiatus, but on the general aversion to hiatus see p. 1; also Sukthankar 1933: xciii).

Double *sandhi* *-aḥ + a-* = *-a + a-* = *-ā-* (e.g. *saṃhāraḥ + ante* = *saṃhārānte*) occurs, 7.47f; 47.5b (Oberlies pp. 23–24). Double *sandhi* *-aḥ + i-* = *-e-* (e.g. *smaḥ + iti* = *sma + iti* = *smeti*) seems to occur, 56.20e; 103.16b (pp. 35–37), although one might alternatively think it terms of the verbal form being without *visarga* (on which see further below under verbs). Double *sandhi* *-aḥ + u-* = *-o-* (*saḥ + upasṛpto* = *sa + upasṛpto* = *sopasṛpto*) occurs, 83.53b (pp. 37–38, giving many examples with *saḥ*). Double *sandhi* *-āḥ + a-* = *-ā-* (e.g. *vṛddhāḥ + abhinandanti* = *vṛddhā abhinandanti* = *vṛddhābhinandanti*) occurs, 17.7b; 34.51b; 63.2a; 83.57b (pp. 40–44). Double *sandhi* *-e + i-* = *-e-* (e.g. *kubje + iti* = *kubja iti* = *kubjeti*) occurs, 51.21c; 71.23a; 77.33c (pp. 47–48). Double *sandhi* *-aḥ + ū-* = *-o-* (*vimānārohiṇaḥ + ūrdhvaḡāḥ* = *vimānārohiṇa ūrdhvaḡāḥ* = *vimānārohiṇordhvaḡāḥ*), though not mentioned in Oberlies, occurs, 67.62b.

### Nominal System

For feminine stems in short *-u*, the accusative plural in *-vaḥ* (not *-ūḥ*) occurs, 59.24c (Oberlies p. 62). Likewise for feminine stems in short *-i*, the accusative plural in *-yaḥ* (not *-īḥ*) occurs, 31.2d; 39.7b; 45.13b; 54.33b; 86.75b (pp. 60–62). Similarly, for feminine stems in long *-ī*, the same accusative plural in *-yaḥ* (not *-īḥ*) occurs, 20.21b; 77.6a, c (pp. 63–65). For the diphthong stem *go*, the accusative plural *gāvaḥ* (not *gāḥ*) occurs, 45.20c (p. 68; Monier-Williams 1899: 363 col. 3). For stems in *-mat/-mant*, the accusative plural in *-antaḥ* (not *-ataḥ*) occurs, 36.51d (i.e. formed from the strong stem, Oberlies p. 71). Such cases are said to be due to the confusion of nominative and accusative plural forms. Conversely, the form *viduṣaḥ* (not *vidvāmsaḥ*) occurs as a nominative (not accusative) plural, 39.11b (pp. 75–76).

Regarding singular forms: at 45.18c and 19b the normally neuter form *mahat* qualifies the accusative of the normally masculine stem *vaṃśa* (*mahad vaṃśam*). Monier-Williams (1899: 794 col. 2) says that ‘in ep. often *mahat* for *mahāntam*’ (i.e., *mahat* would be accusative masculine here); but Oberlies (pp. 70–71) instead diagnoses a change of gender in such cases (thus *vaṃśam* would be accusative neuter here). Either explanation would do.

### Stem Transfer

At 100.60b the accusative singular form *ātmayoninam* (not *ātmayonim*) is a case of stem transfer, with *ātmayoni* declined as if the stem were *ātmayonin* (Oberlies pp. 85–86). At 98.1c there is the genitive plural form *yaṣṭānām* (not *yaṣṭrīnām*), where the feminine noun of agent is declined as if the stem were *yaṣṭā* (cf. pp. 87–88). And at 113.82a there is the accusative singular form *āścaryaparvam* (not *āścaryaparva*), where *-parvan* seems to be declined as if the stem were *-parva*. Oberlies (p. 88) lists comparable examples of transfer from a *-man* to a *-ma* stem. Or the odd *m* may perhaps be an inserted ‘sandhi-consonant’ (p. 5; the next word is *akhilam*). For names with more than one stem, see pp. 140–44 below.

### Pronouns

With regard to the *Harivaṃśa*’s pronoun usage. Oberlies comments in his *Epic Sanskrit*:

There are quite a lot of instances where *me*, *te* and *naḥ* are construed with a verbal adjective in °*tā-* or with a *participium necessitatis* ... These pronominal forms need not be [unusual] instrumentals ... but could be understood to be regular genitives ... The genitive case is the adnominal case *per se* which may be used as the agent of the verbal adjective.

(Oberlies 2003: 102–03; cf. p. 272)

At 107.32c (*kriyate na ca te subhru*), however, where there is no verbal adjective, the form *te* stands in for the instrumental *tvayā* (p. 105; Speijer 2009: 194 n. 3).

### Verbal System

With regard to verbal endings, Oberlies speaks of ‘the exchanges of primary and secondary endings’ (p. 170). This mainly affects plurals, and it means that by losing or gaining a *visarga* an indicative form can sometimes look like an imperative or an unaugmented imperfect (of which there are many), or vice versa. Thus on many occasions for the first-person plural indicative we find the ending in *-ma* (not *-maḥ*): 41.27a; 60.23d; 62.40d; 77.3a, 5a, b, 13b, 16c, 17a, 18b, 32c, 34a, b; 83.7d, 10c, 14b, c, 16d; 89.21b; 100.72b; 102.23c; 103.12b, 29c, 30a; 110.12b, 13b, 14a (pp. 171–72). Particularly frequent is *sma* for *smaḥ*, from the root *as* (p. 207). Likewise for the second-person plural indicative we find the ending in *-ta* (not *-tha*), e.g. *jānīta*, 3.16b (p. 172). Conversely (pp. 173–74), *prāpnuyāvaḥ* at 42.29b is a first-person dual optative, and *tarpayāmaḥ* at 59.8d is probably a first-person plural imperative. Sometimes the mood could be ambiguous.

At 48.19d we find the verbal form *anyasat* (from *ni* + root *as*, class I conjugation), where the augment denoting the imperfect tense is unusually placed before the preverb (Oberlies pp. 183–84). For the root *kṛ* we find the present stem *kurva-* (not *kuru-/karo-*) in use, thus at 58.10d *akurvātām* (not *akurutām*) is the imperfect third-person dual *parasmaipada* form (p. 200). For the root *hā* we find a shortened form of the second-person singular imperative, *jahi* (not *jahihi/jahīhi/jahāhi*), 113.28e (p. 211); the root *han*, whose usual second-person singular imperative is *jahi*, seems less likely here. For the root *arc* we find the passive form *arcyate* (not *ṛcyate*), 39.15d (p. 387).

At 36.17cd we find a periphrastic perfect with the imperfect of *as* used as the auxiliary verb ('We may call it *periphrastic imperfect*', Oberlies p. 224 n. 3): *tāṃ māyāṃ śamayām āstāṃ devau daiteyanirmītām*, 'the two gods disarmed the spell the Daitya had cast'. Oberlies also gives examples of a future imperative (pp. 235–37; 'they are not merely cases of the substitution of secondary for primary endings', p. 237); this may be seen at 81.47d.

The distinction is not always made between active and passive, or between simplex and causative. At 38.52d *agrhyata* is a normally passive form with an active sense, 'he grabbed' (Oberlies pp. 243–44, esp. p. 243 n. 2; cf. Brockington 2000: 36). Conversely, at 93.36a *dadṛśe*, the *ātmanepada* form of the perfect, which is used repeatedly in this passage in an active sense ('Kṛṣṇa looked at ...'), has a passive sense ('Dvārakā looked like ...'); and similarly the *ātmanepada* perfect *śuśruve* occurs in a passive sense ('was heard') at 81.29b; 82.17b; 87.76d (Oberlies p. 245, incl. n. 1). At 38.69b the simple imperative *caratu* should probably be understood in a causative sense (*kālam caratu candramāḥ*, 'the moon must move time onward'), and at 113.8a the absolutive *niśāmya* must be understood as if it were rather the causative form *niśāmya* (pp. 253–55).

Regarding absolutives, the absolutive ending in *-tvā*, which ordinarily is only used for simple roots without a prefix, is found also when there is a prefix; thus *anudhyātvā* (not *anudhyāya*), 18.3d (cf. Mbh 9.62.50; Oberlies pp. 281–82). Conversely, the absolutive ending in *-ya*, which ordinarily is only used for compounded roots, is found also for simple roots; thus *dṛśya* (not *dṛṣtvā*), 62.4a; 118.28d; *tyajya*, 77.18d; *vācyā*, 15.50b; 18.8d; 60.19d; 86.3b (pp. 283–84; for *vācyā* with *svasti*, see esp. p. 284 n. 3). We also find the absolutive used as a finite verb, 108.60c (pp. 285–87). Elsewhere, as Oberlies says, 'the agent of the absolutive is not always the same as that of the main verb' (p. 287). This is often crucial.

### Syntax

Plural forms are sometimes used instead of dual ones (Oberlies pp. 289–90): *abhyutsmayantī*, 59.38c (perhaps; it is dual if the water and the sky are smiling at each other, and plural if the lilies and the stars are); *vayaṃ hi deśātithayo mallāḥ prāptā*, 71.28ab; *mallaiḥ*, 73.7b (perhaps; at 72.13–25 just Cāṇūra and Muṣṭika have been briefed to kill, but at 75.7–76.9 three wrestlers fight, those two plus Tosalaka); *śūrāṇām*, 75.33c; *pretebhyaḥ*, 78.46c; *parivardhitāḥ*, 84.2d (perhaps; otherwise, it would suggest that not just Kṛṣṇa and Baladeva but also other leading Yādavas tended to spend their youth at the cattle station). A dual grammatical subject can also take a plural verb-form (p. 300): at 71.11d Kṛṣṇa and Baladeva take the present indicative second-person plural *arhatha*, not the dual *arhathaḥ*.

Moving on, finally, to the use of cases. At 89.11a the root *dā* takes the accusative *aniruddham* where one would rather expect the dative, as per the variant *aniruddhāya* in manuscript T1 (Vaidya 1969: 572; Oberlies pp. 317–18 n. 3). Similarly, at 38.24a (*alam darpabalam daitya*) the indeclinable *alam* goes with the accusative case where one would rather expect the dative, instrumental, or genitive (Monier-Williams 1899: 94 col. 2; Oberlies p. 319). At 107.53ab (so *'yam etaiḥ śataguṇo viśiṣṭaḥ*, ‘he’s a hundred times better than these ones are’) the instrumental has taken over the comparative role of the ablative (pp. 323–26). At 47.36a (*aṣṭamasya tu māśasya*, ‘after eight months’) the genitive case is used to denote the time elapsed, another sometime function of the ablative (pp. 341–42); but at 112.52ab (... *na me 'dya tvaṃ jīvan pratigamiṣyasi*) and 112.93cd (... *na me 'dya tvaṃ mokṣyase raṇamūrdhani*) Oberlies would suggest that the *me* is a genuine genitive, rather than a genitive form in an ablative function (p. 102 n. 1). At 26.18c the locative *putryām* is used in a dative sense (pp. 348–57), and thus the *putrī* would be the new ‘daughter[-in-law]’; without such slippage of case usage, it would look as if Jyāmagha and Caitrā had a *putrikā* daughter.

That concludes my overview of the *Harivaṃśa*’s departures from Sanskrit grammatical norms, as keyed to the discussions in Oberlies’s book. Before moving on, I mention two further very minor specifics. At 26.5d we find a patronym from *Ṛṥthuśravas* formed by lengthening the first vowel and adding the suffix *-a*, but the patronym is *Pārthaśravasa*, not *Pārthuśravasa*. And at 54.11b and 55.14b the word *silīndhra* (‘mushroom’) occurs, apparently a variant spelling of *śilīndhra* as listed in Monier-Williams (1899: 1073 col. 3).

## LIST OF EMENDATIONS

There are errors in Vaidya’s printed text, and I have sought to correct these in my electronic text and in the translation. These corrections amount to 32 emendations of Vaidya’s text. Where my electronic text emends Vaidya, I have added footnotes to provide the details, with the footnote markers placed after the *daṇḍa* at the end of the line (rather than after the emended word), so as not to compromise electronic searchability.

In identifying errors I have been enormously assisted by being able to compare Vaidya’s text with the text as reprinted without apparatus in the slim fifth volume of Dandekar’s *Mahābhārata Text as Constituted in its Critical Edition* (Dandekar 1976). This reprint has corrected some of the errors that I too have corrected, and I have used it in some cases to confirm my suspicion that there is a typo in Vaidya.

For convenience I list here in a table (Table 1) the 32 emendations that I have made, before going on to discuss specific cases. In five of these cases my emendation is to change what I think Vaidya put on purpose, rather than to change what I think his publisher put by mistake. Admittedly, this distinction is rather impressionistic. The five emendations of what I think Vaidya put on purpose I have called emendations of non-typographical errors, and the emendations I have made in such cases are conjectural. In the last column of the table I indicate whether or not I think my emendation is simply to correct a typo (so there

are just five ‘N’s in the last column), and in the last-but-one column I indicate whether Dandekar also makes this emendation.

**Table 1. Emendations Made to Vaidya’s Text**

<i>Harivaṃśa</i> Location	Vaidya has	I have emended to	Dandekar emends?	Typo?
3.28b	<i>mūhūrtajāḥ</i>	<i>muhūrtajāḥ</i>	Y	Y
13.52d	<i>śukasya</i>	<i>śukrasya</i>	N	N
15.44	verse sits after 15.43	verse sits after 15.40	N	N
20.8d	<i>nīpatāta</i>	<i>nīpapāta</i>	N	Y
20.31b	<i>aṅgirasas</i>	<i>āṅgirasas</i>	N	Y
23.45c	<i>adhitrī</i>	<i>adhistrī</i>	N	Y
24.14b	<i>devamīdhuṣam</i>	<i>devamīdhuṣaḥ</i>	N	N
28.44a	<i>ariṣṭanemes tu sutā</i>	<i>ariṣṭanemir aśvaś ca</i>	N	N
31.137c	<i>ājānabāhuḥ</i>	<i>ājānubāhuḥ</i>	Y	Y
35.73b	<i>nīrvīyaiṣā</i>	<i>nīrvīryaiṣā</i>	Y	Y
45.24c	<i>sagārān</i>	<i>sāgarān</i>	Y	Y
47.49a	<i>sumbhanisumbhau</i>	<i>śumbhaniśumbhau</i>	N	Y
57.20a	<i>bhagnorukaṭignīvo</i>	<i>bhagnorukaṭigrīvo</i>	Y	Y
59.8c	<i>vartayāmopabhujjānās</i>	<i>vartayāmopabhuṅjānās</i>	Y	Y
59.36b	<i>sumutkrusṭāni</i>	<i>samutkrusṭāni</i>	N	Y
59.59c	<i>pūjyantām</i>	<i>pūjyatām</i>	N	Y
61.32a	<i>ghūṇamānaiś</i>	<i>ghūrṇamānaiś</i>	Y	Y
62.49a	<i>yuktānām</i>	<i>yuktāyām</i>	N	Y
77.29a	<i>paurajanāsyārthe</i>	<i>paurajanasyārthe</i>	N	Y
77.44b	<i>śloke</i>	<i>śloko</i>	Y	Y
85.61a	<i>kṛṣjēna</i>	<i>kṛṣṇēna</i>	Y	Y
89.7c	<i>nārāyaṇī candrasenā</i>	<i>nārāyaṇīvendrasenā</i>	N	N
92.55d	<i>kama</i>	<i>karma</i>	Y	Y
106.43d	<i>bharaṇīm</i>	<i>dharaṇīm</i>	Y	Y
108.1d	<i>avat</i>	<i>avasat</i>	Y	Y
108.90d	<i>śakratulyaparākamaḥ</i>	<i>śakratulyaparākramaḥ</i>	Y	Y
109.62c	<i>cācintañ</i>	<i>cācintayañ</i>	Y	Y
109.69c	<i>evāssma</i>	<i>evāśma</i>	N	Y
111.9d	<i>māyāivaiṣa</i>	<i>māyāivaiṣa</i>	N	Y
117.4a	<i>āyurhārnyā</i>	<i>āyurhānyā</i>	Y	Y
117.22c	<i>corāś</i>	<i>coraiś</i>	N	Y
117.46d	<i>kaliyuge</i>	<i>kṛtayuge</i>	N	Y



I now discuss these 32 emendations, beginning with the least problematic ones and ending with the most problematic. First I discuss the 27 emendations of typographical errors in Vaidya (those with ‘Y’ in the final column of the table), and then I discuss the five non-typographical emendations (those with ‘N’ in the final column).

## TYPOGRAPHICAL EMENDATIONS

The 27 typographical emendations can be subdivided into those typos in Vaidya which Dandekar has corrected and which I also have corrected (YY in final two columns, 15 instances), and those typos in Vaidya which Dandekar hasn’t corrected but which I have (NY in final two columns, 12 instances).

In the 15 examples of the first subcategory, my suspicion that there was a typo in Vaidya was confirmed by the fact that Dandekar had corrected the obvious error.<sup>31</sup> In two of these cases, metrical considerations also suggested that something was wrong with Vaidya’s text: Vaidya’s readings of *avat* at 108.1d (instead of *avasat*; see Table 1) and of *cācintañ* at 109.62c (instead of *cācintayañ*) yield *pādas* of only seven syllables rather than the

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<sup>31</sup> In addition to helping me confirm typos in Vaidya, Dandekar 1976 also helped to confirm Vaidya’s reading at 106.22d, where in my copy of Vaidya the printing is dodgy and *śūlapāñinā* looks a bit like *śrlapāñinā*, but Dandekar confirms the reading *śūlapāñinā*. However, in Dandekar 1976 I have also found new typos, and/or corrections that I don’t agree with, and I list them here (there will be others that I haven’t noticed). At 8.28d Dandekar changes Vaidya’s *’ti* to the unattested *’pi*, but *’ti* (*kriyate ’ti* = *kriyate + ati*) seems not to be a typo, as it is mentioned also in Vaidya’s apparatus, where *’ti* is differentiated from the more commonly attested *tu* or *ca*. At 16.19e Dandekar changes Vaidya’s *vaighaso* to *vaidhaso*, but *vaighaso* is confirmed by Vaidya’s apparatus; similarly at 23.44a Dandekar changes Vaidya’s *ogho* to *odho*, but *ogho* is confirmed by Vaidya’s apparatus. At 32.17a Dandekar changes Vaidya’s *caturyugāntaparyāye* to *catuyurgāntaparyāye*. At 42.53d Dandekar changes Vaidya’s *eṣa* to *eva*, which Vaidya notes as a variant (T2 G1 M1). At 47.5b Dandekar changes Vaidya’s *strīsanāthāstu* (with its double *sandhi*) to *strīsanāthāsu*. At 51.9a Dandekar changes Vaidya’s *jānubhir uddhṛṣṭaiḥ* to *jānubhiddhṛṣṭaiḥ*, mistakenly omitting the syllable *ru*. At 67.66b Dandekar changes Vaidya’s *eṣyasi* to *eṣyati*. Vaidya lists no variants for *eṣyasi*, but Dandekar’s change is congruent with Kinjawadekar’s text (1936: 210), with Dutt’s translation (1897: 338), and with Menon’s text and translation (2008: 300). I’ve translated Vaidya’s *eṣyasi*, but it may nonetheless be a typo. At 71.19d Dandekar seemingly changes Vaidya’s *dhanaughair* to *dhanaugher*. At 71.29b Dandekar changes Vaidya’s *darśane* to *daśane*. At 71.46d Dandekar changes Vaidya’s *kācocchvāso* to *kākochvāso*. At 87.12d Dandekar changes Vaidya’s *’nvaśān* to *’vaśān*, but *’nvaśān* (= *anvaśāt*, imperfect of *anu* + *śās*, third-person singular) is confirmed by Vaidya’s apparatus. At 93.35a Dandekar changes Vaidya’s *dāsārhair* to *dāsāhair*. At 105.6–7 Dandekar (as also Menon 2008: 450) numbers Vaidya’s 105.6ef as 105.7ab, thus presenting 105.7, and not 105.6, as a three-line *mahāpañkti* verse. At 107.49d Dandekar changes Vaidya’s *ratitaskaraḥ* (‘stealer of sexual pleasures’, i.e., Aniruddha as the rapist in Uṣā’s dream) to *ratibhāskaraḥ*, which Debroy translates as ‘the one who gave you that blazing pleasure’ (2016: 387), and which Menon prints but doesn’t translate (he has ‘that stealthy lover’, 2008: 463). Vaidya records no variants here, Kinjawadekar like Vaidya has *ratitaskaraḥ* (1936: 438, *Viṣṇuparvan* 118.37d), and I suspect that Dandekar’s change is not a typo but an emendation designed to make light of Aniruddha’s deed. Finally, at 110.61c Dandekar changes Vaidya’s *śaighryāl* to *śaghryāl* (apparently; the first glyph is out of line in my copy).

statutory eight. In two cases, it was possible to confirm my suspicion with reference to Vaidya's apparatus. At 59.8c Vaidya has *vartayāmopabhujjānās*, but the apparatus for that *pāda* says that various manuscripts read '-*yuñjānās* (for -*bhu°*)', thus indicating that -*bhuñjānās* was intended in the text (Vaidya 1969: 385); and at 61.32a Vaidya has *ghūṇamānaiś*, but the apparatus lists some variants '(for *ghūrṇa°*)', thus indicating that *ghūrṇamānaiś* was intended in the text (Vaidya 1969: 399).

The 12 examples of the second subcategory are typos that I have spotted but Dandekar didn't. There are six such cases where Vaidya's apparatus confirms what was intended in the text (and that this was not what was printed there), in the manner just illustrated. So at 20.8d Vaidya has *nipatāta*, but the apparatus says 'D<sub>2</sub> *niṣpapāta* (for *nipa°*)', indicating that *nipapāta* was intended in the text (Vaidya 1969: 139); at 20.31b Vaidya has *aṅgirasas*, but the apparatus says 'some Mss. *aṅgirasas* (for *ā°*)', indicating that *āṅgirasas* was intended in the text (Vaidya 1969: 141); at 47.49a Vaidya has *tataḥ sumbhanisumbhau ca*, but the apparatus says 'some Mss. *tataḥ sumbhanisumbhau ca*', indicating that something other than *tataḥ sumbhanisumbhau ca* was intended in the text (Vaidya 1969: 326), the natural candidate then being *tataḥ śumbhaniśumbhau ca*, since the names are Śumbha and Niśumbha at 65.51c on the only other occasion that they occur; at 59.36b Vaidya has *sumutkrūṣṭāni*, but the apparatus mentions variants 'for *samutkrūṣṭāni*', indicating that *samutkrūṣṭāni* was intended in the text (Vaidya 1969: 388); at 59.59c Vaidya has *pūjyantām* (as in the previous line), but the apparatus says 'Ñ<sub>1</sub> *ījyatām* (for *pū°*)', indicating that *pūjyatām* was intended in the text (Vaidya 1969: 390); and at 117.46d Vaidya has *kaliyuge* (as in the previous line), but as variants the apparatus lists *kaliyuge* and '*kr̥tayugaṃ* (for °*yuge*)', indicating that *kr̥tayuge* was intended in the text (Vaidya 1969: 777). Strictly speaking in such cases the discrepancy between the text and the apparatus indicates that there is an error *either* in the text *or* in the apparatus; but if the text is already suspected of containing an error, then this can effectively be confirmed by the discrepancy.

In the six other cases in this subcategory (count them down), no straightforward confirmation of the error was to be found in Vaidya's apparatus, but sometimes the apparatus was diagnostically useful nonetheless. Thus at 23.45c Vaidya has *adhitrī*, but although the apparatus repeats this non-word by listing four variants 'for [a]*dhitrī ca*', those four variants are '[a]*tha strī ca*', '[a]*dhistrīm ca*', '[a]*tho strī ca*', and '[a]*tho strīm ca*', all of which contain the *strī* sound but none of which are quite the *adhitrī ca* that I think is intended in the text (Vaidya 1969: 162). In this case the fact that a certain variant is *not* listed in the apparatus serves as a kind of argument *ex silentio* for thinking that it may have been intended in the text itself.

Something similar can be seen at 62.49a. Here Vaidya's reading of *yuktānām* is translatable and so perhaps would not have jumped off the page as an obvious error (thus *śaradi yuktānām* = e.g. 'when it's autumn – for those who are ready [for it]'), but Kinjawadekar's edition of Nīlakaṇṭha's vulgate reads *yuktāyām* instead (Kinjawadekar 1936: 195, *Viṣṇuparvan* 19.51a), and Dutt, translating the vulgate, has 'with the beginning of autumn' (Dutt 1897: 311), and yet Vaidya's apparatus lists no variants for *yuktānām* as it ought to have done were *yuktānām* not simply a typo. Here my emendation to *yuktāyām* is

also explicitly supported by Couture ('Lire *yuktāyāṃ* au lieu de *yuktānāṃ*', 1991: 254 n. 18, hence his translation 'Quand cette saison d'automne sera arrivée'), and implicitly supported by Menon (who prints *yuktānāṃ* as per Vaidya, but translates *yuktāyāṃ*, 'with the advent of autumnal days', 2008: 271).

At 77.29a, where I have emended Vaidya's *paurajanāsyārthe* to *paurajanasyārthe*, again no variants are listed when one would have expected there to be some had *paurajanāsyārthe* been intended, and again Couture provides explicit support for the emendation (1991: 319 n. 4).

The remaining three emendations in this subcategory are in principle more speculative, since there is no direct confirmation for them. At 109.69c Vaidya has *evāssma*, which I have emended to *evāśma* (*eva* + *āśma*, imperfect tense of the root *as*, first-person plural), since this is the simplest way to make the text say something sensible. At 111.9d Vaidya has *mayyaivaiṣa*, which I have emended to *mayyivaiṣa* (*mayā* + *eva* + *eṣa*). It could alternatively have been just as easily emended to *mayy evaiṣa* (first-person pronoun locative rather than instrumental), which has almost exactly the same sense, but since the immediately preceding typo was a case of an unnecessarily doubled letter, I have favoured that explanation again here. That may seem slightly arbitrary, and consequently we can be thankful that the two emendation options do not differ in sense here. The seeming slightly arbitrary does serve to illustrate how useful it can potentially be to have, as we do in most of the cases above, a second opinion confirming one's impression of what the correct text should be, since it can sometimes be much easier to see that the printed text is wrong than to see what exactly should have been printed instead.

Finally, at 117.22c Vaidya has *corāś* (as in the previous line), which I've emended to *coraiś* in order that it make sense:

*sasyacorā bhaviṣyanti tathā cailāpahāriṇaḥ |*  
*bhakṣyabhojyahaṛāś caiva bhāṇḍānāṃ caiva hāriṇaḥ || 117.21 ||*  
*corāś corasya hartāro hantā hartur bhaviṣyati |*  
***coraiś** corakṣaye cāpi kṛte kṣemaṃ bhaviṣyati || 117.22 ||*

There'll be thieves who steal grain, thieves who steal clothes, thieves who steal food that you need to chew, thieves who steal food that you don't, thieves who steal merchandise, and thieves who steal from the thief; and there'll be someone who kills the thief, and it'll [only] be safe after the thieves have been wiped out by the thieves.

This is the third typo where the erroneous text repeats something from the previous line, so this may seem to be a particular type of typesetting error; but again, the corrected text is slightly speculative.

## NON-TYPOGRAPHICAL EMENDATIONS

In this category are the five cases where I have emended the text but where the text seems to be intended as printed. These emendations are harder to justify than the corrections of typos are, and the small number of cases makes it hard to formulate general rules about when to make a non-typographical emendation and when not to. I shall discuss individually the five cases where I have emended non-typographical errors, and then in the following section I shall discuss some hopefully indicative cases where I considered emending the text but decided not to. Along the way, the reader may or may not consider Kenney's observation that 'It has always seemed to most people a more distinguished intellectual feat to correct a text by altering it than to explain it as it stands' (Kenney 1974:114). One can imagine making emendments simply for the textological pleasure of discussing them. By these lights I have attempted to appear as intellectually undistinguished as possible. For who knows what general principle a single instance may later be cited as an example of?

I should emphasise that my intention is not to revise or critique Vaidya's editorial work. I do not want either to confirm or to refute his editorial decisions. Reediting the *Harivaṃśa* on the basis of Vaidya's apparatus would be an enormous task that I am not qualified to begin. Nonetheless, we should remind ourselves that Vaidya's immediate task was just (just!) to reconstruct the archetype of the manuscripts he surveyed. 'The reading in the archetype may by the way still be wrong – no manuscript is perfect' (Hanneder 2017: 62); 'the archetype ... is not as a rule an especially important exemplar' (Trovato 2017: 136; cf. Bronkhorst 2011: 43), and so 'we can scrutinize the archetype MS and propose certain corrections to that text' (Witzel 2014: 22). As Parker notes, 'what is available [in the form of the reconstituted archetype] is not an authorial text, but the product of a more complicated process in which the author's writings have been preserved but also to some degree changed, for better or worse, by his readers' (Parker 2008: 184). Some changes will be invisible to us, but some will be accidental changes and may be relatively obvious.

### Śuka and Śukra (13.52d)

At 13.52d Vaidya has *śukasya*, and thus Sanatkumāra says, to Mārkaṇḍeya:

*teṣāṃ vai mānasī kanyā gaur nāma divi viśrutā |  
tavaiva vaṃśe yā dattā śukasya mahiṣī dvija || 13.52 ||*

Their mind-born daughter [i.e., that of the Sukāla ancestors] was known in heaven by the name of Gau, and she was given away into your own lineage, brahmin – as Śuka's wife.

Śuka is Vyāsa's son, and thus is in Vasiṣṭha's lineage (Vasiṣṭha > Śakti > Parāśara > Vyāsa > Śuka). Mārkaṇḍeya's lineage is that of the Bhārgavas, and Sanatkumāra certainly knows it (Hv 11.40; 12.12, 21; 13.51, 70; 14.10). Furthermore, at this juncture Sanatkumāra has just

recently described Śuka's wife Pīvarī (13.44–48). The text should read *śukrasya* rather than *śukasya*, since Śukra, also known as Uśanas Kāvya, the priest to the *asuras*, is famously a Bhārgava (and he is referred to as such also at Hv 65.39, which alludes to his tutelage of Kaca; cf. Mbh 1.71–73).

Debroy says that 'There may be a typo here, Shukra being intended instead' (2016: 61 n. 216), but *śukasya* is apparently not a typo in Vaidya, since Vaidya's apparatus notes *śukrasya* as a variant reading: 'K4 Ñ3 V1 D3.4 T2 G1 M1.2.4 *śukrasya* (for *śukasya*)' (Vaidya 1969: 108). So here in emending *śukasya* to *śukrasya* I choose to go beyond Vaidya. Here Vaidya has chosen to prioritise the *lectio difficilior* that is found in a surprisingly large range of manuscripts (all of them apart from the ones listed as containing *śukrasya*); and he is right to do so. But it is possible that that *lectio* was a recent error in the transmission, later corrected (in the listed manuscripts) to what it had always been before.

If my translation were to mention Śuka here, then readers familiar with the sages and lineages of ancient Indian literature would immediately identify Śuka as an error and substitute Śukra instead, and readers in the process of forming such a familiarity would likely be led into confusion. The emendment is thus for the reader. I shall return to the role of the audience in explaining any emendment in due course below, after discussing the four other cases. But nonetheless, it is possible to imagine that *śukasya* is correct as far as the *Mahābhārata* text is concerned and that the text thus portrays Sanatkumāra, or Bhīṣma, or Vaiśampāyana, or Ugraśravas (for they are the various framing speakers of this verse) making a slip of the tongue, a slip that might then be evidence that the text's author (Vyāsa, for the sake of argument) is having a bit of fun at their expense. From that perspective, Ugraśravas is the proximate source of the utterance.

Saindon (1998: 222) preserves Vaidya's reading of *śukasya* here, and refers to Nīlakaṇṭha's commentary in support. Nīlakaṇṭha's text reads *śukasya* (Kinjawadekar 1936: 49, *Harivaṃśaparvan* 18.58), and his commentary on this verse is as follows:

*tavaiva vaṃśa iti mārkaṇḍeyasya bhīṣmaṃ prati vacanam | śukasyeyam aparā bhāryā ||*

This indicates that Nīlakaṇṭha understands the reference to 'your own lineage' in the context of Mārkaṇḍeya addressing Bhīṣma (and not in the context of Sanatkumāra addressing Mārkaṇḍeya), and thus that he understands it as a reference to Bhīṣma's lineage, in which, at some remove, Śuka is located (since Śuka's father Vyāsa is the genitor of Dhṛtarāṣṭra and Pāṇḍu); and it indicates that he understands this wife of Śuka's to be another wife of Śuka's in addition to the recently mentioned Pīvarī. It is certainly true that Śuka could have several wives, but Nīlakaṇṭha's comments cannot solve our lineal problem, since his solution only works because instead of reading *dvija* at the end of 13.52d (which in Vaidya's text makes it clear that Bhīṣma is *not* the most immediate addressee to whom the *tavaiva* would refer), his text reads *priyā*.

The Moved Verse (15.44)

Justifying my second non-typographical emendation will require a bit of context, for its effect is to move a verse from one place to another. Bhīṣma is telling Yudhiṣṭhira about his encounter with Ugrāyudha, which began when Ugrāyudha sent Bhīṣma a message immediately after Bhīṣma's father's death. The relevant portion of Vaidya's text is as follows, with this particular verse highlighted:

*mām amātyaiḥ parivṛtaṃ śayānaṃ dharaṇītale |*  
*ugrāyudhasya rājendra dūto 'bhyetya vaco 'bravīt || 15.38 ||*  
*adya tvaṃ jananiṃ bhīṣma gandhakālīṃ yaśasvinīṃ |*  
*strīratnaṃ mama bhāryārthe prayaccha kurupuṅgava || 15.39 ||*  
*evaṃ rājyaṃ ca te sphītaṃ balāni ca na saṃśayaḥ |*  
*pradāsyāmi yathākāmam ahaṃ vai ratnabhāg bhuvi || 15.40 ||*  
*rāṣṭrasyecchasi cet svasti prāṇānāṃ vā kulasya vā |*  
*śāsane mama tiṣṭhasva na hi te śāntir anyathā || 15.41 ||*  
*adhaḥ prastāraśayane śayānas tena coditaḥ |*  
*dūtāntaritam etad vai vākyam agniśikhopamam || 15.42 ||*  
*tato 'haṃ tasya durbuddher vijñāya matam acyuta |*  
*ājñaptavān vai saṃgrāme senādhyakṣāṃś ca sarvaśaḥ || 15.43 ||*  
***mama prajvalitaṃ cakram niśāmyaitat sudurjayam |***  
***śatravo vidravanty ājau darśanād eva bhārata || 15.44 ||***  
*vicitravīryaṃ bālaṃ ca madapāśrayam eva ca |*  
*dr̥ṣṭvā krodhaparītātmā yuddhāyaiva mano dadhe || 15.45 ||*

Here is a straightforward initial translation:

<sup>38</sup> As I lay on the surface of the earth surrounded by my companions, supreme king, Ugrāyudha's messenger came up and delivered these words:

<sup>39</sup> Bhīṣma, bull of the Kurus. Your mother Gandhakālī [i.e. Satyavatī, Bhīṣma's stepmother] is a glorious jewel of a woman. Give her to me today, to be my wife.

<sup>40</sup> And as a result I, the possessor of the world's jewels, will gladly give you troops and a flourishing kingdom, no doubt about it. <sup>41</sup> If you seek what's good for the realm, for your life, or for your family, then obey my command; for otherwise, you'll have no peace.

<sup>42</sup> He ordered me around while I was down there lying on a bed of straw! And the mere words that the messenger conveyed were like tongues of fire. <sup>43</sup> So, having learned of that imbecile's intention, exalted king, I gave an order of my own – to all the marshalls of the army, for war. <sup>44</sup> **When my enemies see my invincible discus blazing in battle they flee at the mere sight of it, Bhārata!** <sup>45</sup> When I looked over at young Vicitravīrya, who was now totally dependent on me, my body flooded with rage, and I set my mind upon war.

The odd thing about the passage is that 15.44 mentions Bhīṣma as typically wielding a discus in battle, when in fact this is something we never see. This oddity is compounded because Ugrāyudha has very recently been described by Bhīṣma as a discus-warrior, who used his discus to annihilate the Nīpas (Hv 15.36). A few verses after our passage, just before Bhīṣma and Ugrāyudha fight, Ugrāyudha is again described as a discus warrior, but it is explained that on this occasion his discus was ‘criticised by the wise’ and was disarmed because of his desire for someone else’s wife (15.56–57), and then the fight apparently proceeds with no discus in use. So it looks as if 15.44 should be attributed to Ugrāyudha rather than Bhīṣma.

When we peruse Vaidya’s apparatus here, we find that under 15.40 he says that ‘After 40, all Mss. (G4 om.) and printed eds. read 44’, and that under 15.44 he again says, ‘G4 om. 44. All Mss (G4 om.) and printed eds. read 44 after 40’ (Vaidya 1969: 120). Given this detail, it seems that Vaidya has undertaken to move the verse himself, seemingly without any manuscript authority. The verse fits much more simply in its original location after 15.40, because Ugrāyudha is the one with the discus, and because 15.41 would work much more effectively as a threat if Ugrāyudha had just advertised his prize (and eponymous) weapon. Why has Vaidya moved the verse? Poetically, it is then Bhīṣma speaking hyperbolically of his own battle-skill in a manner to match Ugrāyudha, at the moment of his belligerence on hearing the latter’s message. But my emendation is suggested by Vaidya’s notes, and I think the verse is correct in its original location, a judgement that is shared by Saindon (1998: 264) and Debroy (2016: 67 n. 247). I have thus moved 15.44 back to where it was in the manuscripts; but in order to avoid confusion, I have left the verse numbers as Vaidya has them.

#### Devamīḍhuṣa (24.14b)

We move on to the third of my non-typographical emendations. Here is the offending verse as Vaidya has it:

*aśmakyāṃ janayāṃ āsa sūraṃ vai **devamīḍhuṣam** |  
mahīṣyāṃ jajñire sūrād bhojyāyāṃ puruṣā daśa || 24.14 ||*

The first line identifies *sūra* as Aśmakī’s son, and the second line mention *sūra*’s wife and ten sons, who, as the following verse reveals, include Vasudeva Ānakadundubhi, Kṛṣṇa’s father. As things stand, grammatically *devamīḍhuṣam* goes together with *sūraṃ* as an additional description, and so we would either have Devamīḍhuṣa and Śūra as two names of the same person, or we would have *sūra* as an adjective denoting Devamīḍhuṣa as ‘the champion’ (twice in this verse, and also at 24.16, 28; 25.4; 90.15; 94.8, 18; 96.42, 65; 97.2; 98.21; 102.22). Then, since we have already heard at 24.1 that Devamīḍhuṣa is a son of Kroṣṭu and Mādrī (and since Mādrī is confirmed as Devamīḍhuṣa’s mother at 28.10), we would read in Kroṣṭu at 24.14a as the unstated grammatical subject of the verb *janayāṃ āsa*, and imagine that Aśmakī is an alternative name for Mādrī.

This scenario is plausible, but it conflicts with the ancestry of Sātyaki as narrated by Saṃjaya in the *Droṇaparvan*, where Devamīḍha is clearly identified as Śūra's father (Mbh 7.119.6–7). Assuming that Devamīḍha and Devamīḍhuṣa can be equated, I have sought to bring the two accounts into agreement by emending *devamīḍhuṣam* to *devamīḍhuṣaḥ*, thus yielding the following translation of Hv 24.14:

Devamīḍhuṣa begat Śūra – a Champion indeed! – on Aśmakī; and Śūra had ten boys from his chief queen, a delectable Bhoja woman.

The translations of Menon (2008: 91) and Debroy (2016: 102) show that they too have adjusted the text in this way (although Menon, as elsewhere, prints the Sanskrit of Vaidya's text even though his translation departs from it). Brinkhaus also suggests this emendation (2005: 379): 'With this reading in the nominative, the grammatical problem of the two personal names in the accusative joined without "and" is resolved' (n. 37). By implication Brinkhaus would have understood the unemended text to mean that Śūra and Devamīḍhuṣa were brothers.

I certainly do not wish to disallow the text from presenting two different versions of the same lineage – it happily does this on several occasions – but the verse looks very odd indeed as Vaidya has it, not just because of the two accusatives, but also because of the missing nominative. The apparatus for this *pāda* reads as follows: 'K4 V2 B1.2 Dn D1.3–6 T2.3 G2–4 *devamīḍhuṣaḥ* (D1 °ṣā); T4 M °*mīḍhuṣāt* (for °*sam*)' (Vaidya 1969: 183). This shows that my understanding of the situation, with Devamīḍhuṣa as Śūra's father, was widely shared by the scribes, and that there are two ways of expressing it, since the manuscripts that read *devamīḍhuṣāt* also have Āśmakī in the nominative case in the first *pāda*.

*Devamīḍhuṣam* certainly seems to be the *lectio difficilior*, and I agree with the emendation that is evident within the manuscript tradition. Note that the word *devamīḍhuṣam* is marked by a wavy line in Vaidya's edition, indicating that this is one of the cases where the hypothetical texts N and S are thought to have contained different readings which cannot be ranked by stemmatic means. Vaidya's practice in such cases (in line with the rest of the *Mahābhārata* editors, and as explained by Sukthankar 1933: xci, quoted on p. 13 above) was to prioritise the reading of N. By implication, Vaidya thinks that in S, Devamīḍhuṣa was explicitly stated to be Śūra's father here. As indicated above, I don't argue with Vaidya's method. But there will be cases where (the scribe who made) N made a mistake but S didn't, and the reconstituted text includes the mistake. And there will be other cases where an ancestor of N and S (the archetype or one of its ancestors) introduced a mistake and S corrected it but N didn't. See again Sukthankar 1933: xlv–xlvii for S's occasional superiority on intrinsic grounds; see Bagchee 2016: 109–11 for clarification that N is not an ancestor of S.

#### Ariṣṭanemi and Aśva (28.44a)

My fourth non-typographical emendation is at 28.44a, still within the Yādava genealogy. It comes at the end of a passage which largely repeats (albeit with several small name-



changes) details presented several chapters earlier. The passages are 24.7–13 and 28.37–44, listing Śvaphalka's, Akrūra's, and Citraka's children (and also, in the later passage, Ariṣṭanemi's). Brinkhaus has discussed this duplication and, making text-historical speculations more readily than I would, has suggested that 24.1–13 is an 'interpolation' (Brinkhaus 2005: 379–83). Here are the final verses of both passages:

*citrakasyābhavan putrāḥ pṛthur vipṛthur eva ca |*  
*aśvagrīvo 'śvabāhuś ca supārśvakagaveṣaṇau || 24.12 ||*  
***ariṣṭanemir aśvaś ca*** *sudharmā dharmabhṛt tathā |*  
*subāhur bahubāhuś ca śraviṣṭhāśravaṇe striyau || 24.13 ||*

<sup>12</sup> [Śvaphalka's brother] Citraka had sons: Pṛthu and Vipṛthu, Aśvagrīva and Aśvabāhu, Supārśvaka and Gaveṣaṇa, <sup>13</sup> Ariṣṭanemi and Aśva, Sudharman and Dharmabhṛt, Subāhu and Bahubāhu, and two daughters, Śraviṣṭhā and Śravaṇā.

*citrakasyābhavan putrāḥ pṛthur vipṛthur eva ca |*  
*aśvaseno 'śvabāhuś ca supārśvakagaveṣaṇau || 28.43 ||*  
***ariṣṭanemes tu sutā*** *dharmo dharmabhṛd eva ca |*  
*subāhur bahubāhuś ca śraviṣṭhāśravaṇe striyau || 28.44 ||*

<sup>43</sup> [Śvaphalka's brother] Citraka had sons: Pṛthu and Vipṛthu, Aśvasena and Aśvabāhu, Supārśvaka and Gaveṣaṇa. <sup>44</sup> And Ariṣṭanemi's children were Dharma and Dharmabhṛt, Subāhu and Bahubāhu, and two daughters, Śraviṣṭhā and Śravaṇā.

Twelve sons of Citraka are listed at 24.12–13, and two daughters (*striyau*, possibly wives). The verses 28.43–44 are almost identical, but here Ariṣṭanemi is in the genitive case (there is no such variant at 24.13a), and so after 28.43 has listed six sons of Citraka, 28.44 plucks Ariṣṭanemi out of nowhere and lists his four sons and two daughters (possibly wives).

These are the *Harivaṃśa*'s only two mentions of Ariṣṭanemi the Yādava. So how should we understand 28.44, which lists Ariṣṭanemi's children? We could take this Ariṣṭanemi to be Ariṣṭanemi the son of Citraka mentioned in the first passage, even though the second list of Citraka's sons does not include him. But then Ariṣṭanemi's sons and daughters in the second passage will have the same names as their aunts and uncles, the sons and daughters of Citraka in the first passage.

So we might infer a textual problem, and consult the apparatus. The details for 28.44 are as follows (Vaidya 1969: 202). Three manuscripts (K1.4 M4) omit 28.44 altogether; the Śāradā manuscript Ś1 omits its first three *pādas*; and six manuscripts (Dn1.2 T3.4 M1.2) read *ariṣṭanemir aśvaś ca*, exactly as in the first passage. If we were to adopt this latter, southern variant, as Nīlakaṇṭha apparently did (cf. Kinjawadekar 1936: 100; Dutt 1897: 156), then the problem would vanish.

If we were to leave 28.44a as it stands, then here Vaiśampāyana would present a slightly inconsistent genealogy. If readers were consequently to think there is a mistake, they might attribute that mistake to Vaiśampāyana (or Ugraśravas), and thus make sense of the text on its own terms, as if the author were making a joke at the expense of one of his characters; but perhaps they would be more likely, if they noticed it at all, to attribute the mistake to the author, the editor, the translator, or the publisher.

Vaiśampāyana *does* make a significant mistake in the genealogy at Mbh 1.89.47–52, where there is no link down to Pratīpa from his forebears, since Vaiśampāyana apparently follows the wrong son of Parikṣit I, and then has to jump sideways to find the link through to Śaṃtanu (Brodbeck 2009: 26). Perhaps as a result, Janamejaya asks him to repeat the genealogy – which he does, with significant variations, but including the missing link (Mbh 1.90). Sukthankar judged there to be ‘a palpable lacuna in the text’ after Mbh 1.89.51 (Sukthankar 1933: 396; Mahadevan 2018: 51 n. 9), but I think that Vaiśampāyana’s mistake and the consequent genealogical non-sequitur is as the text intends. But the case of 28.44a is different, because there is no recognition of any oddity within the narrative, and no request for further clarification from Janamejaya.

The considerations here are similar to the ones mentioned above in connection with 24.14b, where I emended the apparent *lectio difficilior* in Vaidya’s text; and here too I have made the same decision. One might counter that whereas 24.14 presented problems in and of itself, and the comparison with the other version of the genealogy (in the *Droṇaparvan*) provided a way out of those problems, here the account at 28.44 is not problematic unless or until one compares the parallel account at 24.13 (which is something that the reader might quite easily not do), and so the problem would seem to be interpretive rather than textual. But that argument doesn’t work, because as Vaidya has it, 28.44 requires the reader or listener to recall 24.13 immediately simply in order to make sense of the mention of Ariṣṭanemi. As a result, 28.44 cannot be considered in isolation, and it is impossible to interpret the mistake away.

#### Nārāyaṇī Indrasenā (89.7c)

My fifth and final non-typographical emendation is at 89.7c, within the narration of the *svayaṃvara* of Rukmin’s daughter Śubhāṅgī. Here is the verse as Vaidya has it, together with the three preceding verses:

*śubhāṅgī nāma vaidarbhī kāntidyutisamanvitā |*  
*pr̥thivyām abhavad khyātā rukmiṇas tanayā tadā || 89.4 ||*  
*upaviṣṭeṣu sarveṣu pārthiveṣu mahātmasu |*  
*vaidarbhī varayām āsa pradyumnā arisūdanā || 89.5 ||*  
*sa hi sarvāstrakuśalaḥ siṃhasaṃhanano yuvā |*  
*rūpeṇāpratimo loke keśavasyātmajo ’bhavad || 89.6 ||*  
*vayorūpaguṇopetā rājaputrī ca sābhavad |*  
***nārāyaṇī candrasenā** jātakāmā ca taṃ prati || 89.7 ||*

<sup>4</sup> The Vidarbhan lady, Rukmin's daughter, was called Śubhāngī. Beautiful and dignified, she was famous across the land. <sup>5</sup> And when all the august kings were assembled, the Vidarbhan lady chose Pradyumna, the ruin of his rivals. <sup>6</sup> For Keśava's young son was skilled with every weapon, solid as a lion, and peerless on earth for his looks, <sup>7</sup> and the princess, who was blessed with youth, virtue, and beauty, had fallen in love with him ...

I pause the translation here, omitting just the highlighted words, which describe Princess Śubhāngī as *nārāyaṇī candrasenā*.

*Nārāyaṇī* could be another name for Śubhāngī, or it could mean something like 'belonging to Nārāyaṇa'. If Śubhāngī was a devotee of Kṛṣṇa, then no wonder she wanted to marry his son – but it would be a surprising detail, as Kṛṣṇa's divine identity has been something of a secret so far. But the word *candrasenā* is hard to translate sensibly. Perhaps the twinkling stars are her army, or perhaps *candra* alludes to her lunar-line ancestry; but this would be an odd way to say so. So this *pāda* seems simply to offer *Nārāyaṇī* and *Candrasenā* as additional names, which are never used further, for Śubhāngī.

The apparatus for this *pāda* reads as follows: 'K1.3.4 Ñ1.3 V1.2 B3 Dn D1.3.4.6 G3 M3 *cendrasenā* (for *candra*)°. Ñ2 V3 B1.2 Ds M1.2 *nārāyaṇīvendrasenā*' (Vaidya 1969: 572). The first of these two variants is the better attested, but the second, which includes the crucial comparative particle *iva*, changes things in a more interesting way. The translations of Menon and Debroy both follow this variant (Menon 2008: 393; Debroy 2016: 330), according to which Śubhāngī is compared with a woman named *Nārāyaṇī Indrasenā*. This could well be the *Indrasenā* who is also mentioned at Hv 23.99, in the narration of the Pāñcāla lineage, as Maudgalya's wife and Vadhryaśva's mother.

A woman called *Indrasenā Nāḍāyanī*, who would be the same woman, was mentioned once in the *Āraṇyakaparvan* and once in the *Virāṭaparvan* (Mbh 3.113.24 and 4.20.8). The critical apparatus shows regional variation in the name's spelling on both occasions: most of the B and D manuscripts have *Nārāyaṇī*, and most of the southern manuscripts have *Nāḷāyanī*.

In the *Āraṇyakaparvan* *Indrasenā* is mentioned when the wifely devotion of Ṛṣyaśṛṅga's wife Śāntā is described, and Śāntā is compared in this respect with: Soma's wife Rohiṇī, Vasiṣṭha's wife Arundhatī, Agastya's wife Lopāmudrā, Nala's wife Damayantī,<sup>32</sup> Indra's wife Śacī, and finally, Mudgala's wife *Indrasenā Nāḍāyanī* (Mbh 3.113.22–24). The name Mudgala may stand in for Maudgalya here, in the same way that Bharata can mean Bhārata, Kuru Kaurava, and Yadu Yādava. This Mudgala could be the *kṣatriya* descendant of Ajamīḍha mentioned at Hv 23.96–99, or he could also be Mudgala the gleaner, whose story is told at Mbh 3.246–47. The gleaner Mudgala (also called Maudgalya) refused heaven and attained *nirvāṇa* (Brodbeck 2014: 12–14; Mahadevan 2016; Hildebeitel 2016: 71, 124).

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<sup>32</sup> It is in principle possible that our *Indrasenā* could be Nala and Damayantī's daughter *Indrasenā* (mentioned at Mbh 3.57.21 and 3.73.24), with *Nāḷāyanī* / *Nāḍāyanī* / *Nārāyaṇī* functioning as a patronym to indicate her descent from Nala.

Elsewhere in the *Mahābhārata* Mudgala is mentioned only at 12.226.32, where King Śatadyumna is said to have gone to heaven after giving Mudgala a house made of gold and filled with everything he desired. Maudgalyas are mentioned also at 1.48.9 (as a *sadasya* at the *sarpasatra*) and 13.48.10, 27 (as the name of a mixed-*varṇa* caste, the offspring of a *vaiśya* man and a brahmin woman, who has the same occupation as a Saupāka, though it is not stated what this is).

In the *Virāṭaparvan* Indrasenā is mentioned when Bhīma is talking to Draupadī and urging her patiently to endure what remains of the final year of exile with her husbands. Bhīma lists a number of women who are famous for having followed their husbands through hardships: Sukanyā the wife of Cyavana, Indrasenā Nāḍāyanī the wife of a thousand-year-old husband, Sītā the wife of Rāma, and Lopāmudrā the wife of Agastya. Bhīma says that Draupadī will be exalted and famous like these *pativratās* if she maintains her patience for what remains of their exile (Mbh 4.20.7–13).

On both of these occasions Indrasenā Nāḍāyanī is listed as a woman unusually dedicated to her husband. So the comparison fits the current context, emphasising Śubhāngī's love for her husband by saying it resembled Indrasenā's love for hers.

Further details about 'Indrasenā' are added in certain apparatus materials. To discuss the most important of these, we must first backtrack onto the story of Draupadī and her polyandrous marriage to the five Pāṇḍavas. While the Pāṇḍavas and their mother Kuntī were living abroad after surviving the lacquer-house fire, they were making their way towards Kāmpilya, when Vyāsa appeared and told them they would all marry Draupadī because in a former life she asked Śiva for a husband five times, and so now she has to have five husbands (Mbh 1.157.6–15; Hildebeitel 2001: 49). This has been called the story of the overanxious maiden, or the story of Śiva's boon. In Kāmpilya, Arjuna wins Draupadī at her *svayaṃvara* and the Pāṇḍavas decide she must marry them all. As Yudhiṣṭhira is in the process of trying to convince Drupada to allow this, Vyāsa appears and talks privately with Drupada to that end (1.189). Vyāsa tells Drupada the story of the five former Indras, whereby the Pāṇḍavas are those former Indras reborn to do an important deed for the gods, in conjunction with Śrī-Lakṣmī reborn as Draupadī. Vyāsa briefly gives Drupada divine eyesight so that he can see the six of them in their divine identities (1.189.35–39). Then Vyāsa tells Drupada a second story (1.189.41–47), which is the same story of the overanxious maiden and Śiva's boon that he told to the Pāṇḍavas and Kuntī before they got to Kāmpilya.

After Mbh 1.188 – that is, just before Vyāsa tells Drupada those two stories – all the southern-recension manuscripts include appendix passage 100, perhaps interpolated in order to provide extra justification for Draupadī's polyandrous marriage (for the particular ideological interests of the southern-recension interpolators, see Hildebeitel 2011, 2011b, 2018). In appendix passage 100 (summarised in Deshpande 1978: 26–27) Vyāsa explains to Drupada that in a former life Draupadī was Indrasenā Nāḍāyanī, who was so devoted to her husband Maudgalya that she wished there were five of him so that he might satisfy her more completely. Eventually Maudgalya grew weary of satisfying her as much as she wished, and told her she would be reborn as Draupadī, who would have five husbands.

Indrasenā then went into the forest to perform austerities, and there she had an interaction with Śiva in which he told her she would have five husbands in her next life, because she asked for a husband five times. This is a clear reference – at this point – to the story Vyāsa told at Mbh 1.157, and the appendix passage has thus expanded that story such that Śiva’s boon confirms what has already been inadvertently requested by the overanxious maiden herself from her (one) husband, and also decreed by that same husband. More importantly from our present point of view, the passage 1.app100 explicitly names the overanxious (and here oversexed) maiden – who in 1.157 was unnamed – as Indrasenā Nālāyanī.

Appendix passage 100 also identifies Indrasenā as the weeping woman at the beginning of the story of the five former Indras (Mbh 1.189.1–40), since the appendix passage ends with Śiva telling Indrasenā, whom he has decreed will have five husbands, to go to the Gaṅgā and bring the king of the gods to him from there (1.app100.115–16), and Mbh 1.189 follows immediately. For this weeping woman in the Gaṅgā who fetches Indra (i.e., the present Indra, who will be put together with four previous ones already collected by Śiva), see Brodbeck 2009b: 35–36, with discussion of her possible identity as Earth and Śrī and with further references, but without reference to Mbh 1.app100. After Vyāsa has told Drupada the story of the five former Indras in Mbh 1.189, he goes on to tell him the same story that he told the Pāṇḍavas earlier, about the overanxious maiden whom Śiva decreed would have five husbands (1.157.6–15; 1.189.41–46, the story thus effectively being told in the southern recension for the third time). A handful of Telugu and Grantha manuscripts then add a further passage, Mbh 1.app102, which in its first two lines confirms once again that Draupadī in a former life was Maudgalya’s husband Nālāyanī, who received the boon from Śiva.

Geen has argued that the *Mahābhārata*’s account of Draupadī’s former life – its story of Śiva’s boon – is ‘actually a modified version of the Jain story of Sukumārikā’, which is first known from the Śvetāmbara Jain text *Nāyādhammakahāo* (Geen 2006: 578; for the development of the Jain accounts of Draupadī’s marriage across a variety of texts, see Geen 2005). *Nāyādhammakahāo* 1.16 mentions two of Draupadī’s past lives, first as Nāgaśrī in a brahmin family, and then as Sukumārikā in a merchant family. Sukumārikā generates ascetic power and uses it to ordain that in her next life she will have five husbands. The early Jain version doesn’t involve Śiva, but its mention of two previous births of Draupadī is, Geen argues, implicit in the first account of the overanxious maiden, which refers to her former *karman* (at Mbh 1.157.7; Geen 2006: 585).

Discussing the work of the sixteenth- or seventeenth-century Keralan author Nārāyaṇa Bhaṭṭa, Shalom mentions that he wrote a number of *prabandhas* (short dramatic poems) based on episodes from the *Mahābhārata*, the *Rāmāyaṇa*, and the Purāṇas, and that these *prabandhas* included ‘the *Nālāyanīcarita*, in which Vyāsa tells Drupada about his daughter’s previous birth as Nālāyanī, and the events that were to make her destined to marry the five Pāṇḍavas’ (Shalom 2017: 153). This seems to be a continuation of the tradition of Mbh 1.app100.

After the scene at Kāmpilya, Indrasenā appears twice more in the *Mahābhārata* apparatus materials. After Mbh 1.212.1 the southern-recension manuscripts include appendix passage 114 (summarised in Deshpande 1978: 29), which describes how Arjuna and Subhadṛā's love grew while Arjuna was living in Dvārakā disguised as a mendicant. As the disguised Arjuna falls in love with Subhadṛā, he is said to deem her more beautiful than Kṛṣṇā (Draupadī, his wife), or Indrasenā, or Varuṇa's daughter:<sup>33</sup>

*na kṛṣṇāṃ rūpato mene vāsudevasahodarām | Mbh 1.app114.89 |*  
*prāptāṃ hṛdīndrasenāṃ vā sāksād vā varuṇātmaṃjām | 90 |*

So here Indrasenā is a paradigm of beauty. Finally, where Mbh 5.115.8–14 gives a list of 27 happy couples to whose conjugal pleasures those of Mādhavī and Divodāsa are favourably compared, manuscript G3 adds two further couples towards the end of the list: Maudgalya and Candrasenā, and Kāśyapa and Aditi (*maudgalyaś candrasenāyām adityāṃ kāśyapo yathā*, Mbh 5.\*459). Candrasenā is otherwise unknown in the *Mahābhārata* or its apparatus materials, except perhaps at Hv 89.7b, and so she must be our Indrasenā, by typo (*candrasenāyām* for *cendrasenāyām*) or by some other means.

Putting all this together, we see that Indrasenā Nārāyaṇī was (and was to become more of) a paradigmatic example of wifely devotion, conjugal sex-drive, and beauty. She is thus a fitting woman for Śubhāṅgī to be compared with (Pradyumna is, after all, the incarnation of Kāma according to several accounts), as long as one does not press Bhīma's take on things, which seems to emphasise devotion in the face of considerable hardship and deprivation, the like of which we would not expect Kṛṣṇa's daughter-in-law to experience. It is perhaps slightly odd for Indrasenā to be mentioned at Hv 89.7 on her own, since elsewhere she usually appears in lists alongside other women or couples. But nonetheless I think there are good grounds for emending the text to prioritise the variant *nārāyaṇīvendrasenā* at Hv 89.7c. It should be noted that Indrasenā is identified as the overanxious maiden (i.e., Draupadī in a previous birth) not within the critically reconstituted text but *only within apparatus passages*, which I would take as a layer of interpretation subsequent to the references at Mbh 3.113.24, Mbh 4.20.8, and Hv 89.7.

That concludes the separate discussion of the five non-typographical emendations. Those emendations have been made with a view to improving the text and thus improving the experience of the readers of the translation, but nonetheless there are various ways of thinking about them. From one point of view, it might be suggested that proposing such emendations calls the validity of Vaidya's reconstituted text into question to some extent, and that this is not just the case at these five places. At these five places my eventual decision to overrule Vaidya was triggered in the first instance by my extreme discomfort

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<sup>33</sup> Varuṇa's daughter could be Idā, as at Hv 9.3–11, or *vāruṇī*, 'liquor', as at Hv 83.19, or the *apsaras* Puñjikasthalā, for whom see Goldman, Goldman, and van Nooten 2009: 572, 907. Hopkins (1968: 116–21) mentions no other possibilities.

with the text he had reconstituted, and if I deem Vaidya to have erred on these five occasions, the suggestion would go, then how many more times might he have erred and yet reconstituted a text that makes good enough sense for me not to have noticed his error? The suggestion would be that without attempting to check all of Vaidya's editorial decisions, I would have no idea what the answer to this question might be. Then one might wonder how much doubt was cast in advance over Vaidya's text by the frequency of such emendations, five in 118 chapters, one for every 1215 verses on average. Is that a good ratio or a bad one? Who knows. In any case, the suggestion would be that the more emendations one proposes, the less sense it makes to trust Vaidya's reconstituted text in general.

I think that without defending Vaidya, I can argue against this point of view and these suggestions. As stated earlier, I don't want to make a judgement about the success of Vaidya's attempt to reconstitute the archetype. And emending the text for the purposes of this translation does not necessitate making any such judgement in specific cases. But is that really true, the opponent will ask? What about the principle of *lectio difficilior* as applied by Vaidya? That principle may be the explanation for Vaidya's choice of reading in most of my five examples above. Surely I am saying that he applied that principle too often? After all, the question of when to apply that principle and when not to is a delicate one. In his recent discussion of the principle (2017: 146–48), Hanneder adduces the following four quotations: '... [the rule of *lectio difficilior*] should never be invoked to give precedence to readings that are grammatically defective, incoherent, or contextually awkward' (Sanderson); 'There is an important difference between a more difficult reading and a more unlikely reading' (West); 'The principle *lectio difficilior potior* does not extend to nonsense' (Chadwick); and '... there is a temptation to use [the maxim *difficilior lectio*] as a defence of anomalous syntax or usage; in such cases the more difficult reading may be more difficult because it is wrong' (Reynolds and Wilson).

Nonetheless, I don't think I need to commit myself on the question of whether or not Vaidya, in his task of reconstituting the archetype, should have applied the principle of *lectio difficilior* with more circumspection. After all, it is up to me what I do with Vaidya's archetype, and whatever I do I can do in view of the variants he has detailed. Vaidya's reconstituted archetype is separated from the autograph by some unknown distance of transmission, and during that transmission, errors will have been introduced and not (yet) corrected, in some cases introducing secondary readings which Vaidya may then have identified and put into the reconstituted text by the principle of *lectio difficilior*. Indeed, one would think that any reconstruction of the archetype (perhaps of any archetype) which doesn't contain such 'errors' would be suspect; in the terms of that reconstructive project they might not be errors, they could even be suggestions of success (whether or not the editor goes on to emend them). In addition, there will be cases where N makes a mistake that gets wrongly attributed to the archetype because of the bipartite split. The key here is that my project is not Vaidya's project. It has different terms of reference, and if I can easily correct a few errors for the sake of a more readable translation, why wouldn't I? It doesn't have to reflect upon Vaidya's editorial judgement here or anywhere else; I can correct

errors for the reader without taking a view on whether those errors were introduced into the tradition before, at, or after the archetype.

There's even no need to criticise Vaidya for moving verse 15.44. His role as editor unavoidably aligns him into an authorial position, and thus he can move the verse with nothing more than implied intent. It's easy enough to move it back, and if its position in the edition were some kind of lapse on Vaidya's part, it might be the only one. Nonetheless, because of these five non-typographical emendations in particular, it must be acknowledged that my translation isn't quite always a translation of Vaidya's reconstituted *Harivaṃśa*.

### SOME EMENDATIONS NOT MADE

Once one has corrected the typographical errors, Vaidya's reconstituted *Harivaṃśa* almost always makes good sense. Often the editor has clearly chosen the most difficult reading – the *lectio difficilior* – but a sensible translation is nonetheless possible after due consideration. In this section I discuss, in discussions from shorter to longer, a small number of cases where Vaidya's text is problematic, but where I have resisted the temptation to emend the text. Overall I have tried to resist that temptation as much as possible and to emend the text only as a last resort. So the examples given below are merely indicative of some edges of a set of non-emendations that includes the entire text apart from the 32 emendations detailed above.

At the outset I have decided to translate Vaidya's *Harivaṃśa* because I think there is merit in doing so, and I don't want to betray that initial impulse. If the idea behind Vaidya's project was to reconstitute an ancient work of literature, then who am I to try to improve it? Jean Le Clerc's first law of emendation, in his eighteenth-century *Ars Critica*, was that 'emendation must be demonstrably necessary' (Kenney 1974: 43; cf. pp. 113–14), and in the introduction to his edition of the *Āpastamba Mantrapāṭha* Winternitz commented that

There are numerous cases in these Mantras where every editor would be tempted to have recourse to conjectural emendations. But on closer examination he will remember that he has to edit, and not to correct his text, and that even a grammatically impossible reading has to be retained, if it is warranted by the best authority.

(Winternitz 1897: xv)

While reading the following it should also be remembered that the Sanskrit text I have arrived at and presented in the appendix is not presented as my best improved version of Vaidya's text, but simply as the Sanskrit text that I have translated. As a translator, I only needed to emend the text when I could not discern its sense. So infelicities in Vaidya have generally been tolerated if they are comprehensible.



### Extra Syllables *Metri Causa*

At 33.9c we find the eight-syllable (i.e., one-*pāda*) compound *śailotkarimasamkāśam*, = *śaila* + *utkarima* + *samkāśam*. But the word *utkarima* doesn't occur in the dictionaries. I read the syllable *im* as an intraverbal interjection *metri causa*, thus the sense is *śailotkarasamkāśam*, 'looking like a pile of rocks'. Manuscripts K2 Ñ2.3 V B1.2 D T4 achieve the same effect by reading *śailotkarasamakāram* (*samakāra* has the same meaning as *samkāśa*, but is one syllable longer). In addition to *śailotkarasamakāram*, Vaidya lists the following variants for this *pāda*, all of which likewise get around the issue: *śailendrottamasamkāśam*, *śailotkarasamakīrṇam*, *śailārkahimasamkāśam*, *kailāśasīkharākāram*, *taṃ śailottamasamkāśam* (Vaidya 1969: 239).

I've translated the apparent sense of the Sanskrit, into good English. There is perhaps an argument for translating the apparent sense into slightly faulty English, but I've resisted that temptation, because it is difficult to assess exactly how faulty the Sanskrit is on this occasion, and because there is no equivalent, in English prose, of an intraverbal syllable added *metri causa*. In any case, there's no need for an emendation here, because a sensible translation is possible without it. Even though *utkarima* is a *hapax legomenon*, its translation, in context, is straightforward. The single syllable *im* has its own entry in the Monier-Williams dictionary as an interjection (in the *Maitrāyaṇī Saṃhitā*) with no semantic content (Monier-Williams 1899: 167 col. 3).

A similar case appears at 35.60d. Here the compound *niṣprāṇanakarāv* seems to have an extra syllable *na*; I have translated the compound as if it were *niṣprāṇanakarāv*, taking the extra syllable to be there *metri causa*. Variants for this *pāda* (*niṣprāṇanakarāv iha*) are: *niṣprāṇanakarāv ubhau*, which also has the extra *na*, but also *lokān iti punaḥ punaḥ*, *lokān iha punaḥ punaḥ*, *lokān iti punar mune*, *niṣprāṇikaraṇāya vai*, *niṣprāṇakaraṇāya vai*, *niṣprāṇikaraṇāya hi*, and *niṣprāṇikaraṇāya ca*, which do not (Vaidya 1969: 254).

### Strong-Armed Vaiśampāyana

At 100.1a Janamejaya addresses Vaiśampāyana with the vocative *mahābāho*, 'strong-armed'. This is exceptional and anomalous, because that epithet is used as standard for *kṣatriya* addressees, but not for brahmins. Some manuscripts (M1.2 Ds) have the variant *mahābāhoḥ*, which is a genitive that would be construed with the immediately following *kṣṇasya jagatīpateḥ*. This fits much better, but I don't think there are good enough grounds for emendation here. Janamejaya is at liberty to call Vaiśampāyana whatever he likes. Indeed, elsewhere I have suggested that Vaiśampāyana is Parikṣit's eldest son and Janamejaya's elder brother (Brodbeck 2009: 221–57; Brodbeck 2009c), so perhaps the vocative *mahābāho* fits him better than one might initially think.

Similarly odd but unambiguous trivial features are found elsewhere too, and could have been emended but weren't. For example, at 54.9a *saṃtaptā bhāskarajalair* ('burned by the sun's waters') is odd, and although I have translated it, Couture emends *jala* ('waters') to *kara* ('rays'), even though, as he explicitly admits, Vaidya's apparatus registers no variants here (Couture 1991: 215 incl. n. 3).

### The Stealing of the *Pārijāta* Tree

Exactly what happened when Kṛṣṇa took Indra's *pārijāta* tree? When the incident is narrated at Hv 92.62–68, Indra is said to have allowed the tree to be taken without a fight:

*śrutvā tad devarājas tu karma kṛṣṇasya vai tadā |  
anumene mahābhūḥ kṛtaṃ karmeti cābravīt || Harivaṃśa 92.67 ||*

When the strong-armed king of the gods heard what Kṛṣṇa had done he allowed it, saying that what was done was done.

At 93.58 and 97.14 Kṛṣṇa is said to have fought the gods that were guarding the tree, but not Indra. But at Hv 105.10ab Vaiśaṃpāyana in his list of Kṛṣṇa's deeds says that Kṛṣṇa defeated Indra in battle for it (*vāsavaṃ ca raṇe jivā pārijāto hr̥to balāt*), and the Vṛṣṇi stalwart Anādhṛṣṭi states this also, at 109.42–43. There are also allusions in the foregoing books of the *Mahābhārata* that indicate awareness of a fight between Kṛṣṇa and Indra (Austin 2013: 253; Söhnen-Thieme 2009: 359–61). Interpolated *Harivaṃśa* apparatus materials expand the story considerably: appendix passage 29 narrates a battle between Indra and Kṛṣṇa in detail (Austin 2013: 251–52). According to Austin, 'The short and uneventful Critical text of HV 92.63–70 ... seems to have been completely out of step with a far more popular, and not necessarily later, understanding of this adventure centering on conflict with Indra' (p. 254). So within the reconstituted text there are conflicting accounts; and I have left this as it is. How could and why would one emend the discrepancy away? I have added a footnote in the translation to reassure the reader that the potential contradiction is there.

There are many other more minor narrative discrepancies of this kind. Where the text's language is ambiguous I have generally tried to make translation choices that minimise narrative contradiction or confusion, if possible. But where the text is clear, apparent unemended narrative discrepancies still stand. Across the *Mahābhārata* Vaiśaṃpāyana presents Kṛṣṇa career in such a way as to imply – within the text's world, not within the real historical world – multiple versions of different tales.

### Madhu and Pr̥śni (28.36a)

There is a difficulty at 28.36a, within the Yādava genealogy. Here is the verse, together with my translation (in its eventual orthography):

*madhoḥ putrasya jajñe 'tha pr̥śniḥ putro yudhājitaḥ |  
jajñāte tanayau pr̥śneḥ śvaphalkaś citrakas tathā || 28.36 ||*

Vrishni was the son of Madhu's son Yudhājit. And Vrishni had two sons, Shvaphalka and Chitraka.

There are two main problems with the interpretation of this verse: the first word *madhoḥ*, and the name *pr̥śni*.

As the reader approaches the verse, he or she has already heard on two occasions about a character called Vṛṣṇi. The first of these occasions is at 24.3:

*mādryāḥ putrau tu jajñāte śrutau vṛṣṇyandhakāv ubhau |  
jajñāte tanayau vṛṣṇeḥ śvaphalkas citrakas tathā || 24.3 ||*

(Kroṣṭu’s wife) Mādrī’s two sons were named two verses earlier as Yudhājī and Devamīdhuṣa. So in 24.3 Vṛṣṇi is either the son of Yudhājī or another name for Yudhājī himself, depending on whether one interprets the first occurrence of the dual verb-form *jajñāte* as active (‘brought into existence’) or passive (‘were brought into existence’). The two interpretive options are presented in diagrammatical terms in Figure 6.

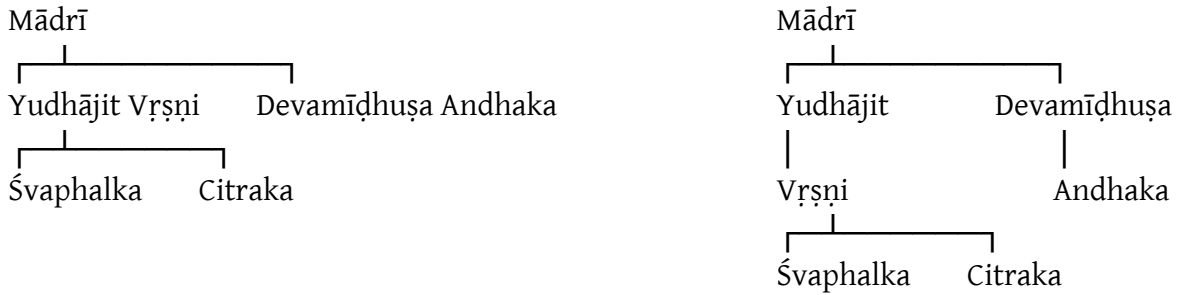


Figure 6. Two Genealogical Options from Mādrī

I have opted for the latter scenario (even though it requires *jajñāte* to be read first as active and then as passive within the same verse), because it is more compatible with the mention of *prśniḥ putro yudhājītaḥ* at 28.36b.

The second occasion on which the reader heard about Vṛṣṇi prior to verse 28.36 was at 27.1–2, where it was stated that Satvat and Kausalyā had four sons, Bhajin Bhajamāna, Devāvṛdha, Andhaka, and Vṛṣṇi, and that their four sets of descendants would now be narrated. The descendants of the first three of these sons have indeed been narrated (Bhajin Bhajamāna’s at 27.3–5, Devāvṛdha’s at 27.6–15, and Andhaka’s at 27.16–28.8), but as the reader approaches 28.36 the descendants of Vṛṣṇi are still pending. In the meantime there has been a repeat mention of Kroṣṭu’s two wives Gāndhārī and Mādrī (28.9–10, roughly duplicating 24.1), mentioning again that Yudhājī was Mādrī’s son, but without mentioning (*his son*) Vṛṣṇi.

When the reader now encounters verse 28.36, it presents Śvaphalka and Citraka (already known as sons of Vṛṣṇi) as the two sons of Prśni, and it presents Prśni as the son of Yudhājī (already known as father of Vṛṣṇi). So it very much looks like Prśni should be Vṛṣṇi. There is perhaps a reason for the doubling of the character here, because it seems that Vṛṣṇi/Prśni, the father of Śvaphalka and Citraka, was both the son of Yudhājī (at 24.1–3) and the son of Satvat and Kausalyā (at 27.1–2). But even if that might serve as an explanation for the two similar names, there is no solving the problem: because both Vṛṣṇi and Prśni have the same descendants, they must be the same person. Accordingly, in my

translation I have taken *pr̥śni* as an alternative spelling of *vṛṣṇi* and presented them in the translation as one name, ‘Vrishni’ (see Table 2 below, p. 143).

Having dealt with the variant name, what is the reader then to make of the claim (in 28.36a) that Vṛṣṇi, the father of Śvaphalka and Citraka, was the son of *Madhu’s son* Yudhājit? We know Vṛṣṇi either as the son of Kroṣṭu and Mādrī’s son Yudhājit (at 24.3), or as the son of (Madhu’s son) Satvat and Kausalyā (at 27.1–2). In the second instance, the only way that Vṛṣṇi could be the son of Madhu’s son Yudhājit would be if Madhu’s son, who is already seemingly called both Purutvat (at 26.26) and Satvat (at 27.1), is also called Yudhājit. That is possible. But to some readers it may seem that when Yudhājit is identified at 28.36a as the son of Madhu, he should rather be identified as the son of Mādrī. Accordingly, I have been tempted to emend *madhoḥ* to *mādryāḥ* at the start of the line. Vaidya’s apparatus here reads as follows: ‘K1.4 M4 om. 36–44. D3 om. 36<sup>ab</sup>. – <sup>a</sup>) B3 Ds1 T1 *mādryāḥ*; Dn *mādrī-* (for *madhoḥ*)’ (Vaidya 1969: 201). So such an emendation would not be an innovation; the idea that Yudhājit is here the son of Mādrī is formulated in two different ways within the manuscript tradition, while the omission of the line in some versions may indicate a distaste for the text that Vaidya reconstitutes.

However, from a broader perspective it should be noted that exactly the same section of genealogy – the segment descending from Vṛṣṇi/Pr̥śni – is presented twice, first within Kṛṣṇa’s paternal lineage (as descending from Mādrī’s son Yudhājit), and then within Kṛṣṇa’s maternal lineage (as descending from Madhu’s son Purutvat/Satvat; Brinkhaus 2005: 377–83). In these terms the line 28.36ab, which provides an impossible composite description of Vṛṣṇi/Pr̥śni that includes elements from both contexts, could have been designed in order to make this bizarre circumstance explicit. And so I have left it as it is. The problem that the reader comes up against here is one that the author/s must be aware of, and that cannot be solved by replacing *madhoḥ* with *mādryāḥ* at the start of the line.

#### The Vṛṣṇi Divisions (81.96–104)

In this passage Vaiśampāyana, describing the siege of Mathurā, gives details of how the Vṛṣṇi army split into different divisions to attack different parts of Jarāsaṃdha’s army.

First one Vṛṣṇi division is described:

*tataḥ śinir anādhṛṣṭir babhrur vipṛthur āhukaḥ |*  
*baladevaṃ puraskṛtya sainyaśyārdhena daṃśitāḥ || 81.96 ||*  
*dakṣiṇaṃ pakṣam āseduḥ śatrusainyasya bhārata |*  
*pālitaṃ cedirājena jarāsaṃdhena cābhibho || 81.97 ||*  
*udīcyaiś ca mahāvīryaiḥ śalyasālvādibhir nṛpaiḥ |*

<sup>96</sup> Then, with Baladeva leading them, Śini, Anādhṛṣṭi, Babhru, Vipṛthu, and Āhuka’s son [Ugrasena], armed with half the army, <sup>97</sup> attacked the right-hand flank of the enemy army, mighty Bhārata, which was protected by Jarāsaṃdha

and the king of Cedi, <sup>98</sup> and by the powerful kings from the north – Śalya, Śālva, and so on.

So this division is led by Kṛṣṇa's brother Baladeva and consists of half of the Vṛṣṇi army, and it attacks the right-hand flank of the enemy army.

Then another Vṛṣṇi division is described. It is led by Kṛṣṇa and consists of half of the Vṛṣṇi army:

*srjantaḥ śaravarṣāṇi samabhityaktajīvitāḥ* || 81.98cd ||  
*āgāvahaḥ pṛthuḥ kahvaḥ śatadyumno vidūrathaḥ* |  
*hr̥ṣīkeśaṃ puraskṛtya sainyasyārdhena daṃśitāḥ* || 81.99 ||

[At the same time,] firing downpours of arrows and setting no store whatsoever by their lives, <sup>99</sup> Āgāvaha, Pṛthu, Kahva, Śatadyumna, and Vidūratha, with Hṛṣīkeśa leading them, and armed with half the army ...

Verse 81.99 is structured like verse 81.96: the first line names the main Vṛṣṇis in the division, and the second line names the leader of the division and says it included half of the army. Here are the two second lines:

*baladevaṃ puraskṛtya sainyasyārdhena daṃśitāḥ* || 81.96cd || ...  
*hr̥ṣīkeśaṃ puraskṛtya sainyasyārdhena daṃśitāḥ* || 81.99cd || ...

So at this point, by analogy, the reader might expect the next verse, 81.100, to begin in a similar way to 81.97, and thus to specify that this division attacked the left-hand flank of the enemy army. But instead, 81.100 is weird; it has no verb, and it centres upon an isolated genitive, *abhiguptasya*, which seemingly has nothing to qualify.

*bhīṣmakaṇābhiguptasya rukmiṇā ca mahātmanā* |  
*prācyaiś ca dākṣiṇātyaiś ca guptavīryabalānvitaiḥ* || 81.100 ||

... <sup>100</sup> [attacked the part of the enemy army that was] protected by Bhīṣmaka and the illustrious Rukmin, and by the eastern and southern [kings], who were braver and stronger than they looked.

As my translation indicates, *abhiguptasya* must be understood in an implicitly accusative sense, with the verb (*āseduḥ*) supplied from 81.97; thus the second Vṛṣṇi division attacked a part of the enemy army that was protected by Bhīṣmaka and Rukmin and company. Although the syntax is very odd, I don't think the text needs to be emended to yield this sense (what other sense could it have?); but this sense is given more straightforwardly by the variant in manuscripts Ś1 K3 Dn D4, which reads *abhiguptaṃ tad* instead of *abhiguptasya*, and by the variant in manuscripts D6 and M1, which reads *abhiguptaṃ ca* (Vaidya 1969: 524).

Having attained this understanding, we still have some questions. Is the part of the enemy army that is protected by Bhīṣmaka and Rukmin and company the left-hand flank? If so, why isn't this stated? But if not, what will happen to that left-hand flank? It seems that no one is left to attack it. Baladeva and Kṛṣṇa form a pair with no obvious third, and two halves of the Vṛṣṇi army have already been deployed.

But beginning in 81.102, there is a third Vṛṣṇi division. It contains nine named warriors, 'surrounded by a large force' (*balena mahatā vṛtāḥ*). And they attack the left-hand flank of the enemy army.

*sātyakiś citrakaḥ śyāmo yuyudhānaś ca vīryavān |*  
*rājādhidevo mṛduraḥ śvaphalkaś ca mahābalaḥ || 81.102 ||*  
*satrājic ca prasenaś ca balena mahatā vṛtāḥ |*  
*vyūhasya pakṣaṃ te savyaṃ pratiyur dviṣatāṃ mṛdhe || 81.103 ||*  
*vyūhasyārdhaṃ samāsedur mṛdureṇābhirakṣitam |*  
*rājabhiś cāpi bahubhir veṇudārimukhaiḥ saha || 81.104 ||*

<sup>102</sup> [Meanwhile,] Citraka, Śyāma, manly Yuyudhāna Sātyaki, Rājādhideva, Mṛdura, mighty Śvaphalka, <sup>103</sup> Satrājit, and Prasena, surrounded by a large force, proceeded in battle against the left-hand flank of the enemy array. <sup>104</sup> With Mṛdura [leading them], they attacked the half of the [enemy] army that was jointly protected by many kings – Veṇudāri and so on.

I think there is a joke here. The expectation is set up for there to be just two Vṛṣṇi divisions, but that expectation is then dismantled, and the bizarre *abhiguptasya* at 81.100a means that the expectation is dismantled gradually, so the riddle is not fully answered until the left-hand flank is explicitly mentioned at 81.103c. An arithmetical excess remains: two halves plus a large force is greater than one, so the Vṛṣṇis exceed the sum of their parts.

That the riddle is deliberate seems to be confirmed in the chapter's last verse, 81.104, in two different ways. Firstly, by the reappearance of the word *ardha*, 'half' (*vyūhasyārdhaṃ samāsedur*). The text specifies that the left-hand flank comprised *half* of the enemy array, which seems odd since we have already heard about the right-hand flank, and about another part which is apparently neither flank. By implication the right-hand flank was smaller than the left-hand flank; but there is arithmetical puzzle in both armies.

Also recurring in this final verse is the kind of grammatical incongruity that the word *abhiguptasya* embodied. At first glance, *abhirakṣitam* in 81.104b (*mṛdureṇābhirakṣitam*) would seem to go with the immediately preceding instrumental, *mṛdureṇa*, and so the half of Jarāsaṃdha's army that the third Vṛṣṇi division attacked would be protected by Mṛdura. Thus the translation might be: 'They attacked the half of the [enemy] army that was protected by Mṛdura, and also jointly by many [other] kings, Veṇudāri and so on.' But we know from 81.102c that Mṛdura was *in* that third Vṛṣṇi division! There he is, listed between Rājādhideva and mighty Śvaphalka. No Mṛdura on Jarāsaṃdha's side is mentioned elsewhere, and it seems unlikely that two different Mṛduras would be mentioned two

verses apart.<sup>34</sup> Rather, this Mṛdura must be Mṛdura the Vṛṣṇi, who is mentioned also at 24.9 and 28.39. Thus *mṛdureṇa* in 81.104b must qualify the verb *samāsedur* directly, with leadership implied, by analogy with the two other divisions, both of which had named leaders. And then the *saha* at the end of the next line cannot be translated ‘together with’, linking Mṛdura with those other kings. It must describe those kings directly – I’ve translated it ‘jointly’. The syntax is thus strange; but if there is just one Mṛdura, the verse must be construed creatively. So in conjunction with the word *ardha*, this verse repeats the kinds of tricks played earlier in the passage – thus suggesting that they are deliberate, rather than accidents of transmission.

This example shows that the author is playing amusing and sophisticated games with the audience. So it would seem presumptuous to smooth this passage out by emending the strange *abhiḡuptasya*, or the last verse, or by translating *ardha* as ‘part’ instead of ‘half’.

## VAIDYA’S ASTERISKS AND PARVANS

On two occasions, after 113.81 and after 118.42, both Vaidya and Dandekar print a line of asterisks to suggest a break in the text. On the second of these occasions Vaidya adds a note saying that ‘Stanzas 43 to 49 of the last adhy. contain the *phalaśruti* of HV. The author of this *phalaśruti* is either Sūta or at the most Vaiśaṃpāyana’ (Vaidya 1969: 798). But Vaidya says nothing to suggest that either of these two lines of asterisks make a division that is also made in the manuscripts. It looks to me that they are Vaidya’s own interpretive paratextual additions, and as such I have felt at liberty to ignore them when translating, and to omit them from my electronic Sanskrit text. Since they don’t really form part of Vaidya’s text, I feel entitled to omit them without classing this as an emendation. They are also omitted from the Dubrovnik electronic text (Schreiner et al. 2005).

The paratextual details that Vaidya provides are intended to affect the text’s reception and interpretation (Genette 1997: 294–318; cf. Cerquiglini 1999: 13–32), but the rationale for them is much less explicit than the rationale for the constitution of the text. Also in the category of Vaidya’s paratextual details are the names and the dimensions of the *Harivaṃśa*’s individual *parvans* (i.e., *Harivaṃśaparvan*, chapters 1–45; *Viṣṇuparvan*, chapters 46–113; *Bhaviṣyaparvan*, chapters 114–18). I call these paratextual because the text itself does not alert us to the beginnings and endings of these *parvans*. In contrast, there are indications within the text of how it thinks of its division into *adhyāyas*: although the word *adhyāya* is not used, there are end-of-chapter markers which speak of the chapter just completed in terms of its title and its effects. These markers are most common within the

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<sup>34</sup> Nonetheless, this is how Dutt’s translation understands the situation: ‘On that battle encircled by a huge army Salyaki, Chitraka, Shyāma, the energetic Yuyudhāna, Rajādhideva, Mridara, the mighty car-warriors Swaphalka, Prasena, and Satrajit, attacked the left flank of the enemy’s army. They began to fight there attacking half of the enemy’s army led by Mridara and assisted by the highly powerful western kings headed by Venudari and the sons of Dhritarāshtra’ (Dutt 1897: 391). It is clear from earlier in the same chapter that Veṇudāri is on Jarāsaṃdha’s side.

cosmogonical and genealogical chapters (see 1.40; 2.56; 3.112; 7.46, 49; 8.48; 10.80; 20.47–48; 21.37; 22.45; 23.165–66; 24.35; 25.17; 26.28; 27.31; 31.152–53; 111.11; 118.44; cf. 4.23–26; 19.30–33; 23.163, 168; 113.79–80, 82; 118.43–49). For these chapters, they effectively fix the chapter divisions in a particular place. However, when it comes to the larger divisions of the text, there are not many clues within the text. There is mention of a *māthura kalpa* ('tale of Mathurā') at 31.143 and 113.74, but without any indication of where it would start and end. There is also mention of an *āścaryaparva* at 113.82, with *phalaśruti*, and this seems to be the end of that unit. Its beginning isn't so well defined, but it could presumably be at around 30.56–57, where Janamejaya uses the word *āścarya* four times in three verses (Brinkhaus 2002: 162–63). But this is the only *parvan* of the text that is mentioned within the text. Vaidya says that *Āścaryaparvan* is an alternative title for the *Harivaṃśa* as a whole (1969: xxvii–xxviii), but that doesn't square with the title's usage within the text.

The manuscripts give indications of how the scribe thinks of the text's divisions, since they often include colophons at the end of *adhyāyas*, which may provide a name for the *adhyāya*, and/or for the larger section in which it occurs, and/or for the *parvan* in which it occurs. However, the details given in these colophons – which are reproduced by Vaidya in his apparatus at the end of each *adhyāya* – often vary widely from manuscript to manuscript, and they are not part of the text itself; rather, they are aspects of its filing and storage systems. Brockington notes that 'these manuscript colophons are undoubtedly later than anything included in the text by a considerable period' (Brockington 2010b: 77; cf. Brodbeck 2016: 396–97).

Vaidya discusses his division of the text into three *parvans*. It seems that he decided to follow the printed editions as regards the number and names of the *parvans* and the locations of their beginnings and endings. His only innovation in this regard is to number the *adhyāyas* continuously even across *parvan* divisions, a decision he defends with reference to the manuscripts he surveyed (Vaidya 1969: ix; cf. Brinkhaus 2002: 158). Though Vaidya doesn't mention this, continuous *adhyāya* numbering across *upaparvan* boundaries is also the practice elsewhere in the *Mahābhārata* critical edition. It is incorrect to say that Vaidya's edition performs a 'radical elimination of any division into *parvans*' (Brinkhaus 2002: 164), since the only thing eliminated is the rebooting of the *adhyāya* numeral back to 1 at the start of every *parvan*.

The case of the *Bhaviṣyaparvan* is comparatively unproblematic, since this *parvan* is marked off by its taking place outside the dialogue between Janamejaya and Vaiśampāyana; but the location of the division between the first two *parvans* is less obvious, as is the name of the second *parvan*. Here the colophon details (Brinkhaus 2002: 164–69) and the mention of the *āścaryaparva* at 113.82 seem to provide a significant argument for renaming the second *parvan* the *Āścaryaparvan* and having it begin at Hv 30, particularly as the name *Viṣṇuparvan* is not given within the *Mahābhārata*'s *Parvasaṃgraha* as the names *Harivaṃśaparvan* and *Bhaviṣya(t)parvan* are (Mbh 1.2.69, 233; Brodbeck 2011: 228–29). Couture's translation of 'l'enfance de Krishna' begins with chapter 30 (Couture 1991), and all other things being equal, I would probably have preferred to begin the second *parvan* there. However, I decided to adhere to Vaidya's *parvan* divisions and names, because they



were already traditional when he chose to use them, because consequently not doing so would be liable to cause confusion, and because after all it is his text that I am translating. In literary terms, it is also quite neat that the *Viṣṇu-parvan* effectively begins with Kṛṣṇa's birth.

### Part 3. Translating for the General Public

This part of the report discusses the process of translation. It begins by attempting to explain and justify the decision to aim the translation at a non-specialist audience, the general public. Then it discusses various issues that translating for that particular target audience has thrown up, and explains some of the overarching decisions that have been made as a result. The issues discussed concern the format and orthography of the translation, its style in all manner of senses, the question of whether to leave certain words untranslated, and the particular challenges of Sanskrit names. Finally, a series of specific translation problems are discussed, in textual order.

#### CHOICE OF AUDIENCE

The first decision and basic ground-rule of the Cardiff *Harivaṃśa* project was to aim the translation at the general public. This rule was applied as a matter of principle, regardless of the fact that the only initial publishing arrangement we were able to secure was with an academic publisher. Aiming at the general public was possible and necessary because the *Harivaṃśa* isn't a specialised text; as the last part of the *Mahābhārata*, it is intended for a broad audience. Vyāsa instructs his disciples to teach the *Mahābhārata* to all four social classes (Mbh 12.314.45; Hildebeitel 2001: 294), and the *phalaśrutis* indicate that this includes women (Black 2007: 55–56). Likewise the final verses of the *Harivaṃśa* outline the text's benefits to all social classes, and to women (Hv 118.43–50). The *Harivaṃśa* is, or should be, a classic of world literature, especially in its restored form in Vaidya's edition (Vaidya 1969; Dandekar 1976), and producing an accessible translation is thus an obvious thing to do. Why would the translator not play his or her part in trying to give the *Harivaṃśa* as wide an audience as possible? If the basic function of translation is to allow a text to have an audience that previously a language barrier prevented it from having, then the more complete that new audience is, the more completely the function has been discharged. Thus the *dharma* of the translator may very often differ – and certainly does in the case of the *Harivaṃśa* – from the habitual *dharma* of the producer of academic books and papers.

Trovato, discussing the function of textual criticism, emphasises that the restoration of a text in its oldest available form goes hand in hand with the act of making that form available to the present-day public; and he musters a series of quotations to show that this public-service aspect of the philological enterprise has been and still is widely acknowledged and invoked (Trovato 2017: 165–66). Davison says that 'the editor's very justification is that he acts in response to the needs, general and scholarly, of his own society, bringing his author's work before the editor's society so that it may be enabled to respond to it' (Davison 1972: 25). In terms of public service, translation is a necessary final aspect of the same enterprise, since without it the text has only been reconceived, not yet reborn (for the metaphor of translation as rebirth, see Devy 1999: 187), and the service provided by the editor has only been provided to the subsection of the public who can

access the text in its original language. In one of Trovato's quotations, Leonardi says the desideratum is 'a text which ... is not merely accessible to specialists, but recovers a work of the past for a contemporary public to read' (trans. Trovato 2017: 166). By implication, the exclusion of any subset of the public would be undesirable, as it would violate the availability principle.

Goethe wrote in his autobiography that 'If you want to influence the masses, a simple translation is always best. Critical translations vying with the original really are of use only for conversations the learned conduct among themselves' (trans. Lefevere 1990: 17). Whilst 'influencing the masses' might be slightly hopeful in the case of this *Harivaṃśa* translation project, nonetheless the insight that simplicity will facilitate accessibility is a very useful one.

The decision to translate for the general public has dictated most of my translation strategies. The basic kinds of strategy that a translator can adopt when tackling this specific type of material are briefly and nicely presented in the introduction to John Brockington's paper 'Translating the Sanskrit Epics' (Brockington 2002: 97–99). My chosen (and some rejected) strategies are unpacked further in the sections and subsections below, with examples.

Aiming at a general audience throws up particular problems. In the first place, it is an unfamiliar situation for me to be in. Usually in my research work I anticipate learned readers, and I try to contribute to the team effort of pushing back the frontiers of knowledge. With the general public and the *Harivaṃśa*, by contrast, my main task will be to keep the reader turning the magnificent pages.

I must obviously aim at accessibility. But how should I know what would be accessible, when I am so unlike the target audience in the crucial respect that I know the text very well and they are almost certainly new to it? The more widely I intend the translation to be received, the harder it is for me to imagine the audience. That is a general problem for this translation.

What to do? Doniger asks, 'if one is going to publish one version of a translation, who is the particular person for whom it is designed?' (Doniger O'Flaherty 1987: 123), and as if by way of a personal answer she says that Betty Radice, erstwhile series editor of Penguin Classics, 'provided, *in her own person*, the ideal audience for a translation from the Sanskrit: an intelligent, educated, intellectually curious person who did not claim to know very much about Indian literature' (pp. 125–26). At times I have been guided, in trying to imagine my intended audience, by imagining an intelligent person whom I know, but who knows very little about the *Harivaṃśa*. In this capacity I have used in my imagination my nephews and nieces, and other more recent arrivals to whom I am related, and those same youngsters in future years, and my friends, parents, and siblings. This is in a desperate attempt somehow to neutralise the haziness of my view of the general public. After all, I hardly feel that I have my finger on the pulse of culture. And in any case, surely the general public and the general culture is mythical. There are various media presenting versions of it, attempting to speak for it, to tell us what it really is, to co-opt us and invest us into some

version of it (as newspaper articles will talk of ‘we’), but those are in the first instance just stories.

But without the mediation, how would one get a view of the general public? Perhaps by staying on the move and going to lots of places and trying to meet lots of different kinds of people. That’s not my job. Without that, one thinks of public events of one kind or another that draw crowds; but then most of the participants are self-selected. To meet a wider general public perhaps there has to be calamity, unexpected events, disorder, something to force people off their usual comfy paths; and even then, you’d probably only meet the relatively locals. As far as the nation or the internation is concerned, the media versions of public reality are inevitably compelling. I am immeasurably grateful for gossip about the existence, excellence, and interpretation of individual works of art, but at the same time I am sceptical (and hence also ignorant) of many aspects of the press and television media presentation, because the vested interests it serves, in terms of the mobilisation of capital, are not necessarily my chosen own.

I want my fellow people to be able to access the *Harivaṁśa*, and so I do honestly make the translation for the venerable General P. But I realise that I haven’t done my market research, and that my attempt will thus necessarily be hit and miss. At the end of the day, whether framed or not in terms of translation strategies, all I have been able to do by way of solving translation problems is to put something, try to make it accessible, and move on. When Davison says of editing and bibliography that ‘It is simply not possible to wait until one has all the information before one comes to a conclusion’, he co-opts and quotes Heisenberg’s more general judgement that ‘The decision itself is necessary’ (Davison 1972: 25); and Heisenberg’s and Davison’s words apply no less to translation. This is the faith and faithfulness of the translator. To this degree, all the reflections below are necessarily sketchy. They emerge from hurried attempts to standardise, and make suitable for reading swiftly, earlier drafts of the translation that were completed slowly.

As far as imagined readers are concerned, what can one do? Aside from people I personally know, there are other demographic subsections of the general public who might have some particular reason (other than knowing me) to be more interested in the *Harivaṁśa* than in some other things. I list six categories here by way of an experiment. These six include large numbers of people whom I hope will be interested in this *Harivaṁśa*.

- People with Indian family heritage
- Kṛṣṇa-*bhaktas* (in the wake of Swami Prabhupāda and ISKCON)
- People who do yoga
- People who read fantasy books
- People interested in ancient wisdom or literature
- Storytellers

The list could no doubt be extended, depending on one’s imagination.

Imagined readers can sometimes help in the making of translation decisions. But also I am wary of imagining subsections of the general audience too clearly or persistently, for fear of reducing the audience's generality. The audience, beyond the people I know, is unknowable and unimaginable, and that follows from the principle of general accessibility. This is to turn a translation problem into a privilege. I envisage the translation being read aloud, and so I won't even have its audience reduced to people who can read. Comprehension of English, spoken or written, is the only barrier. As Sattar remarks of the *Harivaṃśa*'s receiver, 'surely s/he will be compelled by the urgent pace of the *Harivaṃsha* and the sense of Krishna's manifestly extraordinary destiny' (Sattar 2016).

Yet as intimated above, when translating I will inevitably always put only what I think is best. Because however well any of my hypothetical imagined readers may or may not resemble the translation's eventual actual readers, I know that as long as I survive, the actual readers will include me, and so the translation is most definitely for me. I am a member of the general public, and indeed I have that characteristic in common with the entire envisaged audience.

As a scholar I also know that if I can make the translation package good enough for scholarly use, there is an additional specialised Indological university audience, and their students. I know that translations from Sanskrit get picked apart by more senior scholars, and get reviewed in scholarly journals from perspectives that are radically tangential to those of the more general intended audiences of the translations. But if I can make something that's good for me, I trust that it will also be, for all its inevitable drawbacks, some good for other scholars and students as well.

In general, as revealed in the literature on translation, any particular translation will lie somewhere on the continuum between the pole of fidelity and the pole of accessibility. The former is the pole of the 'literal' translation, and the latter is the pole of the 'free' translation. In alternative terminology, the former is the pole of the 'exoticising' or 'foreignising' or 'defamiliarising' translation, whereby the reader must meet the text on its own terms, and the latter is the pole of the 'domesticating' translation, whereby the translator has made the text meet the reader on the reader's terms (Schleiermacher 2004 [1813]; Venuti 1998; Brockington 2002: 97–99; Johnson 2005; Williams 2008: 36–37).

The very fact of translation is a domesticating strategy, and if translations are to be divided into domesticating and foreignising translations, one might imagine that a translation aimed at the general public would lie closer to the 'domesticating' end of the spectrum. This certainly seems to have been the case in nineteenth-century France, where, as McGetchin argues, the increasing reluctance of Sanskrit scholars to provide domesticated translations conforming to local literary tastes contributed to the marginalisation of Oriental studies (McGetchin 2003). But I am not sure that this would necessarily be the case today. Tymoczko notes that in the history of translation, 'The ideal of exactitude has been reinforced and supported by Biblical translation, for sacred texts are the paradigmatically fixed texts of a culture' (Tymoczko 1990: 54); and there may consequently be some pressure to translate the *Harivaṃśa* as exactly as possible. As the discussions below will demonstrate, while some of my solutions to the general problems of

translating the *Harivaṃśa* have had the effect of domesticating the text, other solutions have left it decidedly foreign. In general I have tried to keep the translation as close to the original sense as is practicable – ‘translating the Sanskrit texts in such a manner that the renderings would look natural in the recipient languages while, at the same time, they are faithful to their originals’ (Sarma 1992: 284) – while simultaneously providing assistance in the apparatus for readers unfamiliar with the literary and cultural context. My idea of what is ‘practicable’ in the translation depends on the idea of readability, which Nemeč emphasises as a desideratum before expanding as follows:

I would suggest that such readability would be best achieved by renderings of texts that are as faithful to the original as possible, but ones that are presented in an idiomatic, contemporary prose relatively free of technical terms from the original language of the text, leaving it to the notes and front matter to spell out technical details, historical context, and the like.

(Nemeč 2009: 764 n. 8)

I have prioritised the serious reader who will engage with the text deeply enough to develop a general appreciation of its own conventions and agendas.

It is dizzying to be so closely tied to the source text yet also so free in how to speak it. For ‘there is no single right way of translating a text’ (Davies 2004: 165), and when Gumbrecht says, as already quoted, that ‘every editor adopts roles that are close to those of singers, poets, or authors’ (Gumbrecht 2003: 26), his words on editing, as is so often the case, are equally true of translating, if not more so. In her diary, Charlotte Guest, translator of the *Mabinogion*, referred to her translation work as ‘authorship’ (quoted in Davies 2004: 163). Zelechow speaks of the text’s ‘authentic re-authoring’:

Reading, interpreting and translating are creative repetitions in which the results are simultaneously the same as the ‘original’ and also new and different ... Rooted in a specific historical conditionality, the translator must make the leap into a horizon that embodies the reality of transhistorical and transcultural communication.

(Zelechow 1993: 136–37)

This is far more than the translator bargained for. After all, his or her only qualifications are sufficient knowledge of the source and target languages, and willingness to do the deed.

It is also far more than the reader bargained for, since the reader, but for the language deficit, would read the original and cut out the translator altogether. Distrust of and distaste for the translator are available as psychological accompaniments to the reader’s gratitude for the service rendered, and their existence is not wholly to be explained by the oft-cited historical situation regarding the translation of sacred texts (on which see Mason 2005: 26–27; Long 2005: 1–7; Williams 2008: 18, 39). In any case, the translator’s creative intrusion is a significant and non-erasable disruptive phenomenon in

the writing and in the reading, and because of this I have given some thought to ‘the translator’s voice in translated narrative’. As Hermans’s article of that title makes clear (Hermans 2010), the translator’s voice is heard directly in any paratextual materials and indirectly anywhere and everywhere in the text. One way in which the translator’s voice breaks through into my *Harivaṃśa* translation is in my occasional translation of the very common substantive *vīra* or *vīryavat* as ‘our hero’ (this is Satyavat at 9.100, and Kṛṣṇa at 31.145 and 55.9), where the added pronoun refers to the deal between me and the reader – a unspecified deal involving all manner of political and ideological complications, including our common location somewhere far removed from the worlds of the text and its envisaged audiences.

More generally, though, for all that it is impossible for the translator to forget how crucial, privileged, and powerful a position he or she is in, I have tried to think of my responses to the various problems of translation as being dictated largely by my perceptions and imaginings of the material and the intended audience, rather than by anything particular about myself, the middle term in the scenario. When the translator’s priority is to press on through the text, there is a limit to how reflexive or self-reflective he or she can afford to be.

Alan Williams distinguishes six models of translation, which are perhaps possible functions: 1. linguistic/philological, 2. specialist, 3. academic exegetical, 4. literary, 5. religious exegetical, and 6. popular. Of these I have principally followed the literary model, which Williams describes as follows:

4. *Literary*: represents the text for a new audience and cultural context of contemporary readers, yet retaining something of the literary/stylistic qualities of the source text; ranges from the more linguistically rigorous (inclining towards 1 or 2, above) to more popular translations (inclining to 6, below)

(Williams 2011: 430, correcting 5 to 6)

Williams remarks that as a translator he has ‘attempted to ... combine and move between’ these models (*ibid.*), and I have found this to be my experience also. Venuti, using the term ‘literary translation’ in contrast to ‘technical translation’, remarks that ‘A literary translator can ... experiment in ... the development of translation methods, constrained primarily by the current situation in the target-language culture’ (Venuti 1998: 244). Without studying and reflecting on my *Harivaṃśa* translation as a past output (rather than as a work in progress), I would not claim to have contributed to the development of translation methods. As stated above, in process I have simply put what seems best to me.

## REVIEW OF EXISTING TRANSLATIONS

The Cardiff *Harivaṃśa* translation project was conceived partly because we could find no full translation of the *Harivaṃśa* as critically reconstituted by Vaidya. Old translations of

the vulgate were available (Dutt 1897; Bose no date), but they were generally of far less use for the *Harivaṃśa* than Ganguli's translation of the *Mahābhārata* vulgate is for the rest of the *Mahābhārata* (Ganguli 2000 [1883–96]), because of how much dramatically shorter the reconstituted *Harivaṃśa* is than the vulgate.

Near the beginning of the project we became aware of K. P. A. Menon's translation of the reconstituted *Harivaṃśa* (2008). Anecdotal evidence (personal communication from Mislav Ježić, September 2017) suggests that Menon's translation was prompted by his attendance at the Second Dubrovnik International Conference on the Sanskrit Epics and Purāṇas (in 1999) and his discovery of the ongoing European scholarly engagement with the critically edited *Harivaṃśa*, but that he was by then very old, and that he died shortly after dictating his translation to an assistant. His published translation is accompanied, verse by verse, by the Sanskrit text in Devanāgarī script, but there are no notes, and the brief introduction by Rajendra Nath Sharma seems to introduce the vulgate version rather than the critically reconstituted version. In fact Menon's may be the shortest *Harivaṃśa* yet, since he sometimes omits parts of Vaidya's reconstituted text. Without explanation, he omits 1.17–40; chapters 3–6; 10.52c–53f; 11.13ab; 16.31; 21.15; 22.31–45; 23.10–19, 31–168; 31.6a–10b, 95–103; 32.1–21, 29, 33, 37; 34.22; 44.32, 56; 45.6, 22; 51.36–37; 53.11; 77.12; 86.45–80; 92.11; 93.31, 35; chapters 99–100; 107.21; 110.73; 113.23, 51; 115.4, 44; and 116.20.

Menon tries to translate each *pāda* as a separate unit, in the style of Edgerton's translation of the *Bhagavadgītā* (Edgerton 1952); each *pāda* occupies half a line and begins with a capital letter, even if it is not the beginning of a sentence. However, the sentences often fail to make grammatical sense, the English is riddled with typographical errors, and the translation is sometimes simply incomprehensible. When puzzling over Vaidya's Sanskrit I have often referred to Menon, but only occasionally with beneficial effect.

When the Cardiff project was much further advanced, Penguin Books India published a translation of the critically reconstituted *Harivaṃśa* by Bibek Debroy, a prominent Indian economist (Debroy 2016). This translation is presented in paragraphs, following the style of Debroy's translation of the rest of the *Mahābhārata* (Debroy 2010–14), but the paragraphs are often rather long. Debroy's English is much better than Menon's, but he has only intermittent respect for the grammar used in the Sanskrit, often ignoring the case endings and thus distorting the meaning. Sattar, it seems to me, is wrong to say that 'Debroy's translation is the closest that a reader will get to the feel of the Sanskrit text in terms of grammar and syntax' (Sattar 2016). Debroy includes a four-page introduction, a map, and quite a lot of explanatory footnotes, but no chapter titles. When producing my own translation I have sometimes been assisted by Debroy, but more usually I have been dismayed by his elementary mistakes. Both Debroy's translation and Menon's are clearly aimed at an Indian market where certain kinds of useful familiarity can be assumed of the reader.

There are also partial translations of the critically reconstituted *Harivaṃśa*: French translations of Hv 11–19, the *Pitrkalpa* (Saindon 1998, including also interpolations in a smaller font size), and of Hv 30–78, effectively 'l'enfance de Krishna' (even if he is only born in Hv 48; Couture 1991); and a beautifully illustrated English translation of (more or less) Hv



46–78 entitled *Young Krishna* (Hutchins 1980; see his p. 129 for the extent of the text translated). Hutchins cuts corners sometimes, but Couture’s translation is very well done and usefully annotated, and I have used it a good deal where its coverage allows. For the *Pitr̥kalpa*, alongside Saindon 1998 I have also used the comments made on specific verses by Rocher in his review (Rocher 2000: 458–59).

There are also hosts of other English translations of shorter passages of the reconstituted text that appear in a variety of places framed by a variety of commentaries and discussions, and that I have looked at to compare them with my own ongoing translation. I list some of these here, beginning with the longest passages: Lorenz 2007 (Hv 51–53, 63); Doniger O’Flaherty 1975: 206–13 (Hv 47–48); Doniger O’Flaherty 1984: 66–67 (extracts from Hv 107–08); Hardy 1983: 68–73 (Hv 1.1–13; 63.5–6, 12, 15–35); Coburn 1988: 276–78 (Hv 47.38–57); Matchett 1996 (Hv 1.1–5; 30.56–57; 70.36–38); Hildebeitel 2011c: 286–87, 579–83 (Hv 115.11, 22–25, 39–45; 117.14, 28–30; 118.12–15); Couture 2017b: 98–105 (Hv 31.13; 55.26, 54, 56; 68.28–29; 76.25; 78.33–34); Matchett 1986: 117–19 (Hv 51.2–5; 56.15, 25); Austin 2014: 36–37 (Hv 87.33–39); Austin 2012: 163 (Hv 78.33–38); Coleman 2010: 388–92 (various verses from Hv 63); Viethsen 2009 (various important verses from Hv 41–44); Vielle in press (Hv 23.62–68); Brinkhaus 2009: 2–5 (Hv 8.6–7; 10.80; 11.1; 13.64–65, 70); Brinkhaus 2002: 161–63 (Hv 30.56–57; 113.82–84; 114.1, 18; 115.1); Söhnen-Thieme 2005 (Hv 1.4–5, 10.80, and various verses from Hv 11–19); Tsuchida 2009: 12 (Hv 115.39–41); Tsuchida 2010: 9, 11 (Hv 115.40; 116.13); Schreiner 2005: 330, 342 (Hv 65.38; 86.24); Ueki 2008: 1060 (Hv 23.121; 114.2); Hawley 1979: 209 (Hv 55.54–55); Brinkhaus 2005: 374 (Hv 23.164); Söhnen-Thieme 2009: 356 (Hv 62.44); Couture 2017c: 737–38 (Hv 10.48); Couture 2006: 74 (Hv 45.30); Karttunen 2015: 39 (Hv 105.19); and Schmid 2010: 740 (Hv 30.15). I have also consulted the many short translated extracts from various parts of the critically reconstituted text that are to be found in Matchett 2001 (esp. chapter 3), and in the articles collected in Couture 2015 and 2017 (many of which are newly translated from the French).

## APPARATUS

The Cardiff translation is to be published as a freestanding book, and the apparatus surrounding the translation is designed specifically to complement the translation and facilitate its mediation to the target audience. It is important not to underestimate the role that the apparatus plays here, and accordingly the potential benefits of devising it with care. It includes all paratextual aspects of the book. In this section I discuss the aspects of the apparatus that are arranged around the text, and in the next section I discuss the aspects that are, as it were, within the text, as conventional aspects of the way it is presented. Bailey comments as follows:

Western and contemporary Indian scholars have attempted consciously to solve the contextual problem by providing an introduction and notes to their translations. Inevitably this removes the context from the translation itself by

setting them up as two separate scholarly endeavours operating within different discursive spaces, even where the relationship between the two is intended to be a symbiotic one.

(Bailey 2001: 189)

This cannot really be helped. It follows from what a text is that it is differentiated from any paratext that is built upon it, and I think that readers will readily understand this, even if some sleight of hand may be necessary on my part, as both translator of the text and author of the paratext.

### Introduction

The introduction is as short as possible. Throughout the introduction the focus will be on efficiently providing a general reader who has no previous knowledge of classical Indian culture with the minimum that he or she might require in order to begin engaging meaningfully with the translation.

The introduction situates the text in its historical, religious, and mythological contexts, and in its literary context as the *Mahābhārata*'s final part, and it gives brief suggestions for further reading. Since the *Harivaṃśa* assumes familiarity with the 18 preceding books of the *Mahābhārata*, a narrative summary of those books is provided. The summary highlights in particular those sections which involve Kṛṣṇa and the Yādava-Vṛṣṇis, since by and large those are the sections of Kṛṣṇa's biography that are not narrated in the *Harivaṃśa*, and accordingly they complement the *Harivaṃśa* in quite an intimate way. A narrative summary of the *Harivaṃśa* follows, so that readers may negotiate their own way through the translation.

The *Harivaṃśa* summary is placed all together at the beginning, rather than section by section along the way (as is the case with the University of Chicago Press *Mahābhārata* translation), as this makes it more useful as an integrated reference tool, and allows it to vary its pace more easily. It also allows the actual translation to run along nicely from section to section without interruption. However, my apparatus has nothing to compare with the fine-grained summaries provided in the Chicago translation (and in John Smith's volume, for the sections he doesn't translate). In this regard my translation relies quite heavily, for general orientation and access, on the chapter titles that are presented throughout at the start of each chapter, and *en bloc* in the table of contents at the start. These chapter titles are paratextual in that they are not part of the Sanskrit text translated, except in those cases (mentioned above, pp. 102–03) where an end-of-chapter marker and *phalaśruti* includes the title of the completed chapter. In such cases I have adopted that title and presented it in the table of contents and at the start of the chapter as well. In other cases, where there is no such end-of-chapter marker within the text, I have usually tried to adopt one of the most common names for the chapter as found within the various manuscript colophons, though these sometimes tend to privilege the events of the latter part of the chapter, and in some cases I have ignored them and simply made up a more suitable chapter title myself.

In view of the slightly forbidding nature of some of the material in the *Harivaṃśaparvan*, the introduction suggests that some readers might wish to begin at Hv 30 or Hv 46. The accessibility of the early chapters of the text to the general reader expecting stories of Kṛṣṇa has been a concern throughout the project, and as mentioned above (pp. 71–72), at an early stage some thought was given to producing an abridged translation, but it was decided that providing the whole text, and making the most of signposting, would be best. The signposting in the Debroy and Menon translations is woeful.

#### Footnotes

It would presumably be a mistake to assume that readers will read the introduction. Speaking personally, sometimes I read introductions first, sometimes afterwards, and sometimes not at all. The footnotes will thus be the most accessible aspect of the apparatus, and will be devised accordingly. They will be minimal, but will help the reader cope by explaining allusions, giving background information, giving cross-references to passages elsewhere in the *Harivaṃśa* or the *Mahābhārata* (there are a lot of the latter), and so on.

#### List of Untranslated Words

Initially I thought I would include a glossary list of untranslated words in the end matter, but in the end I put it at the end of the introduction, within a paragraph, and in a separate paragraph I listed and translated the various names of the characters for whom more than one name is used in the translation. For a full explanation of my policy with names, see further below, pp. 135–44.

#### Family Trees

I've included a series of family trees at the end, before the index. They are quite detailed, but even if most readers may not be interested in the finer details, the genealogical matter is a very important part of the text, and merits proper presentation. The genealogical material in the *Harivaṃśa* is gathered somewhat in certain places, but nonetheless it is scattered around the text, and the accounts keep doubling back on themselves to add other 'branches'; and so the diagrammatical images, which collect the genealogical data nicely into fewer than a dozen chunks, most of which fit on a single page, can serve as a useful reference tool for the reader during the read, making the narrative presentation of the genealogical material more accessible by placing it in its wider genealogical context.

In presenting the marvellous Yādava genealogy I have been helped by Brinkhaus's distinction between Kṛṣṇa's paternal and maternal ancestry (Brinkhaus 2005: 375–79). When he makes this distinction Brinkhaus draws attention to the far greater length of Kṛṣṇa's maternal lineage from Yadu. This, one could imagine (within the text), might perhaps be due to some kind of theoretical lunar-lineage initiation procedure, whereby a clan can become Yādava by adopting a standard genealogy: Yadu, 14 kings, specific founding ancestor (in this case Jyāmagha), 14 kings, then living memory (Hv 26).

### Index of Names

The index is comprehensive. It is keyed to verses (see below on the decision to include verse numbers within the translated text), which makes it much more useful than it would be if it were keyed to pages, since by being more precise it means the index can be used very quickly. It also means that the index pertains (at least partly) to Vaidya's text rather than just to this translation. It is hard to imagine more than a few of the various ways in which the reader might use the index, but it is certainly a potentially useful resource. In providing end matter to the translation I have reasoned that if it's at the end and it's not too many more pages, it doesn't matter if a lot of readers don't use it: it is still good to have it there in good order for those who might.

### FORMATTING

The matters collected for discussion here concern the manner of presentation of the translation on the page.

### Poetry and Prose

The Sanskrit *Harivaṃśa* is almost entirely in *anuṣṭubh śloka*s. These are formed of lines, where each line has two eight-syllable *pādas* (feet). The standard *śloka* is a verse consisting of two such 16-syllable lines, and thus four such *pādas* (labelled a, b, c, and d for reference), with the 13th to 15th syllables of each line conforming to the pattern short, long, short (Stenzler 1997: 116). There are also a lot of *mahāpañkti* verses of three 16-syllable lines (thus including *pādas* e and f), and there are also 15 verses of just one line (9.72; 21.21; 23.118; 24.2, 34; 27.19, 24, 30; 65.28; 85.4; 97.28; 100.73; 105.8; 112.65; and 114.14). In terms of the recitation of the Sanskrit text, one might wonder what effect the notional separation of numberable *śloka* verses would have. Indeed, in some cases one might find oneself wondering why the end of a verse is said to fall where it does, why a certain line is an ef line and not an ab line. Hopkins suggests that 'Sometimes ... where one or three hemistichs make a stanza, it is merely a matter of editing' (Hopkins 1993 [1901]: 194). But the occasional addition of an extra line would presumably have a rhythmic function in the recitation, as would any pauses made for dramatic or separative purposes as the recitation proceeds. In any case, the basic rhythm is in pairs of driving and rolling 16-syllable lines.

Given the form of the Sanskrit, there are obvious arguments in favour of verse translation. Pollock makes such an argument when he says that 'in Sanskrit poetry, **form is a value in itself**, and translation that fails to communicate this value fails as translation' (Pollock 1996: 123). Similarly, at the end of his chapter on 'Whether a Poem can be Well Translated into Prose', Tytler concludes that 'it is impossible to do complete justice to any species of poetical composition in a prose translation' (Tytler 1907 [1813]: 111); and A. W. Schlegel cites as 'one of the first principles of the art of translation' that 'as far as the nature of a language allows, a poem should be recreated in the same metre' (trans. Lefevere 1990: 25). This seems to be a dogmatic argument from fidelity. But it might also be an

argument from fitness, the suggestion that the rhythm of the original matches the narrative, and the consequent fear that presenting the narrative in English without some analogous rhythmic drive might simply not work, might not be compelling enough to hold the reader or hearer's attention. This kind of failing is identified when Alan Williams, commenting on Nicholson's translation of Rumi's *Masnavi*, says that 'the literal, prose form which Nicholson chose for his translation is too monotonous for most readers to appreciate without recourse to the poetic colors and shifts of the original, which are largely expunged from the translation' (Williams 2008: 21). For this kind of reason, Will Johnson suggested that we produce our Cardiff *Harivaṃśa* translation in verse, and he formulated some proposed principles of versification, and my early drafts of some *adhyāyas* were in verse. But the attempt to produce a verse translation was abandoned, effectively for practical reasons, because I have no previous experience of writing verse and not much experience of reading it in English, whereas I do have previous experience of reading and writing prose. When Pollock explains the choice of the Princeton University Press translation team to translate the *Rāmāyaṇa* into prose (Goldman et al. 1984–2016; Pollock 1996: 124), he mentions similar kinds of consideration: that the translator must think not what the best kind of translation would be, but what the best kind of translation would be for this translator to make (now). He also suggests that many potential readers would be more used to reading prose than poetry, and might be put off by a book of verse (ibid.). Similarly, in John of Trevisa's fourteenth-century *Dialogue between a Lord and a Clerk on Translation*, the lord says that 'commonly prose is more clear than rhyme, more easy and more plain to know and understand' (quoted in Lefevere 1990: 20). I think that's probably right, but the first argument is more pressing in practical terms. I can make good English sentences; that's one of my basic qualifications for making the translation. I am a native English-speaker. But my poetry skills are underdeveloped. Tytler, for all that he thinks poetry should be translated as poetry, admits that 'none but a poet can translate a poet', and that 'in this species of translation [i.e., poetical translation], the possession of a genius akin to that of his author, is more essentially necessary than in any other' (Tytler 1907: 111, 205).

So prose it is. Now, the Sanskrit text doesn't have paragraph breaks. There are breaks at the ends of lines and verses, but after that the next division is into *adhyāyas* (chapters). Accordingly, some translations present every verse as a separate paragraph. This is the method of the Princeton *Rāmāyaṇa* translation (Goldman et al. 1984–2016). But when the first parts of that translation were reprinted by the Clay Sanskrit Library with the Sanskrit on the facing page (Goldman et al. 2005–08), the translation was reformatted into longer paragraphs, paragraphs which serve the contours of the narrative, marking changes of subject, or other junction points. I think the latter method, being customary for English prose, is far more pleasant for the reader, as well as being far more informative. It helps the reader to receive and keep moving through the text. So I have adopted that style. Naturally this doesn't prevent the sentences in the paragraphs from being poetic, and nor does it prevent the paragraphs from having a good deal of internal rhythm. The pace of the narrative is often fast, with short, direct sentences, and elsewhere the translation shares something of the rhythm of the Sanskrit without any special effort from the translator.

There is no doubt that adding paragraph breaks amounts to tampering with the text. As King et al. note, ‘Punctuation, under which we include paragraphing, does not merely affect the semantic continuum; it is part of that continuum’ (King et al. 1969: ix). Davies notes that if paragraph breaks are added, ‘as a result the material is visually dissected, giving the impression of discontinuity’ (Davies 2004: 174). But it is legitimate to add paragraph breaks in a translation. In adding paragraph breaks I have tried to use them fairly freely for the reader’s convenience, as each new one is a potential handle on the text. Ganguli’s *Mahābhārata* translation uses far too few of them, and so does Debroy’s (Ganguli 2000; Debroy 2010–14, 2016). I hope I have not overused them. My paragraph breaks usually come at the end of a verse, but not always.

Aside from the stream of 16-syllable lines, the Sanskrit text sometimes contains extra-metrical tags that say who is speaking the following lines, saying simply ‘So-and-so said (*uvāca*)’. Sometimes these tags may seem slightly redundant, for instance where they appear at the beginning of a chapter but there has been no change of speaker since the last chapter, or where the immediately following speech has already been introduced within the metrical lines, as most of the text’s reported speeches are, without the need for an extra-metrical tag. Nonetheless I have translated these tags every time. They are quite a stark feature of the Sanskrit text, and so I have tried to make them slightly dramatic in the translation, presenting each one as a separate short paragraph, ‘So-and-so said’, ending with a colon.

Among the *Harivaṃśa*’s *ślokas* there are also 26 verses in longer metre (for a list of them, see p. 60 n. 26 above). These long-metre verses each have four *pādas* of from 11 to 14 syllables each. Of the 104 (26 × 4) *pādas* in these verses, 44 *pādas* have 11 syllables, 43 have 12 syllables, 16 have 13 syllables, and one has 14 syllables. In the Poona edition these long-metre verses are printed in four lines rather than in two, with a line break at the end of each *pāda*. The *Harivaṃśa* uses such verses sparingly, but in the *Mahābhārata* they sometimes occur in long passages. For surveys of the non-*anuṣṭubh* portions of the *Mahābhārata* (excluding the *Harivaṃśa*), see Fitzgerald 2005 and 2009; for ‘the epic *triṣṭubh* and its hypermetric varieties’, see Edgerton 1939.

In his *Mahābhārata* translations van Buitenen indented these longer-metre verses and translated them as non-rhyming quatrains (van Buitenen 1973–78). It seems right to mark the differences of Sanskrit metre within the translation somehow. In Fitzgerald’s continuation of the Chicago translation, Fitzgerald modifies van Buitenen’s method slightly. He still indents such verses, but he translates them into prose (Fitzgerald 2004: 651 n. 3). I have followed van Buitenen’s method, but I am mindful of how strange and opaque van Buitenen’s verses can sometimes sound, so I have tried to keep things simple and prioritise comprehensibility.

### Diacritics

Diacritics are a nuisance to the general reader because they make the text look funny and they’re no good unless the reader knows what they mean. They can put readers off. Davies notes that ‘modernizing the orthography creates less distance between the modern reader

and the text ... [R]etaining the orthographical forms implies a scholarly translation, accessible to a learned minority or elite' (Davies 2004: 172). And I know from experience that it's hard to teach people to pronounce words with diacritics correctly. Most translations from Sanskrit that use diacritics in the translation (in names, for example) include at the beginning of the book a 'Sanskrit pronunciation guide' that may be several pages long (see, e.g., van Buitenen 1973: xlv–xlvii; Goldman 1984: xix–xx; Roebuck 2003: ix–xii; Olivelle 2004: xlviii–xlix; Smith 2009: ix–x). I wonder whether these guides are studied and followed by readers who are not Sanskrit students. I suspect that in most cases they are not. When reading silently to oneself, after all, one can read names without pronouncing them, or with erroneous ideas of their pronunciation: I remember being surprised to discover that 'picturesque' and 'grotesque' were not pronounced 'pictureskew' and 'grotskew', and horrified to discover that my habitual private pronunciation of 'lieutenant' was American. With the English language the relationship between spelling and pronunciation is often tenuous, but since this is absolutely not the case with Sanskrit, it is a shame for the printing style on the page to obscure the pronunciation of Sanskrit words or names. In presenting my translation of the *Harivaṃśa* I want readers to be able to read any Sanskrit names out loud correctly. I want the whole translation to be easy for the reader to read out loud off the page as an oral text, and easy for the listener to take in and follow as an aural text. And in fact most of the Sanskrit sounds that are often represented with diacritics (ā, ḍ, ḥ, ī, ṃ, ñ, ṇ, ṅ, ṛ, ṝ, ṣ, ś, ṭ, ū) can be prompted well enough using the standard English alphabet. So by and large I have tried to use whatever combination of letters seems able to convey the required sound. I don't mind if some Indologists think this to be a bit of a liberty, as long as it makes the text more accessible for newcomers. The main remaining problem is then the ambiguity in English of the vowel letters a, i, and u, and so I have used a macron to differentiate the long vowels ā, ī, and ū from their shorter counterparts. I was tempted to eliminate macrons too, and use double-a for ā, double-e for ī, and double-o for ū, but that method, though simple and effective, might have given the translation a rather colonial feel, since it has been obsolete for many decades.

#### Quotation Marks

Most of the *Harivaṃśa* is a dialogue between Vaiśampāyana and Janamejaya, which is relayed by Ugraśravas in his dialogue with Śaunaka; elsewhere I have argued that this latter dialogue is itself relayed within an address by Ugraśravas to a company of unnamed ṛṣis (Brodbeck 2009: 245 n. 40, 251, 260); and within Vaiśampāyana's narration characters make speeches, which may narrate further speeches, and so on. So it is impractical to use quotation marks consistently to mark out the speeches. There would be too many sets of opening quotation marks at the beginning of almost every paragraph.

Some speeches are introduced by one of the aforementioned formulaic tags, saying 'So-and-so said'. The Chicago *Mahābhārata* translation doesn't use quotation marks for speeches that are introduced that way, but for all other speeches, it does. 'Quotation marks surround all text that is to be attributed to a speaker different from the speaker noted by the last prose formula' (Fitzgerald 2004: 656). In the Sanskrit text, speeches at the same

narrative level sometimes have ‘So-and-so said’ tags and sometimes don’t, and so in the Chicago translation, speeches at the same narrative level sometimes have quotation marks and sometimes don’t. This doesn’t really matter, provided it is clear where speeches begin and end. But then, one wonders whether quotation marks are *ever* really necessary in the translation to make it clear where speeches begin and end.

The only written punctuation marks in the Sanskrit are the double *daṇḍa* at the end of each verse, which is often best translated by a full stop, and the single *daṇḍa* at the end of the other lines, which is best translated variously, sometimes not at all. So, much of the punctuation in the translation is based on Sanskrit words. Words like *tatas*, *tadā*, *tu*, and *hi* can often be translated directly as punctuation marks. And there are longer examples. Nine times in the *Harivaṃśa*, this *pāda* occurs: *ity evam ukte vacane* (‘thus, this having been said’; 107.47a, 57a; 108.89a; 109.46a, 57a, 70a, 76a; 110.66a; 111.7e). This end-of-speech marker could just be translated as a closing quotation mark. And there are other formulas that could be similarly translated. So if quotation marks were to be added into the translation, one would probably not want also to translate such formulas as words.

Might as well say that the text doesn’t need quotation marks. I’ve decided not to use them in the translation. I’ve thus been able – facilitated in many cases by adding paragraph breaks (as discussed above) – simply to reflect the ways in which the Sanskrit marks the beginnings and ends of speeches.

On his practice of writing without the use of quotation marks (a tradition that he traces to MacKinlay Kantor), Cormac McCarthy says: ‘You really have to be aware that there are no quotation marks to guide people, and write in such a way that is not confusing about who’s speaking’ (McCarthy 2008). Although they weren’t thinking of doing without quotation marks, the *Harivaṃśa*’s authors almost always wrote in such a way. Occasionally, however, I have not hesitated to add an extra ‘So-and-so said’ or something similar at the beginning or end of a paragraph to show that a speech is starting and, if necessary, to make it clearer whose speech it is. But in such cases the ‘So-and-so said’ is never presented as a stand-alone paragraph in the translation.

### Verse Numbers

Here there are three common presentation methods. One can miss out verse numbers altogether (e.g. Ganguli 2000; Debroy 2010–14, 2016). One can number every verse, either as it starts or as it ends (e.g. Goldman et al. 1984–2016; Menon 2008; Couture 1991; Olivelle 2000, 2004); this is often the general method with sacred texts, for ease of reference, and it is the method of Vaidya’s edition. Or, as a compromise, one can put every fifth verse number in the margin (e.g. van Buitenen 1973–78; Fitzgerald 2004; Smith 2009; Wilmot et al. 2005–09).

It may well be that many readers would not especially care for verse numbers in their text. But I want them in my translation partly so that the index can be keyed to specific verses rather than to specific pages, and more generally just to make it more useful. Because the numbering of the translation mirrors that of Vaidya’s edition which is used by the scholarly community, the value of an index is much greater if it is an index to



the reconstituted text as well as an index to this translation. Keying to verses is also much more specific, and so for readers of the book who use the index or the cross-references in the footnotes, being directed to a specific verse will be more handy than being directed to a specific page. So I think that numbering the verses is better for all readers. What harm can verse numbers do? Perhaps they commodify the text; but if so, it is already commodified. And if they are to be used, I think there is no point in compromise: if they are useful, they are far less useful when only every fifth verse is numbered, and so my translation numbers every verse in superscript type at the start of the verse.

The exceptions are the 29 cases where I have translated two verses together (3.103–04; 6.14–15; 9.98–99; 10.28–29; 13.67–68; 15.61–62, 67–68; 20.31–32, 42–43; 23.30–31; 30.21–22; 34.9–10; 36.54–55; 43.16–17; 48.44–45; 72.6–7; 74.3–4, 14–15; 81.63–64; 84.21–22; 87.26–27; 92.8–9, 25–26; 93.24–25, 36–37; 94.4–5; 101.4–5; 107.63–64; 110.1–2), and the one case where I have translated three verses together (113.62–64), and the one case where I have translated four verses together (87.45–48).

There are also a few places where, without necessarily wishing to emend the Sanskrit text, I have switched the order of two consecutive verses in the translation (95.7–8; 109.14–15; 118.43–44). Sanskrit is somewhat indifferent to word order in comparison with English, and so words in a sentence can be and sometimes have to be switched around as they are translated, and perhaps this doesn't necessarily always just apply to individual words. See also Thomson 1963: 46–47: by noting small corrective changes made within Greek and Latin manuscript transmissions, Thomson documents increasing levels of attention to order of items.

### Square Brackets

Translating from Sanskrit into clear and comprehensible English frequently requires the addition of words that have no direct equivalent in the Sanskrit. This being the case, there is a tradition of philological translation whereby such words as are added during the process of translation are placed within square brackets. This shows that they are additional and thus makes the process of translation more transparent, particularly if the reader is using the translation alongside the original. In a way, it allows some of the more interpretive aspects of the translator's activity to be marked out as such, separating what the text actually says from what the translator thinks it implies. The fewer such square brackets there are, one would think, the more 'literal' the translation would be. However, I think that the presence of such square brackets, which would be quite copious in this *Harivaṃśa* translation, would act as what Fitzgerald calls 'scholastic speed-bumps in the translation' (Fitzgerald 2014: 485), and would distract and confuse readers and interrupt their engagement with the translation. The general reader for whom this translation is devised is not expected to have any knowledge of Sanskrit or any linguistic interest in it, so there is nothing to be gained, and much to be lost, by using such brackets.

I am not sure that such square brackets perform a particularly well-defined function even in translations that feature them. A good deal of the translator's most important and potentially most subjective interpretive activity concerns the continuous choice of which

English word to use in place of which Sanskrit one (many Sanskrit words have several different meanings, and English has a larger and more nuanced vocabulary than Sanskrit). This activity will not be indicated by square brackets and will thus be, as it were, lost in translation. Moreover, many of the additions that would be placed in square brackets will hardly be interpretive at all, instead being simply a function of the formal and stylistic differences between the two languages. So perhaps the principal utility of such square brackets is to assist students by providing two different categories of relationship between a Sanskrit text and a translation thereof: the student then knows not to go looking in the original for equivalents to the English words that are in square brackets. This is a valuable service in that context (as is also, for example, the use of many different kinds of bizarre English phraseology), but it ceases to be appropriate as soon as the translation is aimed at someone who doesn't know or necessarily want to know any Sanskrit. Hence during the process of working on the *Harivaṃśa* translation I have put additional words inside square brackets to indicate *to myself*, as I work and rework the translation, that they have no direct equivalent in the Sanskrit original. This has saved me time when checking and revising my own work. But I removed all the square brackets (but not their contents) before sending the translation to the publisher; and for several years before that, I wistfully looked forward to doing so.

The stage at which I removed all the square brackets was the stage at which I also separated my two-column dual-language working document into two separate documents, an English one to send to the publisher, and a Sanskrit one that would become the appendix to this report. I put this stage off for as long as I could, waiting until I thought the translation was completely finished, because once the two languages are separated into separate documents it becomes harder to assess and improve the translation in light of the original. However, Heisig advises as follows:

I would stress the need to add the stage of radically editing a completed translation for readability. Much translation is not bad because it is inaccurate in a first sense, but because it is incomplete, a first draft that deserves to be poured over and rethought with the same care that a good writer gives his own prose. This is a courtesy to the readers and also, as I have been insisting, a courtesy to the original text.

(Swanson and Heisig 2005: 133)

I edited my translation from beginning to end for readability several times before splitting the working document into two documents. This was after having already gone through the text with close reference to the Sanskrit twice, as described above (p. 74); thus in my case it was a second full draft that I started editing specifically for readability, not a first draft as per Heisig. Having the Sanskrit text there opposite the English made it possible for me in many cases to identify my own faulty understanding of the Sanskrit as the cause of the English readability problem, and thus to correct two problems at once. While making

full-draft edits I also kept a note of passages to revisit, and I revisited them all before beginning the next full-draft edit. For these partial, ‘drop-in’ edits too it was valuable to have the Sanskrit always there alongside. But when I finally (and not finally) split the dual-language document in two and could see the English on its own without the convenience of glancing at the Sanskrit, I started looking at it in a different way, as a piece of writing rather than as a translation, and hence I was able to improve it in ways that hadn’t previously occurred to me. It is possible that the translation would have benefited from more editing at this stage. But I was wary of making too many changes without reference to the Sanskrit, for fear of taking the English too far away from it.

## REGISTER

One tendency, with so-called ‘epic’ texts (on this label see above, pp. 30–38, and below), is to translate into rather grandiose and grandiloquent English, sometimes with an evocative feel that is anachronistic with respect to the reader’s temporal location. Davies speaks of a nineteenth-century taste for ‘deliberately archaic’ translations (Davies 2004: 170). But I have tried to keep the English of the *Harivaṃśa* translation as conversational and contemporary as possible. It is to be read by present-day people, and it is a translation of a conversational text, so even though the presentation is somewhat staged and artificial, a relaxed contemporary feel is appropriate. I’ve used ‘isn’t’ instead of ‘is not’, and so on, and I’ve tried to use words and expressions that are plain and direct. The text is vivid, and it usually doesn’t need beefing up in translation. Nonetheless, despite aiming for an informal feel, I have tried to avoid anachronism and anastrophe with respect to the location of the characters in the text: that is, I have tried not to have them express themselves with reference to cultural items that were only invented recently, or that are foreign to India (for examples of this kind of translational impropriety, see Tytler 1907: 140–43). I have also tried to avoid idioms or colloquialisms whose usage would only be understood by a subsection of the readership; but this is difficult, because I don’t necessarily know which of my own habitual expressions might be obscure to, for example, American readers.

There is a general danger of writing convoluted or dated English which might alienate the reader. In part this danger arises because of how the Sanskrit is used. As Sarma says, ‘special efforts require to be made in matters like phraseology, idioms, syntax and voice construction’ (Sarma 1992: 284; cf. pp. 285–89). For example, in the *Harivaṃśa* there are an awful lot of passive sentences (‘Z was Y-ed by X’), which I have tended to turn into active sentences in the translation (‘X Y-ed Z’), in keeping with Orwell’s fourth rule: ‘Never use the passive where you can use the active’ (Orwell 1962 [1946]: 156). There are other similar traps that must also be avoided if one is to avoid writing what might be called Sanskritic English – or Indologese, which according to Heifetz was ‘developed in the nineteenth century and continues to be observed in most translations of Sanskrit literature into English’ (Heifetz 1990: 15). The more tempting among the possible range of Sanskritisms probably vary from person to person. One particular feature of my own

Sanskritic English that I discovered in a pre-final draft of my translation was an alarming proliferation of semicolons, which I eventually managed to tame.

In part the danger of writing alienating English arises because the Monier-Williams dictionary (Monier-Williams 1899) is over a hundred years old, and so its English vocabulary is dated. And it may not just be dated; perhaps its English register also has a certain amount of academic pomposity inherent to the normal context in which English-speaking people used to and perhaps still do learn Sanskrit. Speaking personally, before this project I have generally presented my translations from the Sanskrit to my academic peers and my teachers, in order – in part – to try to impress them with the range and maturity of my linguistic capacities; and so this translation, aimed as it is primarily at people outside academia (and purely for purposes of communication), is a new and uncertain venture that requires a lot of old habits to be reexamined. In the translation I have taken some measures which I don't think will be popular with my colleagues, but which I think will nonetheless help the text to communicate to its intended audience.

In part the danger of writing alienating English also arises because in English there is an uncertain but available dated generic 'epic' register for texts featuring men doing macho things deeply. That register is dated in terms of the typical age of the texts, and also – more importantly – in terms of the style and idioms of their translation into English. But after reading some of the scholarly literature on translation studies and literary studies, it seems to me that the term 'epic' is used rather freely as a genre term allegedly understood by English readers, and I'm not sure that I really know what it means. This may be simply due to my own impoverished knowledge of literature in English. That failing of mine (quite apart from my rudimentary Sanskrit) should mean someone else would translate the *Harivaṃśa*, not me. Too bad. And if I am lamentably ignorant, some of the translation's readers may be too.

Nonetheless, it is perhaps somehow of the nature of genres that to claim discursive ignorance of exactly what 'epic' style entails in the present day does not mean that as a reader one would not be affected by it, that it would not resonate, compounding the feel and sense of the material through association, slotting into a groove in the brain.

On the one hand, authors write as a function of (which does not mean in accord with) the existing generic system, which they can demonstrate both within the text and outside it, or even, in a way, between the two: on the cover of the book ... On the other hand, readers read as a function of the generic system, with which they are familiar through criticism, school, the distribution system for the book, or simple hearsay; it is not necessary that they be conscious of this system, however.

(Todorov 1976: 163)<sup>35</sup>

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<sup>35</sup> Todorov's authors seem to be responsible for their book-covers as part of their texts.

If it exists in that sense, ‘epic’ (or whatever other) style will have come through me into the translation, whether I like it or not.

In general though, and partly because of this, I have tried to like it not. I don’t want knowingly to try to strike a particularly resonant register in the target culture. This is partly because my attempts to do so would always be compromised by my own misperceptions of and difference from most others in that culture; and partly because whatever that register might be, in reality or in my imagining, it must inevitably be alien to the material translated, introducing unhelpful connotations and overtones and stopping the text speaking for itself; and partly because, from the same perspective of ethical intercultural politics, I distrust the associative resonances of ‘epic’.

Regarding this latter distrust. Lefevere says that in ‘representing the other’ through translation, ‘We shall ... have to learn to skip the leap we often call “of the imagination” but which could be much more aptly called “of imperialism”. The question is whether Western cultures are ready for this’ (Lefevere 1999: 78). Lefevere, writing on eighteenth-century Dutch colonial literature, writes of ‘a time when genres were viewed in a strictly hierarchical order’, and interprets the use of the epic genre (or grid), with its ‘lofty tone and diction’, in terms of the author’s admiration for the work’s subject, and the taking of a stance which would obscure ‘the folly of the colonial endeavour, Dutch or otherwise’ (pp. 93–94). As I have suggested elsewhere in my writings on the *Mahābhārata* (Brodbeck 2009: 259–68; Brodbeck 2011b: 96–97; Brodbeck 2014), I think that Vyāsa’s multivocal text does knowingly represent something akin to ‘the folly of the colonial endeavour’, and I wonder whether its reception in terms of ‘epic’ might have (and might have had) the effect of partially obscuring this aspect.

Nonetheless, Jauss’s comments apply to the Cardiff translation and its receivers:

A literary work, even if it seems new, does not appear as something absolutely new in an informational vacuum, but predisposes its readers to a very definite type of reception by textual strategies, overt and covert signals, familiar characteristics or implicit allusions. It awakens memories of the familiar, stirs particular emotions in the reader and with its ‘beginning’ arouses expectations for the ‘middle and end,’ which can then be continued intact, changed, re-oriented or even ironically fulfilled in the course of reading according to certain rules of the genre or type of text ... The new text evokes for the reader (listener) the horizon of expectations and rules familiar from earlier texts, which are then varied, corrected, changed or just reproduced.

(Jauss 1970: 12–13)

In the language chosen for this translation I thus try to regulate and modulate the text’s reception through my expectations of the reader’s ‘memories of the familiar’, even though this is difficult where those memories are not my own. Elsewhere Pratt takes up Traugott’s borrowed notion of the ‘appropriateness conditions’ of an utterance (or writing):

[A]ppropriateness conditions provide a way of building into the description of an utterance the contextual norms ... so that style in any kind of discourse can be represented as the context-dependent phenomenon it is ... By the same token, many kinds of literary deviance can be described as violations of specific appropriateness conditions.

(Pratt 1977: 87)

In these terms I fear my *Harivaṃśa* could be a literary deviant because of my ignorance of what's appropriate. But that fear is natural, and we press on regardless. Also, and more seriously, I suspect, as intimated above, that the *Harivaṃśa* – especially in English translation – is a literary deviant in generic terms, and that any cosy agreement between translator and reader about what kind of thing it is would misrepresent it. In this sense the *Harivaṃśa* is what Heath calls a 'text' rather than a 'work':

Where a work *resembles*, is readable within genre limits that it follows as a condition of its representation to the reader, the text *differs*, transgresses these limits in order to implicate the reader in a writing that disturbs representations. Where the work is on the side of pleasure, modulating a subject's cultural expectations to fulfilment, the text is on the other, that of *jouissance*, coming off from any stability of self in an abruptness of dispersal, the reader pushed out of genres.

(Heath 2004: 173)

### Vocatives

One particular aspect of the grandiose 'epic' feel in some translations is the handling of the vocatives in the text. Vocatives are frequent in the *Harivaṃśa*. They identify the immediate addressee, which is useful because there are many nested levels of narration. And they identify the addressee in various ways, sometimes using specific personal epithets, sometimes epithets that are tailored to the context with a measure of irony or some other playful or creative flavour (Brockington 2002: 98). Because the vocatives are important to the text, I've translated almost every Sanskrit vocative as an English vocative. I've tried to do this as smoothly as possible, but, for example, some of the nuanced Sanskrit synonyms for 'king' cannot be translated directly without clumsiness, so I've almost always reverted to 'your majesty'. Nonetheless, some clumsiness may remain in some cases. Occasionally, when vocatives are long or complicated, I have translated them not as vocatives but as assertions about the addressee ('You are ...'). I have translated the vocative *tāta*, depending on the context and addressee, as 'sir', 'sire', 'daddy', 'my boy', 'my boys', and, when 'my' doesn't work (because the speaker is plural), as '[insert name] boy'. I can't remember what I thought was wrong with 'our boy'.

One problem here is that vocatives are actually quite common in spoken English, but rare in BBC English or written formal English, and consequently many common English vocatives are in a local or colloquial register of the language, which, apart from anything

else, means that potential readers might be unfamiliar with them. So although the vocatives are appropriate to the relaxed conversations in the Sanskrit text, it is hard to choose reliable words and also strike the right tone in the translation. Another problem is that sometimes the Sanskrit contains far more vocatives than one would tend to have in English. Sometimes, in order to deal with this, I have produced a separate English sentence containing nothing but vocatives. In any case, I have avoided using capital 'O' (or even lower-case 'o') as a vocative marker. In the *Harivaṃśa* there are some formal public speeches where a grandiose register would not be out of place, but even so, the vocative 'O' is something that I have never heard used in such situations in this day and age. It is unnecessary, and sounds parodic.

### Exclamations

Another aspect of the translation's register is supplied by the words that are used to translate the interjections and exclamations *aho*, *bata*, *bhos*, *dhik*, and *hā*. Using different translations for these words can make the mood of the whole passage feel very different. I have varied my choice of English vocabulary for these words depending on the context and (my impression of) the mood of the Sanskrit, using 'ah', 'alas', 'argh', 'bravo', 'crikey', 'damn', 'ha', 'hello', 'here', 'listen', 'no', 'oh no', 'see', 'shame', 'shame on', 'what a shame', 'woe betide', and 'to hell with', often followed by exclamation marks. Similarly sensitive is the phrase *bhadraṃ te* ('please', 'if you please', 'with your blessing', 'bless you', 'good luck').

### Repetitions

Words recur in the Sanskrit text. I think the repetition of words is often part of a deliberate alliterative strategy (which can also affect the choice of other words used in addition), and so in the translation I have often translated the same word in the same way several times in quick succession, where in normal English one might rather vary one's vocabulary and use some synonyms. Where alliteration is produced in the Sanskrit not by repeating words but by choosing words which repeat similar sounds, it is often not possible to replicate the effect in the translation. But on many occasions I have chosen my English vocabulary in order to produce alliterative effects, even at places where there are no such particular effects in the Sanskrit. Thus the translation will reflect the text's method to some degree.

### Roots *Svan*, *Han*, etc.

They say the Eskimos have a lot of different words for snow. It is interesting to notice areas in which the range of Sanskrit vocabulary – at least insofar as it is used by the *Harivaṃśa* – is much more restricted than English. For example, the Sanskrit roots *han* ('smash') and *svan* ('make sound') are used quite a lot, but unless the translation is to be very bland indeed they call for a wide range of translations, because there is a very large range of English verbs to choose from, where the choice might depend, in the case of the root *han*, on who or what is being smashed, and by whom, and with what, and why, and with what effect, and what kind of noise it would make, and in the case of the root *svan* on who or what is making the noise, and what kind of noise it would be. English is very onomatopoeic

in that pretty much any sound that a thing might make can function as a verb. English is quite aural in this respect. I have enlivened the translation and to some degree made it more physical by choosing from the available English resources, which in many cases also complement the text's informal conversational style.

### Excellence Fatigue

The considerations sketched in this section are mostly in the form of general principles, but the most basic principle is to be as sensitive as possible to the intended reader and the likely effect of the translation upon them, not just in terms of what the text says, but also in terms of how it *feels*. The application of that principle leads to other specific strategies in dealing with specific textual circumstances. For example, I have often translated verbs in the present tense in Sanskrit into verbs in the past tense in English, or vice versa, so as not to disrupt the flow of the narrative (see, e.g., Hv 59.40–42 and 45, where the Sanskrit switches into the past tense for no discernable reason; strange switches of case, as e.g. at the end of 36.51, have no equivalent in English).

Nonetheless, although I can adjust the feel of the translation to some degree, there is only so much I can do with what the Sanskrit says. Where the text seems ambiguous, I have made interpretive decisions on the basis of my knowledge of the literary and historical context of the original, rather than in terms of probable accessibility to the reader. Yet the narrative style of the original may seem peculiar to the reader in ways that only an unfaithful translation could remedy. Here I am thinking in particular of the text's tendency to use superlatives very freely. This is perhaps just the most systematic effect of a general tendency towards dramatic exaggeration that is also well established in present-day narrative media in many different cultures: so, for example, the *kṣatriya* heroes of the *Harivaṃśa* are only slightly more able than their counterparts in Hollywood films are to sustain multiple injuries in combat, any one of which would probably have incapacitated a normal human being, and yet fight on manfully. But nonetheless, the composers of the *Harivaṃśa*, when describing a character in terms of his or her general functional type (brahmin, *kṣatriya*, man, wife, son, etc.), are very fond of doing so using *tatpuruṣa* compounds with *vara* or *śreṣṭha* as their final member, with the result that hardly anybody is an ordinary one of anything; and I fear that this will seem a bit odd to some readers. Sometimes I have toned down the superlatives for the sake of logical consistency, translating *-vara* and *-śreṣṭha* as 'superb' rather than 'supreme', since there can in principle only be one 'best of' in any particular category, so we can't very well have two different ones in the same scene. Perhaps the factually assertive sense of superlatives is more pronounced in English than it is in Sanskrit. But even with such an adjustment, there is a risk of what we might call 'excellence fatigue'. So be it. This is part of the text's narrative style, and the reader will have to get used to it – which should not be too difficult, since as mentioned above, similar conventions of hyperreality are well known in many different present-day genres.



## DIFFICULT SANSKRIT WORDS

Some words can be left untranslated, and explained in the apparatus; but overall, the reader wants the translator to translate. If the reader has to learn a new concept that doesn't have an English word for it, this effort must be justified by the effect that concept then has in helping the reader understand the text; and often the effect wouldn't justify the effort. In translating for the general public I have tried to make as few demands of this kind upon the reader as possible, while still translating effectively. I have tried to give the reader as few excuses as possible to give up on trying to read the book.

### *Dharma etc.*

In the literature on translation, frequent mention is made of the 'fixed equivalence' method, whereby the same word in the source language is consistently translated by the same word in the target language, as opposed to the 'dynamic equivalence' method, whereby the same word in the source language is translated by different words in the target language on different occasions, depending on the context (Nida 2004 [1964]). Swanson comes out strongly in favour of the 'dynamic equivalence' method:

*There is no one-to-one correspondence between words of different languages ... [T]he cultural background and historical development of a word gives it connotations beyond the dictionary definitions that can never be exactly replicated in another language ... [T]here is a great danger of misrepresentation if a given word in one language is always translated with the same word in another, a 'foolish consistency' that can only be maintained by disregarding the context. Strict adherence to a 'consistent' translation can lead to what one of my acquaintances has called 'dictionary fundamentalism.'*

(Swanson and Heisig 2005: 116–17)

I tend to agree with Swanson on this issue. The verbs *svan* and *han* as discussed above are good examples of where fixed equivalence would lead to a bland translation and would leave an enormous array of possible English resources unused. More problematic are words that denote theoretical concepts.

Consider, for example, the Chicago method of hieratic capitals. Van Buitenen consistently renders *dharma* into English as 'Law' (van Buitenen 1973: xli). Now, in some cases *dharma* is well translated by the word 'law', and in some cases it is not. So when that translation works, the capital 'l' looks odd. If the capital 'l' is superfluous, why use it? But when the translation of *dharma* as 'law' doesn't really work, the capital 'l' can signal to the reader *why* the translation doesn't work. It doesn't work because the translator has decided the word *dharma* is too important to be translated according to the context, and that one capitalised word will be used in all cases. Indeed, van Buitenen's defence of his method is precisely so that 'a social historian, or a historian of religion' can keep track of 'the real

scope of the concept of *dharma*' (ibid.). Thus a possible research project is used as justification for van Buitenen's method. The same problem would arise whatever capitalised English word one were to choose as the consistent translation. But then how is the reader – and though van Buitenen was thinking about a researcher, I am thinking of the general reader – to discern between the two cases, the one where 'law' is a good translation of *dharma*, and the one where it isn't? The reader can only guess. And if the reader guesses it is the latter case, what might the more precise contextual meaning be? Again the reader can only guess, because the translator, who is best placed to know this, has declined to communicate it. The reader must learn a bit about the word *dharma* and mentally substitute it for the word 'Law' when 'Law' might not fit, and try to get around the problem that way. In effect, van Buitenen neither translated *dharma* nor left it untranslated; as he himself rightly admits, 'such literalism might result in a quasi-translation' (ibid.). The reader whose agenda his method would facilitate – 'a social historian, or a historian of religion' (and moreover one who doesn't know Sanskrit, since a Sanskrit-knowing researcher can now ask a computer to search an electronic text) – becomes, in effect, the main kind of reader for whom his translation is suited.

In his continuation of van Buitenen's translation project, Fitzgerald uses many more capitalised formulations than van Buitenen did (Fitzgerald 2004: xxi-xxii, 641–46). In the case of the word *dharma*, he translates it with a variety of different words or formulas, depending on the context, and marks those words with initial capitals in almost every case. This seems to be a belt and braces approach (Brodbeck 2005: 241–42). Since the word is always translated with some sensitivity to the context, the initial capitals very often seem superfluous; indeed, they serve mainly to signal to the reader that the word translated is *dharma* (or one of the other special words that are capitalised in the translation). At the same time, as if to provide some play for that knowledge, an appendix to the translation contains a discussion of the range of meanings of the word *dharma* and explanations and justifications of the various words chosen to translate it (Fitzgerald 2004: 641–43; see also pp. 101–28; cf. Fitzgerald 2004b). Thus the reader is encouraged to learn about this Sanskrit word and its range of meanings. But then, why translate it? If van Buitenen neither translates the word nor leaves it untranslated, Fitzgerald both translates it and leaves it untranslated.

In a similar vein, Witzel writes concerning the translation of the word *ṛta* that 'if choosing an *ad hoc* meaning, the reader will never know what is found in the Sanskrit original (*ṛta*), and we would have to explain each time (e.g. in a footnote) that *ṛta* is intended' (Witzel 2014: 68). He concludes that 'The best solution seems [to be] to translate *ṛta* idiomatically but to add the Sanskrit word in parentheses each time, [so] as to allow the reader to gradually understand the concept of *ṛta* with the whole range of meanings it implies' (p. 69). Witzel's comments would apply equally to the word *dharma*, but Witzel writes as if one of the purposes of translating a Sanskrit text into English is in order to teach the reader Sanskrit, which seems to be a topsy-turvy way of thinking about it. I would suggest rather that one of the purposes of translating a Sanskrit text into English is so as to manage *without* teaching the reader Sanskrit.

In my translation of the *Harivaṃśa* – without taking any view on how central the concept of *dharma* is to this text as compared with the *Śāntiparvan* – I have simply translated the word *dharma* according to context, without capitals, as I would any other word that wasn't a name. More generally, though, the word *dharma* here operates as an example of what in Fitzgerald's translation are a variety of words that are rendered in the translation with hieratic capitals, a method which yields an English translation that contains odd typography and sometimes awkward formulations, and one that I have sought to avoid. One possible exception is the word *manu*, which I have rendered as *Manu* when it is used to denote any one of the 14 of that name and cosmic role (see Hv 7). The explanation and justification of the capitalisation is supplied by Vaiśampāyana in the text, as well as by the simple fact that as well as being a role, *manu* is at the same time the proper name of every incumbent of that role.

In the end, then, it seems to me that one reason for translators to tend towards the method of 'fixed equivalence' (or, in Fitzgerald's case, 'fixed equivalences') is because of a kind of fetishisation of certain words in the source language – a kind of fetishisation which is no doubt the result of profound understanding of the source text and context, but is ultimately incompatible with the task of translation. It is perfectly possible to translate some words consistently without mishap, and in many cases I have done exactly that (examples are given below); but I have done this in service to the reader rather than in service to the word, and in other cases I have not hesitated to translate the same word in different ways on different occasions, not because it is a less important concept, but simply because, in its usage in this text, it seems to me to have a more variable connotation in relation to the available English words. Where I have fetishised words in the source language and given them hieratic capitals, I have only done so because I see them as names – that is, as words that are already fetishised in the original, by virtue of the individual existential corporeality of the text's author, characters, and receivers.

### Species of Creature

The *Harivaṃśa*'s species categories might be seen in terms of human prehistory:

[W]e now have access to genome-wide data from four highly divergent human populations that all likely had big brains, and that were all still living more recently than seventy thousand years ago. These populations are modern humans, Neanderthals, Siberian Denisovians, and Australo-Denisovians. To these we need to add the tiny humans of Flores Island in present-day Indonesia – the 'hobbits' who likely descend from early *Homo erectus* ... These five groups of humans and probably more groups still undiscovered<sup>36</sup> who lived at that time were each separated by hundreds of thousands of years of evolution. This is greater than the separation times of the most distantly related human lineages

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<sup>36</sup> For example, *Homo luzonensis* – see <https://www.theguardian.com/science/2019/apr/10/new-species-of-ancient-human-homo-luzonensis-discovered-in-philippines-cave>.

today ... Seventy thousand years ago, the world was populated by very diverse human forms ...

(Reich 2018: 64)

I have translated the names of various species of semidivine creature, such as *gandharva* and *yakṣa*. Some of these are words that other translators wouldn't try to translate. My translations are approximate, but the details aren't crucial to this text. Hopkins, whose surveys of the various types of being are very useful, remarks that 'certain characters stood out more as individuals than as fixed members of a group ... such individuals are sometimes considered as belonging to one and sometimes to another group' (Hopkins 1968 [1915]: 38). In the *Harivaṃśa*, where creatures such as *gandharvas* and *yakṣas* are mentioned, they are often mentioned *en masse* in long *dvandva* compounds, and so those verses would be particularly troublesome for readers were the words to be left untranslated. But if the words are translated, the translator must make use of concepts readers will already have, and because of the success of various fantasy and storytelling genres there is an available, albeit Tolkienised, range of English words for superhuman beings.

Here is a list of types of creature, in Sanskrit alphabetical order, together with the English words I have used to translate them. I have translated *apsaras* as heavenly nymph, *asura* as demon (in the *Harivaṃśa* these are usually called *Dānavas* and/or *Daityas*, words which I've left untranslated because they are matronymic names, as explained at Hv 3.58–80), *kiṃnara* as mountain-elf, *kiṃpuruṣa* as wild-elf, *gandharva* as light-elf, *guhya* as troll, *cāraṇa* as celestial singer, *nara* as spirit-elf, *piśāca* as fiend, *puṇyajana* as ogre, *bhūta* as sprite (though usually as creature), *yakṣa* as dark-elf, *rākṣasa* and *rakṣas* as monster, *vidyādhara* as sylph, and *siddha* as perfected saint. The many words used for snakes and serpents I have translated using those two words synonymously. I considered the word dragon as a possible translation here: it fits in some respects (flying, fire-breathing reptile) but not in others (treasure-hoarding), and I decided it was unnecessary.

To dilate briefly on the subject of elves. In many ways the word 'elf' was rescued by Tolkien from its associations with physical diminutiveness (for example), and restored towards the broad sense of the Old Norse word *álfar* from which it is etymologically derived (on elves in the Old Norse materials as well as in Anglo-Saxon England, see Hall 2007). The *Prose Edda* divides the category *álfar* into two types, *ljósálfar* and *dökkálfar*:

There are many magnificent places there [i.e., in the heavens]. One is called Alfheim (Elf World). The people called the light elves live there, but the dark elves live down below in the earth. They are different from the light elves in appearance, and far more so in nature. The light elves are more beautiful than the sun, while the dark elves are blacker than pitch.

(Sturluson 2005: 28)

The light-elves seem to match the *gandharvas*, whose ‘natural abode is in the air’ (Hopkins 1968: 157), and their simultaneous similarity with and distinction from the dark-elves seems to match the conceptual relationship between *gandharvas* and *yakṣas*.

There is a risk that the connotations of the listed English words will distract the reader from the material in unhelpful ways, but nonetheless I think that when compared to the alternatives, there is more to be gained than lost by the policy I have adopted. In considering the implications I have tried to be honest about how particular words are likely to be received by readers, and what kinds of connotations they will evoke, and to what extent I will be able to counteract or override those connotations by using the words in the specific contexts of the *Harivaṃśa*. In her article on ‘translation and textual scholarship’, Allen cites a translation of Chekhov in which the word referring to a bagel is translated as ‘roll’, with a footnote to explain that it actually is a bagel, but that the translator has avoided the word because its connotations for the reader – in terms of Jewish communities in north-eastern cities of the USA – would differ markedly and unhelpfully from its connotations for Chekhov and his intended audience (Allen 2013: 213–14). For similar reasons I abandoned my earlier translation of *mahātman* as ‘mahatma’ (a word with which many English speakers are familiar, and which is listed in the *Oxford English Dictionary*), because Will Johnson pointed out that it would inevitably be accompanied, in the reader’s mind, by an image of a bald man wearing a shawl and little round glasses. Conversely, after much deliberation I decided to retain my translation of *yoga* (in some contexts) as ‘yoga’, even though the English word tends to stress the physical postural aspects of *yoga*, which are not necessarily to the point when the word occurs in the text. I have noted in the translation’s introduction that *yoga* is not merely a physical discipline.

### Plant and Tree Names

Sanskrit botanical and zoological terms are particularly difficult to deal with in translation, as sometimes there will not be any word in the target language for that particular species. For me it is important to preserve the idea that the forests described in Sanskrit literature are full of tree varieties unlike the ones that I have known where English is spoken, and so there is no question of arbitrarily pressing familiar tree-names into service to represent the *Harivaṃśa*’s varieties of tree. But I have not lived enough where those varieties live to know first-hand what they are commonly called where (and if) they occur today, and so for my word choices I have been dependent on the standard dictionaries, on the ‘Pandanus Database of Plants’ established under the auspices of the Charles University, Prague (<http://iu.ff.cuni.cz/pandanus/database/>), on other translations, on Couture’s ‘Index des noms de plantes’ (Couture 1991: 434–36), on selected other pieces of Indological literature, and on various illustrated online encyclopedias. My word choices might succeed or fail with certain readers depending on the reader’s own experience of the varieties in question. That is inevitable. In any case, I list here, again in Sanskrit alphabetical order, the words for types of plant and tree and the translations that I have used for them. In some cases the Sanskrit word has been left untranslated and used as if were an English word. I have translated *akṣaka* as rosewood, *arjuna* as arjuna tree, *asana* as crocodile-bark tree, *iṣusāhva*

as arrow-name tree, *utpala* (and *kuvalaya*) as water-lily (not lotus; Rau 1954; Hanneder 2002; Gail 2002, figs 1–2), *kadamba* (and *priyaka*) as kadam tree, *kandala* as plantain tree, *karambhā* as fennel, *karavīra* as oleander, *kāśa* as kans grass, *kumuda* as night-flowering water-lily (Rau 1954; Lienhard 2000; Hanneder 2002), *kuṭaja* as coral-swirl tree, *kuśa* as kusha grass, *ketakī* as screwpine tree, *kesara* as rose-chestnut tree, *kovidāra* as orchid tree, *gavedhuka* as tear grass, *tāla* as palm tree, *tālī* as talipot palm, *darbha* as darbha grass, *nārikela* as coconut palm, *nikumbha* as danti bush, *nīpa* as jungle-flame bush,<sup>37</sup> *pāṭalā* as trumpet-flower tree, *pārijāta* (at 107.3a) as coral tree, *pālāśa* as flame tree, *picuka* as meyna tree, *pumṇāga* as poon tree, *badarī* as jujube, *bandhujīva* as noon-flower, *bījaka* as citron tree, *mandāra* as coral tree, *latā* (sometimes) as mangrove (Couture 2003b: 226), *śaivāla* as eel grass, *saptaparṇa* as seven-leaved blackboard tree, *saṃtānaka* as wishing tree, *sāla* as sal tree, *silīndhra* as mushroom (Levitt 2011: 108–11), *śṃmara* as srimara tree, and *svaṃṇaka* as golden svaṃṇaka tree.

### Bird Names

Here is a similar but shorter list for potentially problematic ornithological words (these are notorious for being applied across species boundaries; cf. Dave 2005, which I have used extensively): I have translated *ulūka* as owl, *kāka* as crow (cf. Fitzgerald 1998: 261), *kāraṇḍava* as duck, *kukuṭa* as jungle-cock, *kurarī* as female osprey, *kokila* as cuckoo, *krauñca* as crane, *cakravāka* as brahminy duck (cf. Pieruccini 2002), *jīvaṃjīvaka* as pheasant, *barhin* as peacock, *balāka* as flamingo, *bhāsa* as vulture, *mayūra* as peacock, *rājahaṃsa* as bar-headed goose (cf. Couture 2015b: 304–05), *vāyasa* as crow, *śikhin* as peacock, *śyena* as hawk, *sārasa* as crane, *suparṇa* as eagle, *stokaka* as crested cuckoo (cf. Rau 1986: 195), and *haṃsa* as goose. There are no swans.

### Other Difficult Words

There are cases where I have consistently used standard translations of specific words that another translator translating for a more Sanskrit-oriented audience might well not have translated. I list here, again in Sanskrit alphabetical order, some of the words whose translations have exercised me in particular. I have translated *adhvaryu* as operating priest, *udgātṛ* as chanting priest, *ṛṣi* as seer, *kiṣku* as cubit, *krośa* as league, *cakravartin* as universal emperor, *caitraratha* as Kubera’s pleasure-grove, *dakṣiṇā* as sacrificial gift / gifts for the priests (not a fee paid for services rendered; cf. Gonda 1954: 227–28, 230; Heesterman 1959), *nandana* as Indra’s pleasure-grove, *nalva* as furlong, *parameṣṭhin* as pre-eminent (cf. Gonda 1985), *purāṇa* as ancient/old story/stories/tale/lore (cf. Fitzgerald 2014b: 49–53; Couture 2015c), *puruṣa* as cosmic person (cf. *Ṛgveda* 10.90; Conger 1933), *purohita* as chief priest, *prajāpati* as patriarch (I tried using progenitor at first, but switched because patriarch seems more accurate notwithstanding and partly because of its modern feminist usage), *brahmarṣi* as brahmin seer, *brahman* as supervising priest, *māhātmya* as greatness, *muni* as (sometimes silent) sage, *yuga* (in both senses, the four individual *yugas* as well as the larger

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<sup>37</sup> Regarding *nīpa* as jungle-flame: at 73.16d some *nīpa* plants are *puṣpakaṇṭhakadhāriṇaḥ*, which I have translated as ‘with their pointy petals’ but could mean the plant has thorns (the jungle-flame doesn’t).

fourfold *yuga* that they comprise) as age, *rājarsi* as royal seer, *vajra* (and *vajrāsani*) as thunderbolt, *sadasya* as superintending priest, *sūta* as storyteller (or, at Hv 62.94, royal retainer), and *hotṛ* as invocatory priest.

For understanding and translating units of measurement, I have made use of Srinivasan 1979; for large numbers, I have referred to Ifrah 1998: 428–29. The only unit of measurement that I have left untranslated (see list of untranslated words below) is the *yojana*, which has several possible senses and there is no approximate English equivalent for any of them.

For many other words too, words not mentioned in the above lists, I have been assisted in translating the specific word by the discussion in a particular item of secondary literature. I list some such words here, in Sanskrit alphabetical order, with references to the secondary literature I have used: *aṃśa* (Couture 2001); *aṭṭasūla* (Dundas 2014: 241 n. 39); *arbuda* (Suneson 1991: 110–11; Selby 2008: 53); *alātacakra* (Fitzgerald 2012); *aṣṭāpada* and *apāṃsula* (Thieme 1984: 422–23); *ātman* (Ingalls 1959); *āścarya* (Kuiper 1961; Brinkhaus 2001); *ekānaṃśā* (Couture and Schmid 2001; Ate 2014; Yokochi 2001: 44–45 n. 18); *aiśvarya* (Malinar 2012); *audbhida* (Tsuchida 2010: 9); *kaṣāya/kāṣāya* (Olivelle 2011: 54; Roebuck 2012: 235); *caturaṅga* (Bock-Raming 1995); *tejas* (Magnone 2009, 2018); *dakṣa* (Long 1977); *dasyu* (Bowles 2018); *dharma* (Fitzgerald 2004b; Bowles 2007: 81–132); *naravāhana* (Hopkins 1913: 60–61); *nirveda* (Fitzgerald 2015: 103); *prāṇa* (Zysk 1993; Killingley 2006); *buddhi* (Fitzgerald 2015, 2017, 2017b); *bhakti* (Couture 2017d); *bhāga* (Couture 2001); *mahābhāga* (Vassilkov 2009); *māna* (von Simson 2007: 241–42); *māyā* (Gonda 1959, esp. p. 164; Saindon 2004: 37); *mārtaṇḍa* (Couture 2009; Jamison 1991: 116, 204–08); *yavana* (Karttunen 2015); *rasa* (Wujastyk 2005); root *vṛt* (Bailey 2017); *śakraḡopa* (Lienhard 1978); *savarṇā* and *sāvarṇa* (Bloomfield 1893: 172–88); *sariddvīpa* (Yokochi 2000: 536–37 n. 28); *sīmanta* (Gonda 1956); *surā* (and other alcohol words, McHugh 2014); *soma* (when paired with fire, Wujastyk 2004); *svārūḍha* (Trautmann 2015: 143).

Some words have proved consistently difficult for me to construe in English, and some of these have had to be translated variously, with particularly close attention and sensitivity to the specific context. Among these difficult words I mention *acyuta*, *antara*, *ātman*, *kakṣyā*, *kilbiṣa*, *kṛtānta*, *gati*, *citra*, *daṃśita*, *doṣa*, *dharma*, *bhūta*, *mantra*, *mahātman*, *māhātmya*, *muhūrta*, *loka*, and the roots *muh* and *saṃkṣip*.

### *Kṛtānta*

As a brief case study here (many others could be added) I discuss the specific compound-word *kṛtānta* (*kṛta* + *anta*), which occurs 11 times in the text. Monier-Williams lists among its various meanings “the inevitable result of actions done in a past existence”, destiny, fate, R.; Pañcat.; Megh.; Vet.; death personified, N. of Yama (god of death), MārKP.; Hit.’ (Monier-Williams 1899: 303 col. 1). Previous translators of the *Harivaṃśa* have tended to choose the second of these two quoted senses. I have translated *kṛtānta* as ‘karma’ at 40.14d, 48.45d, 77.14d (qualified by *balīyas*), 77.25c (qualified by *adrśya* and the present participle of the root *nī*), 77.32d, 77.56b (qualified by *anivartin*), 78.12b, 99.37d (again qualified by *balīyas*), and similarly as ‘the comeuppance for his deeds’ at 85.48c, but I have translated it as ‘Yama’

at 78.6c and 109.8d, where the context seems to require *kṛtānta* to be a person. In many more of the cited cases translating *kṛtānta* as ‘Yama’ would work, but I think the translation ‘karma’ is preferable if possible, because it alludes to an automatic law of nature (the English word ‘karma’ isolates the particularly post-active aspects of the Sanskrit word *karman*) rather than to the potentially whimsical agency of a god, and also because in many of the cited cases a character born as a low-ranking human type (e.g., female), and who might thus already be thought not to be very karmically distinguished, is alluding to their evidently dubious past karma as an explanation for specific additional misfortunes when grown-up. Comparable, though without the use of the word *kṛtānta*, are the sentiments expressed by Dhṛtarāṣṭra at Mbh 11.1.18 and by Gāndhārī at Mbh 11.16.59 and 11.18.11–12, alluding to previous bad karma as a possible explanation for present misfortunes, in Dhṛtarāṣṭra’s case by a man born blind, and in Gāndhārī’s case by a woman who was given away in marriage to a blind man.

### Untranslated Words

I have left some Sanskrit words untranslated, sometimes because the word denotes a concept so important that the reader needs to adopt it (e.g. *kṣatriya*), sometimes because it refers to a specific natural or cultural item, and sometimes because the word has already passed into English and is in the *Oxford English Dictionary* (e.g. *āśrama*). Some words in the second category, for example the names of plants such as *arjuna*, *darbha*, *kuśa*, *sṛmara*, and *svarnaka*, have already been mentioned above. Here is a list of the others: *āśrama* (anglicised as ashram), *ukthya* (offering), *kali* (age), *kṛta* (age), *kṣatriya* (class), *tretā* (age), *darvī* (ladle), *dvāpara* (age), *bṛhat* (chant), *brahman* (the absolute), *brahman* as/or *brāhmaṇa* (both anglicised as brahmin), *brahmaśiras* (missile), *mṛdaṅga*, *yoga*, *yogin* (anglicised as yogi), *yojana*, *rājasūya* (rite/ritual), *ruru* (deer), (call of) *vaṣaṭ*, *vīṇā*, *veda* (sometimes in the plural), *vaiśya*, *śāstra*, *śūdra*, *śrīvatsa* (curl/mark/sign), *sāṃkhya* (philosophy), *sāṃkhyayoga*, *soma* (the sacred drink), (call of) *svadhā*, and (call of) *svāhā*. A list of untranslated words is also presented in glossarial fashion at the end of the introduction to the translation.

### NAMES

There are quite a lot more names in my translation than there are in the original, because I have often added them for clarity, so that the reader will not wonder who is doing what. Sometimes I have added names in place of pronouns, and sometimes to complement verbs that don’t need explicit agents in Sanskrit (where English has ‘he/she/it eats’, Sanskrit can just have ‘eats’, with the agent merely implied). The *Harivaṃśa* is sometimes sparing with names almost to the point of obscurity in the original (several of the problematic passages discussed in the final section below are problematic simply in terms of the interpretation of who is who), but even where this is not the case the Sanskrit, because of its multiple clauses, would sometimes still yield a cryptic or ambiguous sentence in English unless names were added.



One character may have many names in the Sanskrit. Sarma says that ‘The solution lies in the translator adopting one name of each character as the base and use it wherever the person is referred to by his different synonyms’ (Sarma 1992: 288). But the use of multiple names is a significant aspect of the text, and I have wanted to remain generally faithful to this aspect if possible, not least because it seems to have distinctive theological implications: the multiplicity of a divinity’s nomenclature is most evident in the case of the most fundamental gods Viṣṇu and Śiva (Viṣṇu’s thousand names are listed at Mbh 13.135, Śiva’s earlier at Mbh 13.17). Gonda comes to this theological sense in the context of *bhakti*:

Since a name is an actuality expressed in a word which – if the name belongs to a divinity – lends to the divine presence some settled content, it enables the worshipper to form an idea of that divine presence and to come to some understanding with the power which it represents. The name, being a ‘double’ of the object for which it stands or a representative indissolubly associated with that object, gives the person who knows it the power to enter into contact with its bearer, to call, invite, summon or activate that bearer. Knowledge of the name may moreover be a means of attracting or transferring to oneself the special power inherent in its bearer.

(Gonda 1975: 45)

In this light, the maintenance of multiple names within a translation would seem most appropriate in a translation of an Indian religious text. But how is it to be done satisfactorily, so that the reader knows who is who? Here my solution is two-pronged. Sometimes, for clarity, I have added a character’s more common and/or specific name into the translation, so for example *kauśika* can be translated as ‘Vishvāmitra Kaushika’, *vaivasvata* as ‘Manu Vaivasvata’, and *kumāra* as ‘Skanda Kumāra’. In other cases I have allowed several different names to serve for one character, but all the multiple names are listed in the introduction and also resolved to one in the index – so, for example, the index entries for Adhokshaja, Dāmodara, Hari, Hrishīkesha, Janārdana, Keshava, Madhusūdana, Vāsudeva, and Vishvaksena all say ‘see Krishna’. By means of these two strategies I have avoided the need to use footnotes to supply the more common name of characters for whom rarer alternative names are used in the text. Where for example Fitzgerald 2004: 31 has the one-word footnotes ‘Vyāsa’ and ‘Karṇa’ to explain the names ‘Kṛṣṇa Dvaipāyana’ and ‘Rādheya’, I would simply put those footnotes upstairs as if they were part of the text, whenever (and including many more instances of where) such footnotes might be useful to keep the reader on track.

Should personal names be translated? Schreiner consistently translated them in his translation of the *Viṣṇupurāṇa*, where possible (Schreiner 2013); but since not all names can be translated, this is a necessarily hybrid method. Likewise, the meanings of many of the names in the *Harivaṃśa* elude me. Sometimes where I do know what the name means, the meaning seems contextually irrelevant. But sometimes it is crucial, or at least interesting. So flexibility is required in the translation.

But what is a name, anyway? This is a bit arbitrary when translating from Vaidya's Sanskrit into English, because there are no capital letters in Sanskrit, and a lot of the names are of fairly standard types, and so in lists of sons, for example, it can sometimes be hard to tell whether a word is an adjective describing a previously (or subsequently) named person, or is actually a different person. I have tried to use any discernable clues in making the necessary decisions. When possible adjectives occur in isolation referring to a specific person – here they might be thought of as nicknames – it is sometimes not immediately evident who that specific person is. In other cases the nickname is so common in connection with that person or so much a part of the narrative that I have treated it like a proper name, as mentioned just now when I listed the various names for Kṛṣṇa that I've left freestanding in the translation (and of course he is not the only character who receives this treatment).

In making the necessary decisions here I have been aware that in marking what are termed 'proper nouns' with capital letters, the orthographical conventions of English impose a foreign regime upon the text, and that the implications of that imposition are compounded since capitalised nouns will find a place in the 'index of names' whereas non-capitalised adjectives will not. Perhaps a more radical translator might produce a translation with no capital letters at all, just as I have produced one without quotation marks. A full stop should suffice, after all, to mark the junction between sentences (though even then it would be a mysterious addition). Such a translator could say that their decision was taken out of fidelity to the text. Perhaps they might save themselves certain other types of decision as a result. But perhaps the decision to do away with capitals would immediately spawn multiple other problems requiring decisions. That's what my translation decisions have tended to do when taken in order to avoid problems! There is no doubt, though, that in the text the multiplication of nicknames, epithets, matronyms, and overlapping patronyms (every Pāṇḍava, for example, can also be called Kaurava and Bhārata, through the patriline) makes the text potentially rather inaccessible unless clarifying additions are made by the translator. There is probably a limit to what the faithful translator can do here because of the nature of the material – there are simply so many minor characters – but I have tried to err on the side of accessibility.

To return to the question of whether to translate names. A big part of the problem is that if the name is translated then the sound of the name is lost, and that if it is not translated the meaning of the name is lost. In that respect this decision is a subspecies of the kind of basic decision the translator must iteratively make: which tones or aspects of the song shall I preserve, and which others shall I sacrifice to that end, and how consistent, in its rationale for what to preserve or sacrifice, is the optimum method? For with poetry in particular, multiple meanings or nuances of meaning are very often present. Tytler says:

[W]here more than one meaning can be given to the same passage or expression ... the translator is called upon to exercise his judgement, and to select that meaning which is most consonant to the train of thought in the

whole passage, or to the author’s usual mode of thinking, and of expressing himself. To imitate the obscurity or ambiguity of the original, is a fault ...

(Tytler 1907 [1813]: 17–18)

This appeal to the ‘train of thought’ of a ‘whole passage’ and the ‘author’s usual mode of thinking’ collapses upon the subjectivity of the translator, who may typologise his or her activity (or not) in any way convenient. The translator’s idea of types of consistency is, for all we know, arbitrary. In any case, whatever rules I have made, I have tried to be as happy as possible to break them.

In general I have not translated names. Thus the *Harivaṃśa* translation, which in some passages contains an awful lot of names, will suggest something of the sound of spoken Sanskrit. The policy with names has been thought and rethought a number of times, but it was decided that the names had to be used as a feature of the translation. This decision is connected with the aforementioned decision to use a simplified diacritical scheme so that names will be more easily pronounceable for readers (see above, pp. 117–18). But sometimes, when the meaning of a name is important, after the Sanskrit name I have provided the same name translated into English, with initial capitals. This is having the cake and eating it, and it could easily result in an over-enriched offering. Usually the translation only needs to be given once or twice (and in fact most characters are only mentioned once). I give a couple of examples here.

*somasya bhagavān varcā varcasvī yena jāyate |*  
*dharasya putro draviṇo hutahavyavahas tathā |*  
*manoharāyāḥ śísīrah prāṇo ’tha ramaṇas tathā || 3.34 ||*

Soma’s son was illustrious Varcas, the Vigour by means of whom any vigorous child is born. Draviṇa the Goods was Dhara’s son, and so was Hutahavyavaha the Transportation of the Burnt Offering, and so, by Manoharā the Fascinator, were Śísīra the Cool Dew, Prāṇa the Breath of Life, and Ramaṇa the Delightful.

*bhagīrathasuto rājā śruta ity abhiviśrutaḥ |*  
*nābhāgas tu śrutasyāsīt putraḥ paramadhārmikaḥ || 10.67 ||*

Bhagīratha’s son was called Śruta – King Famous. He was famous everywhere. And Śruta’s son was righteous Nābhāga ...

This method allows wordplay to be preserved in translation when it is anchored in a name. In other cases there is soundplay in relation to a name, when a joke etymology is provided for it. The name itself may be meaningless, but is said to be derived from a salient word or words in that character’s story. These jokes (or whatever they are) depend upon the phonological link, so in such cases I have put the Sanskrit word (or words) that the name is

said to be derived from in the translation in brackets, after the word that translates it, so that the reader will be in on the joke, as it were. Here again are a couple of examples.

*viṣṇuḥ svayaṃbhūr bhagavān sisṛkṣur vividhāḥ prajāḥ |*  
*apa eva sasarjādau tāsu vīryam avāsrjat || 1.23 ||*  
*āpo nārā iti proktā nāmnā pūrvam iti śrutiḥ |*  
*ayanaṃ tasya tāḥ pūrvam tena nārāyaṇaḥ smṛtaḥ || 1.24 ||*

Self-born Lord Viṣṇu wanted to have various children. So he first emitted the waters, and then he ejaculated into them. The waters are called Nārā. According to the Veda, that was their original name. In the beginning they were his recourse (*ayana*), and that's how you remember Nārāyaṇa.

*so 'bhavad gālavo nāma galabandhān mahātapāḥ |*  
*maharṣiḥ kauśikas tāta tena vīreṇa mokṣitaḥ || 9.100 ||*

Because he'd been bound by the neck (*gala*), the descendant of Kuśika that our hero had set free was called Gālava. He became a great seer and a great ascetic, my boy.

There are a few names that I have consistently translated. Listed in Sanskrit alphabetical order, they are: *uttarakuru* (plural), the Northern Kurus; *kāmaga* (plural), the heavens of Do-as-you-Please; *kāla* (when anthropomorphised), Time or Death, depending on the context (I'd rather not choose between the two, but needs must); *jvara*, Fever; *dhundhumāra*, the Killer of Dhundhu; *navarāṣṭra*, the New Country; *nidrā*, Sleep; *pākaśāsana*, the Punisher of Pāka (a partial translation); *maṇiparvata*, Jewel Mountain; *ṣaḍgarbha* (plural), the Sixkids; *sanātana* (plural), the Eternal heavens. Various different names of the earth are additionally rendered as Earth in contexts where she is particularly personified, and *pitāmaha*, when used as a nickname for Brahmā, is rendered as (the) Grandfather throughout. My piecemeal policy on this issue is shared by Fitzgerald, who leaves most names untranslated, but translates for example *tanu* as Skinny, *cirakārin* as Slow-to-Act, and *pūjanī* as Adorable, without including the Sanskrit in the translation (Fitzgerald 2004: xx). There are also a pair of cases – Agni and Vāyu – where I have used the capitalised translations Fire and Wind on their own interchangeably with the principal name, in order to make it clear who is meant without having to trouble the reader with several other names that only occur once or twice.

A further question that faces the translator is what to do when a particular name has an anglicised or a more up-to-date Indian version. This is most often the case with names of places and geographical features (e.g. Irrawaddy for *irāvati*, Kashmir for *kaśmīra*, Kampil for *kāmpilya*, Ganges for *gaṅgā*, Gomti for *gomatī*, Jumna for *yamunā*, Rajgir for *rājagrha*, Indus for *sindhu*, etc.). There is certainly a case to be made for using such non-Sanskrit forms, on the basis of their familiarity to (at least some sections of) the target

audience. But because the British engagement with South Asia used to be so much more extensive than it is currently, my sense is that some of these names would tend to have a dated and colonial feel, which it would not be useful to evoke, especially in cases (such as *yamunā*) where the name is fairly common and prominent within the *Harivaṃśa* narrative. Additionally, in the cases of the main rivers the name refers not just to the body of water but also – and sometimes more saliently – to the female personification thereof, but the anglicised form is generally restricted to the former sense, and so doesn't have the required range. Accordingly, I have usually eschewed such anglicised forms. Exceptions here are Punjab and Kashmir, forms which I have used because both are well known in recent decades (Kashmir from the brilliant Led Zeppelin song as well as from the news).

In the Sanskrit text many names, some of which only occur a few times, occur with two or more variant stem-forms, and in such cases I have sought, in the translation, to standardise the name, so as to avoid ambiguity and confusion, not least my own. Usually I have simply standardised to the most common variant, although there have sometimes been other considerations. In some cases the two variants are patronymic forms (patr.) differentiated only by the length of the first vowel – thus in the Sanskrit e.g. Bharata and Bhārata are both used to mean 'descendant of Bharata' (though only the former can also mean 'Bharata himself') – and in such cases I have tended to standardise to the form with the lengthened first vowel regardless of the number of instances, in order to differentiate the ancestor from the descendants. However, I have not done this for example with the use of Yadu in the sense of Yādava (or Kuru in the sense of Kaurava), where I have simply mirrored the text and used 'Yadu' in two senses. In some other cases I have standardised *Harivaṃśa* names to forms that occur in the *Mahābhārata* but not in the *Harivaṃśa*.

Here is a long table called 'tampered names', showing what I have done in this regard. The entries are arranged by Sanskrit alphabetical order of the first variant. Instances where it is not possible to tell which variant occurs – e.g., *kālaneminā* (stem could be *kālanemi* or *kālanemin*), *keśiniṣūdanaḥ* (name stem could be *keśi* or *keśin*) – have not been counted. There are explanatory footnotes keyed to the 'standardised form' within the table.

**Table 2. Tampered Names**

First Variant	No. of Times	Other Variant/s	No. of Times	Standardised Form	Pādas Affected
<i>agāvaha</i>	2	<i>āgāvaha</i>	1	Agāvaha	81.99a
<i>aṅgāraṣetu</i>	1	<i>aṅgāra</i>	1	Angāraṣetu	23.132a
<i>atidānta</i>	1	<i>anirdānta</i>	1	Atidānta <sup>38</sup>	87.55c

<sup>38</sup> It seems likely that the two names Atidānta and Anirdānta are intended to refer to the same character, since they occur quite close together (87.47c, 55c) and Bṛhaddurga follows them immediately in both cases. Vaidya's apparatus indicates that many manuscripts have the same name at both places (Vaidya 1969: 562–63). As for the question of who this character is, it seems to be left open whether this would be

First Variant	No. of Times	Other Variant/s	No. of Times	Standardised Form	Pādas Affected
<i>antardhi</i>	1	<i>antardhāna</i>	1	Antardhāna	2.27b
<i>andhaka</i> (sing.)	5	<i>andha</i>	1	Andhaka	66.2a
<i>ariṣṭa</i>	13	<i>ariṣṭaka</i>	1	Arishta	96.38d
<i>āṅgirasa</i>	3	<i>āṅgiras</i> <i>aṅgiras</i> (patr.) <i>aṅgirasa</i>	4 1 1	Āngirasa / son of Angiras	34.51b 65.18d 65.39b
<i>āhvṛti</i>	3	<i>āhṛti</i>	4 in Mbh	Āhṛiti <sup>39</sup>	80.10e; 96.52d; 97.5a
<i>iḍā</i>	7	<i>ilā</i>	2	Ilā <sup>40</sup>	3.67b; 9.5d, 6a, d, 8b, 9b, 14d
<i>uśata</i>	1	<i>uśat</i>	1	Ushat	26.6c
<i>ṛtaparṇa</i>	3	<i>ṛtuparṇa</i>	32 in Mbh	Rituparna <sup>41</sup>	10.69b, 70a, b
<i>aurva</i>	2	<i>ūrva</i>	6	Ūrva	35.23c, 64c
<i>kārūṣa</i>	5	<i>karūṣa</i> (patr.)	3	Kārūsha	24.22a; 75.22a; 81.38d
<i>kālanemi</i>	12	<i>kālanemin</i>	6	Kālanemi	36.57b; 37.4d, 8d; 38.45d, 48d; 47.12d
<i>kekaya</i> (patr.)	1	<i>kaikaya</i>	1	Kaikaya	10.21b
<i>keśi</i>	2	<i>keśin</i>	28	Keshin	31.77b, 144c
<i>kauṭavī</i>	1	<i>koṭavī</i>	2	Kotavī	112.49d
<i>kroṣṭṛ</i>	1	<i>kroṣṭu</i>	5	Kroshtu	23.134d
<i>garuḍa</i>	37	<i>garutmat</i>	5	Garuda	63.23c; 88.23d; 94.8b; 108.79d; 112.113b
<i>gāṃdinī</i>	2	<i>gāṃdī</i>	3	Gāndinī	28.37c (twice), 29.25d

Atidatta son of Rājādhiveva (28.2c), or Adhidānta son of Hṛdika (28.6c), or someone else. None of these four names (Atidānta, Anirdānta, Atidatta, Adhidānta) appear in Sørensen 1978 [1904].

<sup>39</sup> The name *āhvṛti/āhṛti* is standardised to the version known in earlier parts of the *Mahābhārata*. The version that appears in the *Harivaṃśa* (*āhvṛti*) is listed as a variant at Mbh 2.28.39a.

<sup>40</sup> The name *iḍā/ilā* is standardised to the version known in earlier parts of the *Mahābhārata*. In addition to the Ilā who features in both solar and lunar royal lineages, this is then also the name of Tamsu's wife at Hv 23.45a and Danu's 'son' at Hv 3.67b. Intertextual resonances are thus preserved.

<sup>41</sup> The name *ṛtaparṇa/ṛtuparṇa* is standardised to the version known in earlier parts of the *Mahābhārata*. On the first of the three occasions where this name appears in the *Harivaṃśa* I have included a footnote referring the reader to those appearances. At Hv 10.69b, on the first occurrence of *ṛtaparṇa*, Vaidya's apparatus says 'some Mss. *ṛtu*° (for *ṛta*°)' (Vaidya 1969: 88).

First Variant	No. of Times	Other Variant/s	No. of Times	Standardised Form	Pādas Affected
<i>chidradarśana</i>	1	<i>chidradarśin</i>	1	Chhidradarshana	18.16a
<i>tārā</i>	6	<i>tārakā</i>	9	Tārakā <sup>42</sup>	20.29b, 30c, 36c, 39d, 41b, d
<i>tosalaka</i>	2	<i>tosala</i>	1	Tosalaka	76.3a
<i>triśānu</i>	1	<i>traisānu</i>	1	Trishānu	23.124a
<i>dantavaktra</i>	10	<i>vaktra</i>	1	Dantavaktra	97.5c
<i>dasārha</i> (patr.)	5	<i>dāsārha</i>	11	Dāshārha	81.51e; 93.8a; 94.13a; 96.7a; 97.36a
<i>duryodhana</i>	5	<i>suyodhana</i>	1	Duryodhana	97.18b
<i>daitya</i>	131	<i>daiteya</i>	6	Daitya	21.19b; 31.88a; 33.1b; 36.17d; 91.4b; 112.74b
<i>nandin</i>	3	<i>nandi</i> <i>nandika</i>	1 1	Nandin	112.83a 112.83c
<i>nāgnajitī</i>	1	<i>nagnajitī</i>	2	Nāgnajitī <sup>43</sup>	98.3b, 9b
<i>nārāyaṇī</i>	1	<i>nādāyaṇī</i>	2 in Mbh	Nādāyaṇī <sup>44</sup>	89.7c
<i>parikṣit</i>	7	<i>parīkṣit</i>	4	Parikshit <sup>45</sup>	23.109c, 110a, 113b, 121c
<i>pāñcāla</i>	9	<i>pañcāla</i>	1	Pāñchāla	24.29d
<i>puṇḍraka</i>	1	<i>pauṇḍra</i> (pl.)	4	Paundra	23.32c
<i>pralamba</i>	1	<i>lamba</i>	3	Lamba <sup>46</sup>	31.75d
<i>prahrāda</i>	6	<i>prahlāda</i>	1	Prahrāda	4.4d
<i>prācīnabarhis</i>	4	<i>prācīnabarhi</i>	1	Prācīnabarhis	2.30c

<sup>42</sup> The name in the Sanskrit text is always Tārā when used to refer directly to the character, and always Tārakā when it forms part of the name of the (*tārakāmaya*) war. Nonetheless, since my translation of *tārakāmaya* – ‘[war over] Tārakā’ – includes the character’s name, standardisation is required.

<sup>43</sup> Here I have adopted the rarer variant, because the lengthened first vowel suits its apparent identity as a patronym (Nagnajit is mentioned at 80.15, and at 88.41 this woman’s name is given as Satyā Nāgnajitī).

<sup>44</sup> The name *nārāyaṇī/nādāyaṇī* is standardised to the version known in earlier parts of the *Mahābhārata*. On the one occasion where this name appears in the *Harivaṃśa* I have included a footnote referring the reader to its earlier appearances. See discussion above, pp. 89–93.

<sup>45</sup> Two different people have this name, but although this offers the possibility of spelling it one way for one of them and the other way for the other, this is discouraged by Vaiśampāyana’s explicit statement that two people have the name (23.113). With this name, regardless of the *Harivaṃśa* instances I would want to standardise to Parikshit because that is the form used in the rest of the *Mahābhārata*.

<sup>46</sup> Lamba is reborn as Pralamba (Hv 44.71), so it is anachronistic to call him Pralamba at 31.75d during the Tārakāmaya War.

First Variant	No. of Times	Other Variant/s	No. of Times	Standardised Form	Pādas Affected
<i>baladeva</i>	25	<i>balabhadra</i>	2	Baladeva	29.28d, 110.42b
<i>bhārata</i>	87	<i>bharata</i> (patr.)	48	Bhārata	1.7c; 23.160c; 105.5c; plus all occurrences of <i>bharatarṣabha</i> , <i>bharataśreṣṭha</i> , & <i>bharatasattama</i>
<i>marutvat</i>	1	<i>marut</i>	17	Marut <sup>47</sup>	3.27c
<i>mura</i>	4	<i>murū</i>	4	Mura	91.15b, 19c, 44c; 97.1a
<i>mṛttikāvātī</i>	1	<i>mṛtikāvātī</i>	1	Mrittikāvātī	27.15d
<i>mṛdara</i>	2	<i>mṛdura</i>	3	Mridura	24.9b; 87.47b
<i>rājagr̥ha</i>	1	<i>girivraja</i>	1	Girivraja <sup>48</sup>	80.1b
<i>raivata</i>	4	<i>revata</i>	1	Raivata	86.80a
<i>vatsavat</i>	1	<i>vatsāvat</i>	1	Vatsavat	24.28a
<i>vairāja</i>	2	<i>virāja</i>	1	Vairāja <sup>49</sup>	13.8a
<i>vṛṣṇi</i>	3	<i>pṛśni</i>	2	Vrishni <sup>50</sup>	28.36b, c
<i>śatruḡhna</i>	1	<i>śatruhan</i>	1	Shatruḡhna	28.39d
<i>śaryāti</i>	3	<i>śāryāti</i>	1	Sharyāti <sup>51</sup>	9.30b

<sup>47</sup> On the Marutvats as the Maruts (an identification that Monier-Williams 1899: 790 cols 2–3 doesn't quite make), see Hopkins 1968: 170.

<sup>48</sup> Girivraja is the name commonly used elsewhere in the *Mahābhārata*.

<sup>49</sup> *Virājasya* occurs at Hv 13.8a. Elsewhere we find Virāj son of Viṣṇu (1.38–39), Virāj son of Kāmyā (2.6), Virajā (13.60), and Virajas (7.26), but no Virāja. Because of the word *prajāpateḥ* at 13.7d immediately preceding *virājasya*, I have identified this Virāja with the patriarch Vairāja (2.16, 4.11), who now is not to be confused with his sons the Vairājas (13.8b), still less with Vairāja the cosmic person (1.39, 2.5). On this tricky issue, even if one doesn't confuse one character with another, one can still be left confused.

<sup>50</sup> There are some grounds for doubting whether *pṛśni*, which means 'dappled', would really be an alternative version of the name *vṛṣṇi*, which means 'strong, manly'. However, it seems clear that both words refer to the same character, father of Citraka and Śvaphalka. When the name Vṛṣṇi occurs (twice) at Hv 24.3, it does so in every manuscript; but when the name Pṛśni occurs (twice) at Hv 28.36, more than a dozen manuscripts instead read the name Vṛṣṇi (twice). See discussion above, pp. 97–99.

<sup>51</sup> When the variant *śāryāti* occurs at 9.30b the apparatus notes that 'Some Mss.' have *śaryāteḥ* rather than *śāryāteḥ* (Vaidya 1969: 70), and the imprecision of this note suggests that Vaidya views *śāryāteḥ* (which qualifies the nominative *saṃtatiḥ*) as a variant spelling rather than a materially variant reading. The case is slightly complicated by the immediately preceding genitives, which clearly refer to Śaryāti's descendant Kakudmin Raivata, and which one might wish to construe with this genitive *śāryāteḥ*; but I have taken the first *pāda* of the verse separately, as a genitive absolute. The patronymic *vṛddhi*-formation from the name *śaryāti* is *śāryāta*, not *śāryāti* (Hv 9.34d; Monier-Williams 1899: 1067 col. 1).



First Variant	No. of Times	Other Variant/s	No. of Times	Standardised Form	Pādas Affected
<i>śaṃtanu</i>	5	<i>śāṃtanu</i>	4	Shantanu	23.114d, 115a, 118a, 119c
<i>śrāvasta</i>	2	<i>śrāvastaka</i>	1	Shrāvasta	9.46a
<i>śrutaśravā</i>	2	<i>śrutaśravas</i>	1	Shrutashravā	87.20d
<i>sātvat</i>	2	<i>sātvata</i>	4	Sātvata	26.27d; 81.62c
<i>sālva</i>	7	<i>śālva</i>	many in Mbh	Shālva <sup>52</sup>	31.144a; 80.14c; 81.98b; 97.6c; 105.13c; 109.28b, 40c
<i>sāvarṇi</i>	1	<i>sāvarṇa</i>	6	Sāvarena	7.5a
<i>sunāmā</i>	1	<i>sunāmnī</i>	1	Sunāmā	27.27d
<i>surabhi</i>	5	<i>surabhī</i>	1	Surabhi	3.91d
<i>suhmaka</i>	1	<i>suhma</i> (pl.)	1	Suhmaka	80.13c

### SOME SPECIFIC TRANSLATION DECISIONS

A very large number of verses and passages could potentially be discussed under this subheading, since there are very often ambiguities or difficulties in the Sanskrit. However, here I propose not to enter into the details of most of these, but instead to prioritise just five instances, presented here in textual order, where the translation difficulties I faced and the interpretive decisions I made were perhaps particularly interesting or consequential.

#### The Rebirth of Nārada (3.7–13)

This passage is tricky, not least because the versions of this story known from elsewhere are different stories (see Wilson 1972 [1840]: 98–99 incl. n. 10; Doniger O’Flaherty 1975: 46–53; Mani 1993 [1964]: 526–27). In the *Harivaṃśa* version, Dakṣa was going to curse Nārada for dispersing his sons but didn’t, because it was instead agreed that Nārada would suffer by being born again (as Dakṣa’s daughter’s son). The compound *śāpabhaya* at 3.8d and 3.13d must thus be understood as ‘the danger that a curse might be cast’ (as at 103.8c), and not as ‘the catastrophe of a curse having been cast’ (as at 8.21c). Once it is realised that no curse is actually cast, the principal problems here are the referent/s of the word *parameṣṭhin*, the number of Nārada’s births described at Hv 3.9, and the identities of the speaker and addressee of 3.12cd where the arrangement is made (crucial personal pronouns occur there). Here is the text, highlighted according to these aspects:

<sup>52</sup> The name *sālva*/*śālva* is standardised to the version known in earlier parts of the *Mahābhārata*. Śālva is an occasionally significant secondary character in the *Mahābhārata*’s first 18 books, and always appears there as *śālva*, with a palatal initial sibilant, whereas in the *Harivaṃśa*, where he is a more minor character, the form is consistently *sālva*, which Monier-Williams lists as a variant of *śālva*.

tāms tu dr̥ṣṭvā mahābhāgān samvivaradhayisūn prajāḥ |  
 devar̥ṣiḥ priyasamvādo nāradaḥ prābravīd idam |  
 nāsāya vacanaṃ teṣāṃ śāpāyaivātmanas tathā || 3.7 ||  
 yaṃ kaśyapaḥ sutavaraṃ **parameṣṭhī** vyajījanat |  
 dakṣasya vai duhitari dakṣaśāpabhayān muniḥ || 3.8 ||  
pūrvam sa hi samutpanno nāradaḥ **parameṣṭhinaḥ** |  
asiknyām atha vairānyām bhūyo devar̥ṣisattamaḥ |  
taṃ bhūyo janayām āsa piteva munipuṃgavam || 3.9 ||  
 tena dakṣasya putrā vai haryaśvā iti viśrutāḥ |  
 nirmathya nāsītāḥ sarve vidhinā ca na samśayaḥ || 3.10 ||  
 tasyodyatas tadā dakṣo nāsāyāmitavikramaḥ |  
 brahmar̥ṣiṇ purataḥ kṛtvā yācitaḥ **parameṣṭhinā** || 3.11 ||  
 tato 'bhisam̥dhiṃ cakre vai dakṣas tu **parameṣṭhinā** |  
 kanyāyām nārado mahyaṃ tava putro bhaved iti || 3.12 ||  
 tato dakṣaḥ sutāṃ prādāt priyām vai **parameṣṭhine** |  
 sa tasyām nārado jajñe bhūyaḥ śāpabhayād ṛṣiḥ || 3.13 ||

In the *Mahābhārata*, the most common referent of the epithet *parameṣṭhin* ('the preeminent one') is Brahmā (Mbh 1.1.30; 1.7.23; 1.58.41, etc.). But the word is also used to refer to Śiva (3.38.44), to Viṣṇu (3.101.11; 12.47.11; 12.64.12; etc.), and, perhaps most significantly for us, to Nārada's father, without further specification of who exactly this is (12.322.5, 6; 12.326.17; 12.331.13, 16). In the *Harivaṃśa*, apart from the passage discussed here (in which the word appears five times, at Hv 3.8b, 3.9b, 3.11d, 3.12b, and 3.13b), the word *parameṣṭhin* only occurs twice, at 7.39c and 31.15b. At 7.39c it refers to the father of the four Merusāvarṇa Manus – that is, to Kaśyapa, since these four are also said in the next verse to be Dakṣa's daughter's sons; and at 31.15b its reference is slightly unclear.

In our passage, who is the *parameṣṭhin*? At 3.8, where Nārada's new birth from Dakṣa's daughter is described, *parameṣṭhin* occurs alongside other words in the nominative case, *kaśyapaḥ ... parameṣṭhī ... muniḥ*, together with the aorist form *vyajījanat* – which is in the singular, and thus there is apparently no implicit *ca* and no double agent. Thus in its first appearance in the *Harivaṃśa* and in our problematic passage, the word *parameṣṭhin* unambiguously refers to Kaśyapa as the new father of Nārada. This is the case also in its final appearance here at 3.13b, where the same circumstance is described, with the extra detail that Dakṣa gave away his daughter to (dative case) *parameṣṭhin* to this end.

When the word appears at 3.9b, in the ablative or genitive case (*parameṣṭhinaḥ*), it seems to be an ablative referring to the *old* or *first* (*pūrvam*) father of Nārada. The vulgate has instead the instrumental *parameṣṭhinā*, which would yield the same sense. The obvious candidates for *parameṣṭhin* here are Kaśyapa, because he has just been called *parameṣṭhin*, and Brahmā, because he is the most common recipient of this epithet elsewhere. Let us leave the identity of *parameṣṭhin* at 3.9b open for now. We shall return to it below.

In its next two appearances at 3.11d and 3.12b, *parameṣṭhin* refers first to the person who together with the brahmin seers solicits (root *yāc*) the furious Dakṣa, and then to the person with whom Dakṣa makes the agreement (*abhisam̐dhi*) encapsulated in the line 3.12cd. These two are presumably one and the same person, and again the two most obvious candidates are Kaśyapa and Brahmā, for the reasons just stated. As for the agreement at 3.12cd, *kanyāyāṃ nārado mahyaṃ tava putro bhaved*, it could be addressed to Kaśyapa, whose son (*tava putro*) Nārada will now be, or perhaps it could be addressed to Dakṣa, whose son – qua daughter’s-son and replacement son – Nārada will now be. And who is speaking? If Kaśyapa is the addressee of 3.12cd then Dakṣa could be the speaker, for whose sake (*mahyaṃ*) the agreement is made, and whose daughter (the dative *mahyam* could also be read in a genitive sense, Oberlies 2003: 332) will be impregnated accordingly. And whether Kaśyapa or Dakṣa is the addressee, if *parameṣṭhin* at 3.11d and 3.12b is Brahmā then the speaker here could be Brahmā, for whose sake the arrangement is made (since he was the one to solicit Dakṣa in the first place).

I think it fits the Sanskrit much better if the speaker of 3.12cd is Dakṣa, since he is the character making the agreement in the nominative case in the previous line (*bhisam̐dhiṃ cakre vai dakṣas*), to whom the utterance would thus most naturally be attributed. And working outwards from here, if Dakṣa is the speaker he cannot also be the addressee, so this must be Kaśyapa. And if the utterance in which Dakṣa makes the agreement is addressed to Kaśyapa, then how would this be an agreement that Dakṣa made with Brahmā and not with Kaśyapa? Thus it follows that *parameṣṭhinā* at 3.12b would be Kaśyapa rather than Brahmā, and the same would be true of the *parameṣṭhinā* at 3.11d, who solicits Dakṣa. Translating verses 11–12 without reference to Brahmā makes the scene a lot simpler than it would otherwise be: Kaśyapa has to be involved, since he is the one who will impregnate Dakṣa’s daughter and become Nārada’s new father, but there is no need for Brahmā to be involved. With the brahmin seers to accompany him, Kaśyapa is perfectly capable of mollifying Dakṣa, and the plan that is hatched does not need Brahmā to hatch it.

Wilson’s understanding of the *Harivaṃśa* story has Brahmā mollifying Dakṣa: ‘Daksha, being about to pronounce an imprecation upon Nārada, was appeased by *Brahmā* and the Ṛshis, and it was agreed *between them* that Narada should be again born, as the son of Kaśyapa, by one of Daksha’s daughters’ (Wilson 1972: 98 n. 10, italics added). Dutt’s translation is similar (translating our verses 11–12):

When Daksha, of immeasurable prowess, was ready to destroy Nārada, *Parameshti (Brahmā)*, with leading saints before him, begged him (not to do it) ... Thereupon Daksha made this contract *with Parameshti* that *his son* Nārada would be born as the son of his (Daksha’s) daughter.

(Dutt 1897: 11, italics added)

In Dutt’s translation, it seems to me that Dutt reports Dakṣa making the agreement in a speech to Brahmā, and so the *tava putro* (‘your son’, which Wilson has omitted in his paraphrase) would be Brahmā’s son. That is, the optative mood of *bhaved* in v. 12d would

apply not to *tava putro* but only to (some part of) *kanyāyām nārado mahyaṃ*, since Nārada, in Dutt's estimation, is at this point a son of Brahmā.

Debroy is no help here, since by ignoring case endings he interprets *parameṣṭhin* at 3.8b and 3.9b as Nārada, and at 3.11d, 3.12b, and 3.13b as Viraṇa. Debroy thus manages to devise a new scenario, certainly not described in the Sanskrit and rather confusing to the reader, whereby the reference to Asiknī and Viraṇa at 3.9c seems to fit in. In Debroy's version Viraṇa gives his daughter Asiknī to Dakṣa in order to give birth to Nārada, when Asiknī and Dakṣa are in fact already enough of a couple to have had several thousand sons who were lost thanks to Nārada.

Nonetheless Debroy's version does make some slight sense of 3.9cd, which is one of the remaining problems for our interpretation. The three lines of 3.9 contain, respectively, the words *pūrvam*, *bhūyo*, and *bhūyo*, and so it would seem that perhaps three different births of Nārada are described here in three lines, with the third presumably being the birth from Dakṣa's daughter as per 3.8. Dutt, by glossing out one of the occurrences of *bhūyo*, reads the last two lines of 3.9 together, whereby Kaśyapa and Asiknī produce Nārada at 3.39c–f, but this is not Nārada's birth from Dakṣa's daughter described at 3.8. Thus according to Dutt, 3.9 describes two births of Nārada, but not yet the birth he is about to undergo instead of being cursed:

Nārada was formerly begotten by Brahmā ; and then that foremost of celestial saints (Kaśyapa) again begat that best of ascetics on Asikni, daughter of Virana.  
(Dutt 1897: 11)

The second of these two births cannot be Nārada's new birth from Dakṣa's daughter (mentioned in the previous verse), because, as Wilson points out, 'Asiknī is the wife, not the daughter, of Dakṣa' (Wilson 1972: 98–99 n. 10); but in Dutt's scenario there are still actually three births. If one is prepared to gloss out one of the occurrences of *bhūyo*, a further option might be to split verse 3.9 in half at the middle of its second line, whereby there would only be two births of Nārada in total, both of them described in 3.9, the first his birth from Asiknī and *parameṣṭhin*, and the second his birth from Dakṣa's daughter and Kaśyapa.

In any case, the fact remains that whether it is his first or his second birth, Nārada's penultimate birth, in which he was responsible for eliminating Dakṣa's sons, is a birth from Asiknī, whom we know from 3.5–6 as Dakṣa's wife and the mother of his children. And the detail that Nārada is Asiknī's son may serve as a key to the passage. If as well as her sons by Dakṣa, Asiknī also has another – implicitly older – son, then a rivalry between that son and her sons by Dakṣa is easy to imagine, as is the swiftness of Dakṣa's antipathy towards him. Thus although Nārada's character as presented elsewhere (he is fond of dispensing speeches which cause strife for others) would perhaps be sufficient motive on its own for his actions here, an additional motive is now evident, and it is a motive that is congruent with the way the problem is corrected when Nārada becomes Dakṣa's descendant.

The remaining pieces of the jigsaw are the identity of *parameṣṭhin* (Nārada's first father) at 3.9b, and the question of how many births are described in 3.9. Thus far, we

understand all four other appearances of *parameṣṭhin* in these verses as referring to Kaśyapa, and we understand that the curse of Dakṣa upon Nārada was averted because of the intervention of Kaśyapa, not Brahmā. Now, one of the points presumably in favour of the common idea that it was Brahmā who would have made the intervention is that Brahmā's job is to oversee the cosmos as a whole and respond if things go awry. So what can Kaśyapa's motive be if it is rather he who intervenes, as we suspect?

Kaśyapa is not known for interfering in other people's affairs. But he does have affection for his children, as we see at Hv 8.4, where at the birth of his son the sun he makes, out of affection (*snehād*), the utterance that gives Mārtaṇḍa his name. So having interpreted all but one of the appearances of *parameṣṭhin* in this passage as referring to Kaśyapa, we may now go the full hog and interpret its appearance at 3.9b in that manner as well, and simultaneously read verse 3.9 as describing the only two births of Nārada mentioned in this passage, the first being his birth from Kaśyapa and Asiknī, and the second being his birth from Kaśyapa and Dakṣa's daughter.

In the end, then, I split the three-line verse 3.9 in half at the middle of its second line, I do not take the two instances of *bhūyo* as indicating two births subsequent to the first, and I take the *devarṣisattamaḥ* of 3.9d to be Kaśyapa, not Nārada (he would only have been Nārada had each line of 3.9 described a different one of Nārada's births).

Thus in terms of the commentaries of Sūradāsa (quoted in the apparatus to the *Harivaṃśa* edition, Vaidya 1969: 18) and Nīlakaṇṭha (Kinjawadekar 1936: 21), I agree with the commentators' suggestion that the *devarṣisattamaḥ* is Kaśyapa (as stressed by Wilson 1972: 99 n. 10), but I disagree that Brahmā is mentioned in this scene. The only prompt for Brahmā's introduction is the repeated occurrence of the word *parameṣṭhin*, but that word clearly refers to Kaśyapa on its first occurrence at 3.8b, and there is no reason not then to take it to refer to Kaśyapa throughout the passage – indeed, I would say that unless one takes it to refer to Kaśyapa throughout the passage, one changes the story. The problem that this story deals with is that two lines through Asiknī, one from Kaśyapa and one from Dakṣa, threaten to compete and destroy each other; and the solution is that those two lines are merged into one line in which both Dakṣa and Kaśyapa have a legitimate stake.

#### Vivasvat's Sons (8.44, 47)

Hv 8 describes the family of Vivasvat Āditya, the sun. First we are told that Vivasvat had three children from his wife Saṃjñā: Yamunā, and two *prajāpatīs*, Manu and Yama (8.6–7). Then Saṃjñā leaves her husband, setting up in her place her shadow and lookalike Savarṇā. Vivasvat, suspecting nothing, has a further son from this second woman, and because that son looks like the first Manu, he is named Manu too – Manu Sāvarṇa (8.16–17).

At this point Vaidya's apparatus reads as follows: 'After 17, K Ñ2.3 V B Dn Ds D1.2.4–6 T G M4 read 47<sup>cd</sup> repeating it in its proper place. D3 reads 47<sup>cd</sup> after 17' (Vaidya 1969: 61). Now, 8.47cd reads as follows: *dvitīyo yaḥ sutas tasya sa vijñeyaḥ śanaiścaraḥ*. When this line appears here after 8.17 in so many manuscripts, the apparatus to 8.47cd says that almost all of those manuscripts read *tasyāḥ* instead of *tasya* (p. 65). In context after 8.17, the *tasyāḥ* refers to Savarṇā (mentioned in 8.16), and Śanaiścara is specified as her second son (*dvitīya*

*suta*). So now there are four sons, two from each woman. Hence Dutt’s translation of the vulgate (verse numbers removed):

Then taking the second Sajnā for the (real) one, Aditya begat a son on her after his own self. This lord was like the first-born Manu and people designate him as Manu Sāvarni. He became Manu Sāvarni. Her second son was known by the name of Sani.

(Dutt 1897: 41)

This expanded version is not quite smooth, though, because the text immediately relates how the deception was eventually uncovered: because the second woman clearly loved her own son more than she loved the other children. Even in the manuscripts which duplicate 8.47cd after 8.17 (and so have *two* new sons), this favoured son is in the singular (*svasya putrasya*, 8.18b; *yavīyāṃsam*, 8.22d); and this is the case in Dutt’s translation too.

The anticipatory duplication of 8.47cd after 8.17 is probably due to a difficulty thrown up later in the chapter, which was solved by hypothesising an extra son. This difficulty occurs at 8.44c, and at 8.47cd. Here are the relevant verses:

*manuḥ prajāpatis tv āsīt sāvarṇaḥ sa tapodhanaḥ |*  
*bhāvyaḥ so ’nāgate tasmin manuḥ sāvarṇike ’ntare || 8.43 ||*  
*meruṣṭhe tapo nityam adyāpi sa caraty uta |*  
*bhrātā śanaīścaraś cāsya grahatvaṃ sa tu labdhavān || 8.44 ||*  
...  
*manur ity ucyate loke sāvarṇa iti cocyate |*  
*dvitīyo yaḥ sutas tasya sa vijñeyaḥ śanaīścaraḥ || 8.47 ||*

On the face of it, 8.43 and 8.44ab describe Manu Sāvarnā, and so when 8.44c mentions *bhrātā śanaīścaraś cāsya*, the *asya* seems to refer to Sāvarnā and the *pāda* seems to mention – or, without the addition after 8.17, to introduce for the first time – a fourth brother called Śanaīścara (after Manu Vivasvata, Yama, and Manu Sāvarnā).

As far as 8.47cd is concerned, when it occurs here the *tasya* stays as it is (and most manuscripts read *manor bhrātā* for *sa vijñeyaḥ*; Vaidya 1969: 66), so the difficulty is to understand how Śanaīścara could be *dvitīyo yaḥ sutas tasya*, ‘his second son’. The *tasya* must refer to Vivasvat, since no other male person’s sons have been mentioned in this chapter, but *his* second son is Yama (8.6–7). Śanaīścara, qua Sāvarnā’s brother (8.44c), would be Vivasvat’s *fourth* son.

In this situation, the manuscripts that included this line already after 8.17 (with *tasyāḥ* instead of *tasya*) have a precedent for Śanaīścara being called *dvitīya suta* – in the sense of being Sāvarnā’s second son. So in those manuscripts the difficulty is significantly reduced, and the description of Śanaīścara as Vivasvat’s second son is understood in terms of his being Vivasvat’s second son *from Sāvarnā*.

Our problem is how to understand 8.44c and 8.47cd when 8.18 follows straight after 8.17, as per the critically reconstituted text. Menon and Debroy's translations of that text have Vivasvat with four sons: both Menon and Debroy take the *asya* at 8.44c to refer to Sāvarṇa, and by implication they both leave the reference to Śanaiścara as Vivasvat's 'second son' at 8.47c to be interpreted by the reader as referring to his second son from Savarṇā (Menon 2008: 19; Debroy 2016: 36–37). But in the absence of the repeated line after 8.17 this is very awkward, and reader confusion is the likely result.

What other option is there? Well, whether 8.47cd is repeated after 8.17 or not, there is evidence of an account with no fourth son, since as mentioned, the son whom Savarṇā loved more than the others is singular. So I have attempted to translate the passage without introducing a fourth son. This involves equating Sāvarṇa with Śanaiścara.

A possible consideration is that after the story has finished at 8.40, in the remaining verses of the chapter Vaiśaṃpāyana runs through the chapter's main characters in summary fashion. 8.41–42 describe Yama; 8.43–44 (as per the discussion above) describe Sāvarṇa and/or Śanaiścara; 8.45 describes Saṃjñā's father Tvaṣṭṛ; 8.46 describes Yamī-Yamunā; and 8.47 again describes Sāvarṇa and/or Śanaiścara, before the chapter concludes with a *phalaśruti* at 8.48. The doubled and/or split description of Sāvarṇa and/or Śanaiścara (at 8.43–44 and 8.47) is notable, but even more notable is the fact that Vivasvat's firstborn son Manu Vaivasvata is not included in this summary catalogue of characters. He is by far the most important child of Vivasvat as far as the following chapters are concerned, so his absence here would be odd.

My strategy is to read 8.43a and 8.43b as describing different people: Manu Vaivasvata and Manu Sāvarṇa, respectively. The former is called a *prajāpati*, just as he was twice in 8.6–7, and the latter is called an ascetic (*tapodhana*). So Manu Vaivasvata is included in this summative passage as one would expect, and crucially he is then also available to be the referent of the word *asya* at 8.44c. Thus, rather than 8.44c describing Śanaiścara as Sāvarṇa's brother, it can describe Śanaiścara-Sāvarṇa as Manu Vaivasvata's brother. Indeed, the word *śanaiścara* might here be understood as a nickname for Sāvarṇa, given to him because of his being slower in taking up the role of Manu than his eldest brother is, quite apart from it also being the name of the (slow-moving) planet Saturn.

Continuing into 8.47 with this interpretation, we can still take the *tasya* at 8.47c to refer to Vivasvat, but by bringing the relative pronoun *yaḥ* into play we can read the first and third *pādas* of this verse together. Thus the otherwise problematic *dvitīya suta* is 'the second son of his who is called Manu' (*manur ity ucyate ... dvitīyo yaḥ sutas tasya*), and the verse states in its second and fourth *pādas* that this second Manu is called (Manu) Sāvarṇa and can (also) be identified as Śanaiścara, the planet Saturn. The 'can (also) be identified as' (*sa vijñeyah*) is replaced in most manuscripts by *manor bhrātā* (Vaidya 1969: 66), but both of these variants are probably compatible with the three-son and the four-son interpretation.

#### The Removal of the Foetus (48.3–4)

This issue concerns the prehistory of Baladeva's birth. The overall account runs as follows. Vasudeva has two wives, Devakī and Rohiṇī. Kaṃsa is told by Nārada that Devakī's eighth

child will kill him (Hv 46.15), and so he resolves to kill all of Devakī's children (47.1–2). Devakī's first six children are killed at birth. The seventh is transferred, in the seventh month of the pregnancy (47.30), from Devakī's womb to Rohiṇī's by the goddess Sleep, and is then born in the cattle station outside Mathurā (49.1) and grows up as Baladeva, son of Rohiṇī. Because Devakī is no longer pregnant, Kaṁsa thinks she has miscarried her seventh child out of fear (*patito devakīgarbhaḥ saptamo 'yaṁ bhayād iti*, 47.32ab). Devakī's eighth child is Kṛṣṇa, and he is born after just eight months of pregnancy (on this circumstance, cf. Selby 2008: 54), but at the same time as Devakī gives birth to him Yaśodā gives birth to a girl (48.11), and Vasudeva succeeds in switching these two infants around (48.18–19). Thus when Kaṁsa kills what he thinks is Devakī's eighth child, he fails to kill Kṛṣṇa. Vasudeva then tells Yaśodā's husband Nanda to take Yaśodā and the son that they think is theirs to the cattle station, and, in his own absence, to look after Rohiṇī's son too (49.2–12).

Our focus here is on the question of what exactly happened when Sleep transferred the unborn Baladeva from Devakī's womb to Rohiṇī's. Squeamish readers or those who have been affected by miscarriages or abortions should be warned at this stage that what follows will require the reader to think about dead babies and weird gynaecological procedures. This section of the report is certainly not intended to be upsetting or disturbing, but please do not read it if you think you might find it so.

Here are the salient lines, together with a deliberately non-committal translation. The verses that concern us in particular are verses 3 and 4, so I have highlighted these.

*āpannam saptamaṁ garbham sā nināyātha rohiṇīm* || 48.2cd ||

*sārdharātre sthitam garbham śātayantī rajasvalā* |

*nidrayā sahasāviṣṭā papāta dharaṇītale* || 48.3 ||

*sā svapnam iva taṁ dr̥ṣṭvā garbham niḥsrtam ātmanaḥ* |

*apaśyantī ca taṁ garbham muhūrtaṁ vyathitābhavat* || 48.4 ||

*tām āha nidrā samvignāṁ naiśe tamasi rohiṇīm* |

*rohiṇīm iva somasya vasudevasya rohiṇīm* || 48.5 ||

*karṣaṇenāsya garbhasya svagarbhe cāhitasya vai* |

*saṁkarṣaṇo nāma śubhe tava putro bhaviṣyati* || 48.6 ||

*sā taṁ putram avāpyaiva hr̥ṣṭā kiṁcid avānmukhī* |

*viveśa rohiṇī veśma suprabhā rohiṇī yathā* || 48.7 ||

When the seventh child was on the way, [Sleep] took it to Rohiṇī. <sup>3</sup> **In the middle of the night, Sleep suddenly entered her. As she was divested of her established foetus, she fell to the ground, bleeding.** <sup>4</sup> **As if in a dream, she saw the foetus that had come out of her body. A short while later she couldn't find the child, and she became distressed.**

<sup>5</sup> Vasudeva's Rohiṇī was as [precious to him as] the moon's [favourite wife] Rohiṇī [was to *him*]. Rohiṇī was frightened, and in the darkness of the night Sleep said to her: <sup>6</sup> And because this foetus that's been deposited in your womb



was extracted [from another], you'll have a son named Saṃkarṣaṇa the Extraction, pretty woman.

<sup>7</sup> Rohiṇī was overjoyed to receive that son, but when she entered the house shining like [the star] Rohiṇī, she lowered her face slightly.

Here are some previous translations of the two tricky verses 48.3–4:

At midnight, the pregnant Rohiṇī let her embryo fall from her womb; then she was overcome by Sleep, and she fell to the ground. As if in a dream, she saw the embryo slip out of her and when she could not see the embryo inside she became confused and distressed for a moment.

(Doniger O'Flaherty 1975: 211)

En plein milieu de la nuit, Rohiṇī qui s'était mise à avoir des épanchements de sang, fut soudain possédée par Nidrā et tomba sur le sol en avortant de l'enfant qui se trouvait en elle.

Elle avait cru voir en songe l'enfant sortir de son corps; n'apercevant pas l'enfant [à son réveil], elle fut un instant troublée.

(Couture 1991: 192)

The vulgate reads *ardharātre* for *sārdharātre* at 3a, *pāṭayantī* for *sāṭayantī* at 3b, and *sve garbhe garbham ādadhat* for *garbham niḥsṛtam ātmanaḥ* at 4b (Kinjawadekar 1936: 163; this doesn't quite match the apparatus for Dn given at Vaidya 1969: 328–29). None of these changes are particularly consequential:

Once on a time at the dead of night while Rohini was sleeping a sound sleep there took place a discharge of blood followed by an abortion (3). Rohini, in a dream, saw the falling of her embryo and when she awoke, a little after, she was greatly pained on not seeing it (4).

(Dutt 1897: 254–55)

The translations of Doniger, Couture, and Dutt are very similar to each other, the main difference being that at 3c Dutt reads *nidrayā ... āviṣṭā* to mean that Rohiṇī was overcome by sleep, rather than by the goddess Sleep. Setting that detail aside, all three translators interpret these verses as being about Rohiṇī, and they make this interpretation explicit in their translations (as does Nīlakaṇṭha in his commentary: *rajasvalā rohiṇī*, Kinjawadekar 1936: 163). It seems, according to their view, that at the time of the transferral of the unborn Baladeva from Devakī to Rohiṇī, Rohiṇī herself was already pregnant.

Couture also makes this interpretation explicit elsewhere. See Couture and Schmid 2001: 174 (the goddess 'then has to transport the seventh son of Devakī into the womb of Rohiṇī, another of Vasudeva's spouses who lives in a neighboring cowherd settlement

where she is in the seventh month of pregnancy’) and 175 (‘The goddess next takes control of Rohiṇī, who believes that she is dreaming when she sees the child she is carrying come out of her body. This miscarriage is immediately camouflaged by the goddess, who transports the seventh son of Devakī into Rohiṇī’, ‘She extracts the embryo [from Devakī] and transfers it into Rohiṇī, whom she has caused to miscarry’); see also Couture 2009: 14 (‘The goddess provokes the miscarriage of Rohiṇī’s child’) and 26 (‘Devakī’s seventh embryo was drawn from ... her womb before being inserted into Rohiṇī’s womb, whose own baby is said to have “fallen”’).

On this view, just as Devakī had an unborn child removed from her womb in order to be transferred to Rohiṇī, so also Rohiṇī had an unborn child removed from her womb in order to make room for Baladeva, who was inserted in its place. Thus the verses 48.3–4 would describe the removal of Rohiṇī’s preexisting unborn child, and apart from the vague overview line 48.2cd, there would be no description of the removal of the unborn Baladeva from Devakī, or of his insertion into Rohiṇī once there is room for him there. Yet by the time Sleep addresses Rohiṇī in 48.6, all three operations have taken place.

Let us imagine this scenario a bit more closely. What happens to Rohiṇī’s original foetus? We are not told. Whether she is awake or asleep during her two operations, Rohiṇī senses that the first one has occurred (i.e., that she has miscarried), and she is disturbed that the foetus that has left her body is not in evidence. So it has gone. The goddess Sleep must have made it vanish. Nīlakaṇṭha says that ‘As soon as the foetus came out of her, she couldn’t see it because it had been made invisible by magic’ (*patitam api garbham apaśyantī yogamāyayā tasyāntardhānaṃ nītatvāt*, Kinjawadekar 1936: 163). Indeed, if the Rohiṇī trick is to be fully convincing and the son to which Rohiṇī gives birth is to be taken for the one she was already pregnant with, then Sleep would have good reason to remove the miscarried foetus. Likewise, if the Devakī trick is to be fully convincing and Devakī is to be deemed to have miscarried (this is after all what Kaṃsa is intended to conclude, 47.32ab), one might think that Sleep’s best move would be to take the foetus that was removed from Rohiṇī, and leave it with Devakī. If I were Kaṃsa and I had resolved to kill all of Devakī’s children, and I was then told, after killing six, that the seventh had miscarried in its seventh month, I think I might want to see the evidence. But maybe it is not within Kaṃsa’s power to do so.

The text doesn’t tell us what Sleep did with the miscarried foetus; and when Viṣṇu gave Sleep her instructions some years earlier (47.26–56), he apparently didn’t mention this detail then either. But by reporting that Rohiṇī noticed the miscarried foetus wasn’t there when she expected it to be, the text raises this issue. That is, it raises this issue *if the woman described in these two verses is actually Rohiṇī*, and if Rohiṇī was already pregnant. In fact there is no statement in the text telling us that Rohiṇī was already pregnant, and the only thing that can explicitly suggest she would have been is if she is the woman described in these two verses – since clearly the verses describe a pregnant woman losing her baby. But the verses don’t name the woman: Rohiṇī’s name has been added by the translators.

Before we explore the alternative to the scenario presented by Doniger, Couture, Dutt, and Nīlakaṇṭha, it is worth thinking briefly about verse 48.7. This verse describes how Rohiṇī is slightly bashful before her family and tries not to draw their attention to her

great joy. Why? Presumably because her and her junior co-wife were both pregnant (Rohiṇī is said at 25.1 to be Vasudeva's first wife), but now Devakī has miscarried and she herself is still pregnant, and moreover she has it from good authority that her child will be a boy. She has good reason to be overjoyed, but it would be insensitive to express this honestly.

Now let us look at some other translations of 48.3–4:

At midnight hour, as the foetus fell down, The lady had her menstrual discharge, When, suddenly being overtaken, By Nidrā, she fell asleep on the ground.

As in a dream noticing the foetus, In herself, and then its disappearance, Devakī became greatly afflicted, Being unable to see the foetus.

(Menon 2008: 209)

She [a footnote specifies that this is Devakī] was then in her season and was sleeping. When Nidra suddenly entered her in the middle of the night, she fell down on the ground. In her sleep, she saw that the conception had left her. When, in an instant, she could no longer see the conception, she was distressed.

(Debroy 2016: 196)

The various translations differ in their understandings of the word *rajasvalā* as well as in their understandings of whether the woman is said to be asleep in 48.3. But the most obvious difference between the translations is that both Menon and Debroy make it explicit that the woman here is Devakī, not Rohiṇī.

Since the text's only indication that Rohiṇī was already pregnant is found in these verses *if they are taken to be describing Rohiṇī*, Menon and Debroy's translations leave open the question of whether Rohiṇī was already pregnant. If she was, then although, as detailed above, the goddess Sleep must have performed three gynaecological operations (two extractions and one insertion), according to Menon and Debroy the only one described is the extraction of the unborn Baladeva from Devakī. If Rohiṇī wasn't already pregnant, then the operation that is described is one of just two. We can imagine in this case that the final operation – the insertion of Baladeva's seven-month-old foetus into Rohiṇī's womb – is perhaps a rather more difficult operation than it would have been if Rohiṇī had not already (until just now) been seven months pregnant with a *different* foetus. But the goddess Sleep is evidently capable of managing such things.

More seriously, if Rohiṇī wasn't already pregnant, then, as far as any onlookers are concerned, the overall outcome is rather different: instead of two women both being pregnant and then one miscarrying in the seventh month and the other giving birth thereafter to a boy, we have two women, one pregnant and one not, and the pregnant one miscarries, while simultaneously the one that wasn't pregnant suddenly becomes seven months pregnant. The question of what the wider family and friends might make of this – and Kaṃsa too, if he knows of it – is something that the reader must decide for themselves whether to take seriously. From a realistic perspective (if there is any scope for such a

perspective on such an impossible story as this one), the reader might like to hypothesise that Rohiṇī was already pregnant, in order to make the deception more credible. On Menon and Debroy's understanding, such a hypothesis is possible. The implications of such a hypothesis affect also what the reader will imagine at 48.7, where Rohiṇī is said to be bashful in household company. Being bashful because one's co-wife has miscarried and one has not is different from being bashful because one's co-wife has miscarried and one has suddenly become seven months pregnant apparently from nowhere. But without further details about household dynamics and visibility, in principle both possibilities are compatible with what is stated at 48.7, and both possibilities are also compatible with Rohiṇī moving to the cattle station before giving birth (49.1).

Our question, however, is not whether or not Rohiṇī was already pregnant, but how to translate these two verses. Although the Sanskrit doesn't specify the woman's name, all the cited translators do, and it is easy to imagine why: without that service, the reader will be confused. Two women are named in the previous two verses, Devakī in 48.1 and Rohiṇī in 48.2, and so in principle either of them could be the one who is miscarrying here, but leaving the translation ambiguous in this regard is not helpful. So how can we tell which one it is? To my mind, there are three possible considerations suggesting that it is Devakī.

The first consideration is that this is the story of how Baladeva came to be. He is an important character in the *Harivaṃśa*, and Viṣṇu's plan to have him transferred from Devakī's womb to Rohiṇī's is detailed in advance at 47.30–32. In contrast, if a second foetus also had to be aborted in order to facilitate the plan for Baladeva, this is a comparatively minor feature, and the removal of that foetus from Rohiṇī's womb is probably not as worthy of narration as the removal of Baladeva's foetus from Devakī's.

The second consideration is that if that is what is supposed to have happened, it is nonetheless probably enough of a feature for the text's authors to have wanted to make it explicit and tie up any loose ends it might seem to leave. The destruction of Devakī's first six children is provided with a background story (of the *śaḍgarbhas*) that turns what otherwise would have been a sad waste of human life into a part of Viṣṇu's cosmic plan. Likewise, the destruction of Yaśodā's daughter would have been a sad waste of human life if she had not been the goddess Sleep working under cover and subsequently rewarded for her pains by receiving a dwelling in the Vindhya and the acclaim of many devotees across the world. So if the births of Kṛṣṇa and Baladeva had required, in addition to the deaths of these seven newborn infants, the further death of a seven-month-old foetus, one would have expected this circumstance to be narratively ramified. Who was it that took the form of that foetus, and why?

The third consideration is that in 48.4, after the miscarried foetus has left the woman's body, it vanishes. Now, if the woman were Rohiṇī, what reasons we can imagine for the goddess Sleep to make the miscarried foetus vanish? Perhaps in order to spare Rohiṇī's feelings; but the text states that the woman was distressed at its disappearance. Or perhaps in order to facilitate the deception at Rohiṇī's end, so that Rohiṇī's son will be thought to be the foetus she was pregnant with all along; but there is no suggestion of the need for this. Or perhaps in order to transport the dead foetus to Devakī's bed in order to

facilitate the deception at *her* end, as discussed above; but again, there is no suggestion of the need for this. On the other hand, if the woman at 48.3–4 is Devakī, then of course the foetus has to vanish once it leaves her body, since the only reason it leaves her body in the first place is in order to be transported elsewhere and inserted into someone else’s.

It thus makes far more sense, to me, for the woman to be Devakī than Rohiṇī. In my view, the text reads very nicely as a result of this interpretation. We know in advance what the goddess Sleep’s mission is, and we can follow all the steps: the foetus of Baladeva is removed from Devakī and disappears and she is distressed (48.3–4), and the next minute something weird has happened to Rohiṇī and she is frightened, so Sleep explains enough to cheer her up (48.5–6); and as long as Rohiṇī can subsequently front it out and Devakī isn’t asked to produce the body for Kaṁsa, the job, for now, is done.

### The Speech of the Yādavas about Wrestling (75.11–13)

The whole speech of the Yādavas is 75.10–15. Here are the three most difficult verses:

*adbhiś cāpi śramo nityaṁ vineyaḥ kāladarśibhiḥ |*  
*karīṣeṇa ca mallasya satataṁ prakriyā smṛtā || 75.11 ||*  
*sthito bhūmigataś caiva yo yathāmārgataḥ sthitaḥ |*  
*niyudhyataś ca paryāyaḥ prāśnikaiḥ samudāhṛtaḥ || 75.12 ||*  
*bālo vā yadi vā madhyaḥ sthaviro vā kṛśo ’pi vā |*  
*balastho vā sthito raṅge jñeyaḥ kakṣyāntareṇa vai || 75.13 ||*

All three of these verses have difficulties, not least: in 75.11 *kāladarśibhiḥ*, in 75.12 *paryāyaḥ*, and in 75.13 *kakṣyāntareṇa*. There isn’t much to go on, Nīlakaṇṭha doesn’t elucidate (Kinjawadekar 1936: 226), and *in extremis* I have improvised, on the basis of elements of wrestling procedure that I know from my own culture. Here are the other translations:

11. Selon les spécialistes, cet exercice doit toujours se pratiquer [après s’être lavé] avec de l’eau; il faut également toujours s’enduire le corps de bouse.
  12. Que le lutteur soit debout, au sol, ou quelle que soit la position qu’il ait prise, les arbitres doivent veiller à ce que son adversaire procède de la même façon,
  13. On peut être un enfant, un homme d’âge moyen ou un vieillard, un homme chétif ou vigoureux, mais il faut toujours [si l’on veut lutter] sur une arène [selon les règles], être reconnu comme appartenant à des catégories différentes.
- (Couture 1991: 309–10)

Fatigue of the combatants is to be, Removed by spectators giving water, And, the combatants are to be honoured, By supplying the powder of cow-dung. (11)  
Any contestant standing on the ground, Is to be engaged by one on the ground.  
Each one is to fight against another, So it has been enjoined by the umpires. (12)

One who is a young lad or an old man, A middle aged person or a lanky one, Or a mighty one standing on the ground, Is to be matched with one of the same group. (13)

(Menon 2008: 328–29)

Those who know about the progress of time have laid down the rules. What must be done has also been instructed. For exhaustion, there must always be water. Dried cow dung must also be kept ready for the wrestlers. The judges have instructed that the duel must be one by one, stationed on the ground, or stationed in any other mode. If a person is stationed in the arena, depending on whether he is a child, middle-aged, old, thin or strong, a rival must be found for him from within that same category.

(Debroy 2016: 280)

Here is my translation:

<sup>11</sup> Combatants may always use water to stem their fatigue, as the time-keepers allow, and the received custom of wrestlers is always to use cowdung as well. <sup>12</sup> The end of the round may be called by the umpires whether a combatant is on his feet, on the ground, or in any other position.

<sup>13</sup> Be he young, middle-aged, or old, be he feeble or mighty, when a man stands up in the arena he must be judged on the basis of his time in the ring.

#### The Gambling Scene (89.35–41)

In Hv 89, a Vṛṣṇi party travels to Vidarbha for the wedding of Rukmin's granddaughter Rukmavatī to Kṛṣṇa and Rukmiṇī's grandson Aniruddha. Despite the marital links, Rukmin has been nursing a grudge against Kṛṣṇa since the abduction of his sister Rukmiṇī years before (*vispardhann api kṛṣṇena ... rājā*, 'the king was still feuding with Kṛṣṇa', 89.12), and during the festivities he is persuaded to host a dicing match at which he plans to defeat Baladeva. The situation mirrors that of the dicing match in the *Mahābhārata's Sabhāparvan*, with Rukmin playing Śakuni's role as the skilled player whose victory is anticipated by the hosts, and Baladeva playing Yudhiṣṭhira's role as the visitor who is fond of playing dice but rather inept at it (cf. Mbh 2.44.18ab = 2.45.28ab, *dyūtapriyaś ca kaunteyo na ca jānāti devitum*, with Hv 89.20cd, *priyadyūtaś ca rāmo 'sāv akṣeṣv anipuṇo 'pi ca*).

In the game between Rukmin and Baladeva, Baladeva begins with a stake of ten thousand gold coins (*niṣkāṇāṃ tu sahasrāṇi suvarṇasya daśāditaḥ ... baladevo glahaṃ dadau*, 89.27), but he loses repeatedly – we are not told how many throws – until Rukmin wins Baladeva's stake of ten million gold coins (*suvarṇakoṭiṃ jagrāha glahaṃ tasya mahātmanaḥ*, 89.29cd). At that point Rukmin says that Baladeva is beaten and scoffs at him, and the king of Kaliṅga laughs at him. Baladeva is furious, but he maintains his composure, and his only response to the provocation is to propose a final throw, for a stake of a hundred billion gold coins. The crucial but cryptic passage discussed below begins with this proposal:

*daśakoṭisahasrāṇi glaha eko mamāparaḥ |*  
*etaṃ saṃparigrhṇīṣva pātayāḥṣān narādhipa |*  
*kr̥ṣṇākṣāṃl lohitākṣāṃś ca deśe 'smiṃs tvam apāṃsule || 89.35 ||*  
*ity evaṃ āhvayāṃ āsa rukmiṇaṃ rohiṇīsutaḥ |*  
*anuktvā vacanaṃ kiṃcid bādham ity abravīt punaḥ || 89.36 ||*  
*akṣān rukmī tato hr̥ṣṭaḥ pātayāṃ āsa pārthivaḥ |*  
*cāturaḥṣe nivṛtte tu nirjitaḥ sa narādhipaḥ || 89.37 ||*  
*baladevena dharmeṇa nety uvāca tato balam |*  
*dhairyān manaḥ saṃniyamya sa na kiṃcid uvāca ha |*  
*baladevaṃ tato rukmī mayā jitam iti smayan || 89.38 ||*  
*baladevas tu tac chrutvā jihmaṃ vākyaṃ narādhipāt |*  
*bhūyaḥ krodhasamāviṣṭo nottaraṃ vyājahāra ha || 89.39 ||*  
*tato gambhīranirghoṣā vāg uvācāśarīriṇī |*  
*baladevasya taṃ kopaṃ vardhayantī mahātmanaḥ |*  
*satyam āha balaḥ śrīmān dharmeṇaiṣa parājitaḥ || 89.40 ||*  
*anuktvā vacanaṃ kiṃcit prāpto bhavati karmaṇā |*  
*manasā samanujñātaṃ tat syād ity avagamyatām || 89.41 ||*

The passage is difficult, in part, because the rules of the game are obscure and have to be inferred from the action, the main contours of which are as follows. Rukmin makes the final throw of the dice, and loses it (he is *nirjitaḥ*, 89.37). But then Rukmin claims he has beaten Baladeva (*baladevaṃ ... mayā jitam*, 89.38cd). In the last two verses of the quoted passage, an incorporeal voice speaks out (*vāg uvācāśarīriṇī*, 89.40b), asserts that Baladeva has won fairly (*balaḥ śrīmān dharmeṇaiṣa parājitaḥ*, 89.40ef), and explains why. Once the matter has thus been cleared up, in the subsequent verses Baladeva goes on to give violent expression to the anger he has kept a lid on until now: he kills Rukmin with the gaming board, knocks out the king of Kaliṅga's teeth, and wrecks the building. Now, if we are to provide a satisfying translation of the quoted passage, our principal tasks are to determine – and communicate to the reader – on what grounds Rukmin claims that he, not Baladeva, has won, and on what grounds the incorporeal voice refutes this claim.

In Dutt's translation of the vulgate version of the passage, these aspects are not at all clear (I have removed the verse numbers):

‘My next bet is one hundred *koti* gold coins, O king. Throwing red and copper coloured dice in this sinful country<sup>53</sup> take all this.’ Thus addressed by Rohini's

<sup>53</sup> At 89.35f Vaidya's text reads *apāṃsule*, with a negative prefix (thus ‘not dusty/sullied/defiling’), but in many manuscripts the word is not negated: with various adjustments in the preceding syllables in order to fulfil the metre, the recorded variants are *adhipāṃsule* (V2 D6 Dn Ds T4 G1.3), *pāṃsule* (T1.3 G4 M), *avipāṃsule* (G5, where the two negative prefixes cancel each other out), and *supāṃsule* (D3). These manuscripts might be interpreted in terms of the high passion and/or sharp practice that tends to taint

son, Rukshmi, the wretch of a man, did not say anything at first, then saying ‘very well’ he again threw his dice. When the dice, bearing four marks, were thrown by him Rukshmi was rightly defeated by Rāma. But the descendant of Bhoja did not admit it but smilingly said ‘I have won the game.’ Hearing those deceitful words Baladeva was again filled with anger and therefore did not give any reply. Thereupon increasing the anger of the high-souled Baladeva an invisible voice said solemnly like the muttering of clouds – ‘Truly has said the beautiful Baladeva. Rukshmi has been defeated in a fair play. Although they know at heart that they have been defeated still they do not admit it in words. Though Baladeva says nothing, still in fact, he has won the game. This is the truth.’

(Dutt 1897: 509)

According to Dutt’s translation, Rukmin’s strategy is to claim he has won when he knows that ‘rightly’ he hasn’t, with no apparent justification for his lie apart from that he wishes it were true. Although the incorporeal voice says that Rukmin ‘has been defeated in a fair play’, Rukmin has not suggested any foul play, and the incorporeal voice apparently wins out simply by being more authoritative than Rukmin. Though Baladeva is said to have spoken truly, he is also said to have said nothing, which seems to be why the incorporeal voice is necessary (Dutt follows Nīlakaṇṭha here; Kinjawadekar 1936: 305 ad v. 44).

Menon’s translation is as follows (reparagraphed):

‘Here is one other stake I am placing, It is hundred millions of gold coins. Do accept this as the challenge from me, And do cast the dice now, O great monarch! Through the dices of black and red colour, In this land polluted by dark passion’ (35).

With these words did the son of Rohiṇī, Summon Rukmī to the game of gambling. With no words in reply he accepted, ‘Be it so’ and once again with those words (36). The monarch Rukmī in a joyous mood, Accepting the challenge threw the dices. With the dice of four numbers coming up, That monarch had been vanquished in the game (37). By Baladev in the righteous way, ‘Not so’ the monarch told Baladeva.

Controlling his mind with his fortitude, He did not speak anything in reply.

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the playing of dice. Nīlakaṇṭha comments: *adhipāṃsule adhikarajaske krodhakāle dyūtadeśe cety arthaḥ* (Kinjawadekar 1936: 305 ad v. 37). Thieme notes and argues against Lüders’s view that this mention of a dusty place is evidence that in this scene the dice were thrown on the ground (Thieme 1984: 422). Thieme also (ibid.) opines that the word *adhipāṃsula* that appears in the vulgate *Harivaṃśa* is an unlikely compound, and suggests it may have been a corruption of *adhipa* (‘your majesty’) + *aṃsula* (‘radiant’); if so, this brings us close to the meaning of the variant *apāṃsule* that occurs in Vaidya’s reconstituted version. *Apāṃsule* fits well with the description of the perfumed hall in which the game occurs (89.22–23), and with Baladeva’s self-controlled intention to be a good guest. Menon and Debroy, quoted below, both follow the vulgate here.



Then with a smile Rukmī put up his claim, Before Baladeva, ‘I’m the winner’ (38). On hearing the words coming from the king, The words that were untrue and deceitful, Flaming in great anger, still once again, But he did not speak a word in reply (39).

Then a voice was audible with no form, Majestic and sonorous, from the air Which escalated the rising anger, In the heart of Bala, the mighty Lord. ‘The glorious one Bala has spoken true, By fair means this one has been is defeated (40). He might not have put his claim through his words, Through his action, he has still won the game, In his mind he has accepted the claim, Do all of you understand this as true’ (41).

(Menon 2008: 396)

Menon’s translation gives a similar impression to Dutt’s, in that it does not suggest that Rukmin has any grounds for his claim of victory. As in Dutt’s translation, here too the incorporeal voice contradicts itself on the issue of whether Baladeva has spoken truly or not spoken at all, and as in Dutt’s translation here too the apparent reason why the incorporeal voice was necessary was because Baladeva didn’t explicitly claim the victory himself. However, in Menon’s final verse Baladeva is the one who is not saying what he knows to be true, whereas in Dutt’s final verse it is ‘they’ (i.e., presumably, Rukmin and the other who suggested the game).

Now here is Debroy’s translation:

‘My next stake is of ten thousand crores. O lord of men! Accept this stake and throw the black dice and the red dice in this place that is full of dust.’<sup>54</sup> Speaking these words, Rohini’s son challenged Rukmi, who didn’t say anything, but offered a stake again. King Rukmi cheerfully threw the dice and when the four-sided dice had stopped rolling, the king had been defeated. However, he told Bala, ‘Baladeva has not followed dharma in defeating me.’ Baladeva resorted to the patience in his mind and controlling himself, did not say anything. Rukmi smiled and told Baladeva, ‘I have really won.’ Baladeva heard the words spoken by the king, about him having resorted to deceit. Though he was again overwhelmed by rage, he did not say anything in reply. At this time, a deep and invisible voice that spoke the truth was heard from the sky and this increased the great-souled Baladeva’s rage. ‘The prosperous Bala has won through the use of dharma. Even though he has not said anything, he has obtained success through his deeds. It should indeed be considered that he mentally accepted it.’

(Debroy 2016: 332–33)

In Debroy’s translation it is clear that Rukmin’s claim is based on an appeal to *dharma*, which is then expanded into the claim that Baladeva ‘resorted to deceit’, although it is not

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<sup>54</sup> Debroy’s footnote (2016: 332 n. 918): ‘Alternatively, in this place that is full of the rajas quality’.

clear how (Debroy here misconstrues *jihmaṃ vākyam* at 89.39b, which is simply a reference to Rukmin's 'lying words'). In Debroy's translation it is the incorporeal voice that tells the truth, not Baladeva, which thus spares Baladeva the nonsense of having to tell the truth while not speaking. In refuting Rukmin's claim, the incorporeal voice simply states that Baladeva has won through *dharma*, and that by implication Rukmin's claim is false, but no further explanation is given. As with Menon's translation, the final line in Debroy's translation focuses upon Baladeva's unstated mental conviction that he has won, although it is not clear how this would be relevant.

None of these translations give a comprehensible account of what has happened. Menon makes a start when he says that 'The monarch Rukmī in a joyous mood, *Accepting the challenge* threw the dices' (Menon's translation of 89.37ab, italics added). Debroy comes closest, but this is not evident in his above-quoted translation but only in a footnote, placed at the end of the penultimate sentence of the quoted passage (after the word 'deeds'), as follows:

This needs explanation. Rukmi had flung the dice, without waiting for Balarama to offer a stake, or accept the challenge. Balarama had not said anything. Hence, the formalities of the challenge being accepted had not been completed.

(Debroy 2016: 333 n. 920)

This footnote doesn't seem to fit with Debroy's translation, because it is hard to square Baladeva's silence with the idea that he 'resorted to deceit'. Also, the footnote doesn't fit the Sanskrit text, because Baladeva *did* offer a stake, and since it was Baladeva who made the challenge he could not really have *accepted* the challenge, and he offered the stake and made the challenge *before* Rukmin threw the dice. Nonetheless, despite the confusing presentation, the idea presented in the final sentence of Debroy's footnote – that 'the formalities of the challenge being accepted had not been completed' – is the key to the passage. I now present my own understanding of what happened.

After Baladeva names his one final stake (*glaha eko mamāparaḥ*), he offers that stake to Rukmin, saying: *etaṃ samparigrhṇīṣva pātayākṣān*, 'Accept it, [and] throw the dice.' Thus he gives the challenge (*ity evam āhvayām āsa*) to Rukmin (89.35–36ab).

Rukmin, who is now expected verbally to accept the challenge, doesn't reply (*anuktvā vacanaṃ kiṃcid*, 36c). Presumably he is taken aback by Baladeva's wish to continue the game when Baladeva has already lost so consistently, and presumably he is also taken aback by the magnitude of the stake, which eclipses by orders of magnitude what Rukmin has won so far.<sup>55</sup> If they play a final throw Rukmin will of course (he thinks) win, but the

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<sup>55</sup> The stakes began at ten thousand and continued up to ten million with Baladeva losing. Nīlakaṇṭha thinks the stake always had to be divisible by ten (Kinjawadekar 1936: 305 ad v. 39). If each stake was ten times the previous stake, then Baladeva would have lost four stakes: ten thousand, a hundred thousand, a million, and ten million, adding up to 11,110,000. The final stake is 100,000,000,000 in the vulgate and in

consequences may then be rather more serious for Baladeva and his close relatives than Rukmin might wish. Rukmin was persuaded by his cronies to have a dicing match in order to win money from Baladeva and tease him about it, to put him in his place as a visitor, especially in view of what happened with Rukmiṇī. He didn't necessarily want to bankrupt him. But now, if Rukmin accepts and plays this final throw, perhaps he will. At the very least, it will probably be a far more crippling financial blow to the Vṛṣṇis than Rukmin had gone into the game expecting to inflict, and the Vṛṣṇis are, after all, his people's relatives by ancestry, and his own relatives by marriage. So no wonder he doesn't reply. Perhaps he is waiting to see if, once everyone has had a little think about it, Baladeva might be able to take his proposal back, the idiot – which he might presumably do as long as they haven't shaken hands on the bet, as it were.

Baladeva doesn't do that. Maybe that's not what's happening anyway. In any case, the *anuktvā vacanam kiṃcid* at 36c can describe Baladeva just as well as Rukmin, and can probably describe everyone else as well, eyeballing each other. Eventually Baladeva speaks and at 36d says *bāḍham*, 'I mean it.'

My impression (strengthened by the *punaḥ*) is that this *bāḍham* is spoken by Baladeva. In comparison to the previously quoted translations of Dutt, Menon, and Debroy, this is a minority opinion. After Baladeva has named the stake and made the challenge, Dutt's translation reads as follows

Thus addressed by Rohini's son, Rukshmi, the wretch of a man, did not say anything at first, then saying "very well" he again threw his dice.

(Dutt 1897: 509)

Dutt's 'wretch of a man' seems to be a gratuitous addition, but his sense that Rukmin is the subject of both *pāda* 36c (where he does not speak) and *pāda* 36d (where he does) is seemingly followed also by Menon and Debroy. Menon translation says, 'With no words in reply he accepted, "Be it so" and once again with those words' (Menon 2008: 396). Debroy's translation says that Rukmin 'didn't say anything, but offered a stake again' (Debroy 2016: 332). This is a bit obscure, but how could Rukmin 'offer a stake again' at 36d without himself, after a pause, speaking? Debroy perhaps thinks that Rukmin must do something to match the stake that Baladeva has proposed, and that his doing so is indicated by the Sanskrit words *bāḍham ity abravīt punaḥ*. This is clearly a speech act, but perhaps Debroy thinks of it as accompanying the movement of chips or coins. In any case, by interpreting 89.36d as an affirmative speech act made (after the delay of 36c) by Rukmin, the translations of Dutt, Menon, and Debroy mean that the ensuing controversy that prompts the incorporeal voice cannot be about Rukmin's failure to give verbal assent.

I think that the *bāḍham* at 36d is Baladeva effectively repeating his challenge (after Rukmin has significantly failed to accept it in 36c). If it is, then after Baladeva has said he

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Vaidya (Menon's 'hundred millions' is wrong, as is Dutt's 'one hundred koti'), which is more than nine thousand times more than has already been won.

means it, what can Rukmin do then, at his own party? So Rukmin throws the dice. Maybe Rukmin deliberately doesn't comply with Baladeva's request for him to make the statement of acceptance before throwing the dice.

Once he – miraculously – loses the throw, Rukmin immediately points out that he didn't formally accept the bet, so the throw doesn't count (*baladevena dharmeṇa nety uvācā tato balam*, 89.38ab). The word *dharma* here simply refers to the formal rules of the game. So that's that. Again there is a pause, during which Baladeva keeps himself under control, *dhairyān manaḥ saṁniyamya* (89.38c) – which is, to the letter, what he had been doing just before, after Rukmin had teased him and the king of Kaliṅga had laughed, before he proposed the preposterously high stake (*dhairyān manaḥ saṁniyamya*, 89.34c = 89.38c).

You would think Baladeva had earned back a bit of respect for himself in this company, since at the very least he has won one throw, even if it didn't count for money. If it didn't count for money, it wasn't Baladeva's fault that it didn't. And maybe Baladeva thinks it should have counted. Surely if you throw the dice, that means you accept the stake? But if Baladeva thinks that, he doesn't try to argue it out with Rukmin.

But now Rukmin rubs it in that that throw didn't count for money. The other throws did count! Grinning, Rukmin says, 'I've beaten you' (*mayā jitam iti smayan*, 89.38f) – which is true, unless Baladeva can successfully argue that throwing the dice means you accept the stake, against Rukmin's apparent house rules, and in the company of Rukmin's posse, which of course he can't. 'He was filled with anger once again, but he said nothing in reply' (*bhūyaḥ krodhasamāviṣṭo nottaraṁ vyājahāra ha*, 89.39cd).

Then the incorporeal voice speaks. Vaiśaṁpāyana says that it spoke (*vāg uvācāśarīriṇī*, 89.40b), and that it spoke truly (the first two words here, *satyam āha*), saying:

[*satyam āha*] *balaḥ śrīmān dharmeṇaiṣa parājitaḥ* || 89.40ef ||  
*anuktvā vacanaṁ kiṁcit prāpto bhavati karmaṇā* |  
*manasā samanujñātaṁ tat syād ity avagamyatām* || 89.41 ||

Glorious Bala has won, according to the rules.<sup>41</sup> Even if [Rukmin] didn't say anything [to accept the final stake verbally], he accepted [it] in the act, because he must have mentally accepted it [before he threw the dice]. That's [the rule] that must be followed.

When the incorporeal voice speaks it makes Baladeva more angry. Maybe he thinks that Rukmin and company may have known this rule all along. Anyway, he now goes berserk, the killing of Rukmin and the removal of the hall pillar presumably symbolising the financial depletion of Rukmin's house to the benefit of his own.

## Appendix:

### Electronic Text of the (Corrected) Reconstituted *Harivaṃśa*

Version 1: Plain Sanskrit Text

Version 2: Sanskrit Text with Paragraph Breaks to Match Translation

Version 3: Plain Sanskrit Text (Harvard-Kyoto encoding)

All three versions are available for download as searchable Microsoft Word documents from the following webpage: <http://orca.cf.ac.uk/118624/>

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