

FOREWORD

1 GENERAL INTRODUCTION

It is now more than thirty five years since Pierre Lévêque – my doctoral supervisor and, later, close friend – suggested that I put together a collection of several of my articles to appear in the same series that had published *Antigone le Borgne* in 1973. So it came about that *Rois, tributs et paysans* (RTP), containing fifteen articles published between 1972 and 1982, saw the light of day in 1982.¹ This made it possible to make articles published in obscure journals and works more widely accessible. A quarter of a century later (2008) 23 articles, that had appeared in French between 1979 and 1999, were translated into Persian with the title *Central Power and cultural polycentrism in the Achaemenid empire*.²

And now I am in a position to publish a third collection, this time in English, thanks, in the first instance, to my friendship with Amélie Kuhrt, with whom I have worked since the early 1980s, and who has already translated several of my articles published in English, as well as a work on Alexander the Great (*Alexander the Great and his empire*, Princeton UP, 2010). I am extremely grateful to her for having given generously of her time to make this translation. It is due to her that the articles published here will become more widely accessible to the English speaking public. My thanks go also to the Fondation Hugot of the Collège de France, which has generously provided financial assistance for compiling the indices. Further, to Omar Coloru, who has checked the references, drawn up the index of sources, and contributed to the completion of the *Index nominum*. Last, but not least, my thanks go to Josef Wiesehöfer and the Steiner Publishing House for accepting this volume for inclusion in the Series *Oriens et Occidens*.

While several questions recur throughout the collection,³ the articles are organised in five separate parts: two bring together regional studies (I: *Asia Minor*; II:

1 The latest article (“Sources grecque et histoire achéménide”) was, in fact, published there for the first time since, for a variety of reasons, it had not appeared, as originally planned, in an issue of *Annales ESC* 1982/5–6. Among the reviews of RTP, note those by J. Wiesehöfer in *Gnomon* LX (1988): 33–35, R. van der Spek in *BiOr* 47 (1990): 300–303, and I. Svenciskaya & B.I. Vainberg, “The question of ancient and hellenistic Middle Eastern society in the work of P. Briant,” *VDI* 167 (1994): 178–185 (in Russian).

2 The translation was organised by Nahid Forughan, and published by Akhtaran (Teheran) in conjunction with the Institut Français de Recherches en Iran (IFRI); it includes several of the articles published in English here (chapters 1, 2, 3, 5, 8, 12, 16, 21, 23).

3 The subject of relations between central/satrapal power and local sanctuaries is treated in both Part I (chapters 2–4) as well as Part II (chapter 6); *Asia Minor* (Part I) reappears in Parts IV (chapters 16–17, 19) and V (chapter 23–27); and the same is true of Egypt, which figures not only in Part II, but also Part IV, chapter 18.

Egypt); the next two relate to overarching subjects that I have been interested in since the 1970s (III: *The Great King, land and water*; IV: *Communications and exchange*); in the title of the fifth section (V: *The transition from the Achaemenid empire, Alexander and the hellenistic kingdoms*), the reader will immediately recognise one of my most enduring concerns. The chronological range spans the period from 1979 to 2008. The majority of the articles selected date from the 1990s (14) and 2000s (9); two from the 1980s (chapters 1 & 5), just two from the late 1970s (chapters 10 & 20). The last two were, in fact, already included in RTP 1982. Nevertheless, I decided to include the first (chapter 10), as it demonstrates the interest I began to develop in the Persepolis tablets in the course of the 1970s; as for the second (chapter 20), it represents, to my mind, a kind of interim assessment of the theme ‘continuity and change’ (cf. RTP: 11–12). Two of the articles selected (chapters 9 & 17) are notes, little more than a page in length, published in two short-lived journals (*DATA*, *La Lettre de Pallas*); another (chapter 28) presents only the conclusions of a lengthy study running to 150 pages. As for the historical commentary on the customs account from Achaemenid Egypt (chapter 18), this was written in collaboration with Raymond Descat, and I thank him profoundly for allowing me to publish an English translation in a book appearing under my name. Readers should be aware that the analysis and conclusions are attributable to both of us (cf. note 5 of the chapter).

In my introduction to *Rois, tributs et paysans*, I explained that “republishing these studies does not imply that they are free of errors” (p. 7). The same applies here. Like all researchers, I obviously like to think that my publications have stimulated, and will continue to stimulate historical reflections, but as time passes the author is also faced with another fact – namely, that the studies are, and (ideally) will increasingly become, part of the field of historiography. At the point when a study is published, it no longer belongs *stricto sensu* to its author; its life is exposed to the eyes of the readers who are, generally, equipped with solid and sensible critical faculties. For this reason, I decided that it would not be very useful to modify the text nor to add an up-dated bibliography at the end of each article⁴.

It would, of course, be somewhat strange were I to pretend to be ignorant of the reactions that my interpretations have stimulated. I consider these in the following pages, where I discuss each of the five groupings. As part of that, I refer to studies and comments some of which have adopted (totally or partly) my conclusions, and others which have questioned one or other of them and put forward alternative answers, of which it is only right that the reader should be made aware. Further, given that twenty to thirty years or so have passed since their publication, I have changed

4 For the convenience of readers, I have generally cited my *Histoire de l'empire perse* (HEP 1996) in its English translation (HPE 2002). It is nevertheless crucial that the latter be checked, as it is not always exact and sometimes wanders rather far from the French original. Similarly, where certain books have been translated into English, they are cited in accordance with the English translation (e.g. Le Rider 2007; Perrot 2013; the same applies to P. Briant, *Darius in the Shadow of Alexander*, 2015). [Some of the Aramaic texts are cited according to J.M. Lindenberger's 2003 English translation. -AK].

my own ideas, so that some interpretations I proposed originally now strike me as dubious, while I feel in other cases that my original idea has been strengthened. As for the bibliography appended here, this is by no means intended to be exhaustive, as those contained in the *Bulletin d'Histoire Achéménide* I (1997) and II (2001) were;⁵ it relates exclusively to publications dating between 2001 and 2015–16 which discuss and/or add to the articles presented. That explains why, now and again, I have taken the liberty of giving some information on the genesis and background of some of the articles and interpretations, and how I view them now, which explains the autobiographical tone of some passages. Thus equipped, the reader will be in a better position to understand the articles presented here, and/or reconsider them anew.

2 PRELUDE (CHAPTER 1)

As a kind of prelude I have placed a paper which I submitted for the publication of the 1983 *Achaemenid History Workshop*, the proceedings of which were published in 1987.⁶ The paper was quite different from the one presented on the occasion of the colloquium, which was already earmarked to form part of a forthcoming book.⁷ In response to Heleen Sancisi-Weerdenburg's urging, I agreed to submit a new text which, written in 1983–4, presented my overall image at that time of the structures and functioning of the Achaemenid empire, albeit in a preliminary fashion. It may have been that it was with an eye to the book I had (somewhat unwisely) committed

- 5 I have tried to make a new (partial) assessment in my article "The Achaemenid empire" in: *The World Around the Old Testament*, edited by Bill T. Arnold and Brent A. Strawn (Grand Rapids: Baker Academic), 2016.
- 6 The title of the published volume (*Achaemenid History I: sources structures, synthesis*) differs from that of the Workshop programme: 'The last century of the Achaemenid empire: decadence?'
- 7 In 1978–79 J. C. Gardin invited me to participate in the work of his CNRS team (URA no. 10: *Le peuplement antique de la Bactriane orientale*). I was given the task of gathering all the classical texts relating to Achaemenid Bactria in order to compare the textual (classical) sources with the material being produced by the archaeological surveys (cf. below Chapter 20 § 2.4). H. Sancisi-Weerdenburg was very much influenced by the thesis of "Bactrian autonomy" developed by Kuz'mina and Cattenat & Gardin (cf. *Yaunā en Persai*, 1980, ch.4), so that she shared a long-established view, which appeared to be given solid support by the archaeological finds. This explains why, in 1983, she had invited J. C. Gardin to present the survey results. The latter suggested that I stand in for him, and thus it came about that, quite by chance, I took part in the Workshop! I presented on that occasion a paper devoted to my ongoing research, namely *Pouvoir central et autonomies locales dans l'empire achéménide: le cas de la Bactriane*, where I must certainly (I have not kept it in my files) have summarised my work in progress, as I did in November 1983 at the Dushanbe Franco-Soviet Conference. (Heleen's report on my 1983 paper (*Persica* XI [1984]: 189–190) shows, in brief, that she had serious reservations regarding the core of my interpretation on the situation of Bactria within the Achaemenid empire.) In order not to duplicate my book (*L'Asie Centrale et les royaumes proche-orientaux*, 1984), I sent another paper for the Workshop publication (here Chapter 1), where I alluded to the Bactrian question (§ 2 *Text and image*).

myself in 1979 to write for the publisher Albin Michel,⁸ and/or in order to respond to a question raised by the organisers (*AchHist* I: xiv),⁹ that I provided an assessment of my own research into Achaemenid matters since the beginning of the 1970s. Some of the results had remained unpublished; others had been published to suit specific journals and colloquia, and reappeared in *Rois, tributs et paysans* (1982),¹⁰ which is where I developed what Clarisse Herrens Schmidt has called ‘the hard image’ of the Achaemenid empire.¹¹ The reason I have used this paper as an introduction is that it touches on a number of the subjects referred to or discussed below, while not trespassing on any of the regional or thematic categories.

The central issue which I tried to address here is the working of the empire, characterised as it was by its simultaneous unity and diversity. To do this, I suggested excluding two simplifications (the thesis of autonomy and that of centralism), and to draw a distinction between ‘power’ and ‘control’. And it was within this framework that I sketched out my ideas on the basis of a selected set of material that I have returned to subsequently: the relations between Pixodaros and Xanthos (note 5; cf. Chapter 3), Widranga’s action in Elephantine (§ 1.3; cf. Chapter 5 § 3, and Chapter 6), as well as the revolts in Egypt (§ 3.3; cf. Chapter 5) and Babylonia (Chapter 1 § 3.3)¹². I also introduced (§ 2) the methodological problem connected to the inescapable, but often difficult, intersection of written sources and archaeological material (cf. Chapters 13, 15; also 11). It was while working on this article that I presented the concept of the ruling *ethno-classe*, which I had already introduced in my Hellenistic studies (RTP 261–2), but whose full definition and implications I worked out on the occasion of the *Achaemenid History Workshop* in London (1985; cf. chapter 5 § 1.1). I was, at that time, heavily influenced by the work of Louis Robert, which had led me to formulate somewhat debatable ideas on the hermeti-

8 Early drafts of some chapters date from 1983–86.

9 “How should all these divergent and disparate data be assembled into an overall synthesis of the Achaemenid empire?”

10 See also “‘Brigandage’, conquête et dissidence en Asie achéménide et hellénistique,” *DHA* 2 (1976): 163–259, the idea for which emerged from an Achaemenid seminar I had given at the University of Tours in 1972–4. Much of the substance was included in *État et pasteurs au Moyen-Orient ancien*, 1982. Some of the ideas I developed there have been fleshed out by W. Henkelman (2005, 2011) using the Persepolitan documentation; D. Potts (2014, chapter 3) adds little that is new to this question (see the review of Potts’ book by J.-P. Digard 2015).

11 *Abstracta Iranica* 2 (1979), no. 23 (referring to an article of 1976 = RTP 175–225). Matt Stolper 1999 adopted the phrase in the title of the review-article he devoted to *HEP* (“Une ‘vision dure’ de l’histoire achéménide. (Note critique),” *Annales HSS* (1999): 1109–1126).

12 I subsequently returned to the issue of the Babylonian revolts in Xerxes’ reign (*StIr* 1992), using the classical sources, albeit convinced that the answer would eventually come from the Babylonian material: “Use of the classical sources is simply the historian’s last resort” (p. 15). And the question has since been thoroughly re-examined: see Waerzeggers 2003/4; Kuhrt 2010; Henkelman-Kuhrt-Rollinger-Wiesehöfer 2011; Kuhrt 2014b; note also the intriguing text (BM 72747) discussed by C. Waerzeggers 2014, which shows that, at the beginning of Xerxes’ reign (485), there was, in the Sippar temple, a statue of Darius in receipt of regular daily cult offerings.

cism of the ruling *ethno-classe* and “cultural scission”.¹³ Yet I made clear, at the same time, that the contacts and acculturation between Persians and local élites grew and deepened continuously (§ 3.5), a point which I have emphasized throughout (cf. Chapter 5 § 8; Chapter 6 § 2; Chapter 7 § 2.2.; Chapter 27 § 2). Although royal pronouncements attribute a dominant role to Persia and Persians (see my comments in *Topoi* 4/2 (1994): 459–463), and the top positions are held by Perso-Iranians (or individuals with names of an Iranian type), I have to admit (although I do not reject) that the concept of ‘*ethno-classe dominante*’, which has entered the vocabulary of Achaemenid history specialists, requires some clarification, in view of the numerous studies published in the last thirty years on the concept of ethnicity and the realities of intercultural contacts.¹⁴ This was also one of my aims when I co-organised in Paris in November 2007 a colloquium (= Briant & Chauveau, éd. 2009) intended to develop, using regional examples and imperial themes, the programme indicated by the title of section 6 in BHach I (1997) and II (2001): *Peuples, langues et cultures: acculturations personnelles et politique impériale*.¹⁵

As for the question of Persian cultural survival in the Hellenistic period as envisaged by L. Robert, I expressed my strong reservations on this quite early on (Chapters 25 § 4, and 27 § 2.2). My drastically revised thoughts on the Droaphernes inscription (Chapter 2) is the culmination of that process of revision as, on the contrary, it illustrates the cultural contacts between Persians and other Lydian communities.¹⁶ However, despite often bloody dynastic struggles,¹⁷ the unity of the Persians of Persia around the dynasty and its values (Chapter 1 § 4) has always seemed to me to constitute a feature of fundamental importance for understanding the

13 Cf. for instance § 3.1: “Inasmuch as a ruling group is both numerically small and zealously attached to the advantages to be gained from the exploitation of conquered lands, it is well aware of the necessity to keep intact its cultural characteristics and not let them be diluted in some kind of melting pot which would entail a division of power and privileges.” Weinberg’s criticism of this issue is apt, at least in part (cf. BHach II, nn. 401–402). My interpretation had been challenged for the Hellenistic period: e. g. K. Goudriaan, *Ethnicity in Ptolemaic Egypt*, 1988, p. 116, n. 1, together with R. Van der Spek’s comments (*BiOr* 47/3–4 (1990), p. 302–303); I subsequently modified my position in the article “Colonizzazione ellenistica”, 1998; I should add that the Tyriaion inscription from Phrygia published in 1997 is a clear instance of a royal [Atalid] policy aimed at integrating the ‘natives’ (*egkôrioi*) into the framework of a city (see, for example, Ph. Gauthier’s presentation of the text in *BÉp* 1999, n° 509).

14 See e. g. I. Delemen *et al.* (eds.), 2007; Gruen (ed.) 2011: 67–182 (*Perceptions and constructions of Persia*); and my own contribution to the Münster-Colloquium on Asia Minor in 2013 (“À propos de l’empreinte achéménide”, 2015).

15 I presented on that occasion an outline of the argument, with the title “De Sardes à Samarkand. Ethnicité, culture et pouvoir dans l’empire achéménide”. Unfortunately, other obligations have prevented me from publishing the text.

16 I should mention that, on receipt of the offprint, Henri Metzger gave me his reaction in the following humorous way: “I think it rather nice that you find fault with Louis Robert’s pan-Iranism of which he accused F. Cumont in the 50s” (personal letter, 15 February 1999).

17 See my article, “Guerre et succession dynastique chez les Achéménides: entre ‘coutume perse’ et violence armée,” (2002). H. Klinkott’s 2008 article conveniently presents the material on the Persian nobility.

strength of Achaemenid power at the centre and in the provinces, at the very time of Darius and Alexander.¹⁸

3 ACHAEMENID ASIA MINOR (PART I)

The first section is devoted to Asia Minor. For reasons connected to my original involvement with Hellenistic history (which has never ceased),¹⁹ I have worked extensively on this area, which figures repeatedly elsewhere in this volume – whether it be on the question of royal ideology (Chapter 11), exchange and communications (Chapters 16–19), or that of the process of transition (Chapters 20–28). For a very long time Asia Minor, and Anatolia as whole, were the best documented regions of the empire of the Great Kings. The reason for that does not simply reside in the survival of Greek narratives, but also – indeed more so – in the stream of new publications relating to archaeological, iconographic, epigraphic and archival (Daskyleion, more recently Seyitömer: Kaptan 2010) material. This has increased steadily over the last thirty years,²⁰ which explains the prominent place analysis of its structure and diachronic development occupies in *Histoire de l'empire perse*.²¹

- 18 See, too, “Hérodote et la société perse” (1990), “De la ville de Suse au pays des Hanéens” (1994), and *HPE* 302–354 and 842–852.
- 19 As the two first sections of my dissertation focussed on the early phases of Antigonos Monophthalmus’ career, I had inevitably to deal with the satrapy of Greater Phrygia under Alexander and his predecessors (*Antigone le Borgne* 1973: 16–295). The same applies to the articles written at that time, one of them concerned with the *laoi* (1973 = RTP 95–135), the other, published in 1972–3 (RTP 13–93), with the transition from Alexander to the Successors, as many of the examples and documents come from Asia Minor. It was again in 1974 that the first edition of my *Alexandre le Grand* (Que-sais-je? 622, Paris) appeared; I tried already then to reintroduce Darius and the Achaemenid empire into the story of the Macedonian conquest (as Claire Préaux noted in a personal letter (20.4.1974): “The Achaemenids seem to me to be more present in your text than in most histories of Alexander”): see now the expanded English translation (Princeton 2010), and the 8th revised French edition (Paris 2016). Among my studies of Hellenistic history, see, e. g., [with P. Brun and E. Varinoğlu], “Une inscription inédite de Carie et la révolte d’Aristonikos,” 2001.
- 20 See Chapter 27 § 1.1. It would serve no useful purpose to draw up a list of documents relating to Asia Minor and Anatolia published since 2007. The main trends in Anatolian archaeology of the first millennium B. C. are laid out very clearly by L. Khatchadourian 2011 (and in a more a global perspective in 2012); on Lydia, see Roosevelt 2009. See also the recent synthesis by E. Dusinberre, 2013 (with Dusinberre 2016), and the Proceedings of the Colloquium held in Münster in February 2013, *Zwischen Satrapen und Dynasten: Kleinasien im 4. Jahrhundert v. Chr.*, now published in the series *Asia Minor Studien* (Bd. 76, 2015), with my own paper, p. 175–193 (“À propos de l’empreinte achéménide (*Achaemenid Impact*) en Asie Anatolie. Notes de lecture”); on Armenia, see Khatchadourian 2016: 118–193.
- 21 *HEP* 507–521; 572–582; 596–600; 608–618; 626–630; 634–699; 706–709; 718–733; 837–848; 862; 872–876 = *HPE* 491–505; 554–563; 579–583; 591–600; 608–611; 615–675; 688–690; 697–713; 817–828; 842–844; 852–857; since then, see BH Ach I, 15–27, BH Ach II, 32–52 et 148–156.

In the wake of Louis Robert's pioneering work,²² I have focussed on a particular sub-set of material to study, namely Greek and/or multilingual inscriptions.²³ This explains why, in the first part of this collection, the emphasis is on how the historian of the Achaemenid empire can use them, by looking at three examples, one of which (*Letter of Darius to Gadatas*) has been known since 1886, the other two (the Droaphernes inscription and Xanthos trilingual) since 1973–74 (shortly after the discovery of the famous Darius statue at Susa on 23rd December 1972). These three inscriptions relate to a larger dossier of material, which form the subject of a detailed future treatment by myself:²⁴ i.e. the Greek and multilingual epigraphic documentation for Achaemenid Asia Minor. It is one of the projects in which I am particularly interested, but which I have not been able to complete due to pressing obligations, as well as my decision to concentrate on other topics. I should also make clear that while the three inscriptions are discussed in *HEP* 1996, the texts here reflect a later stage of my thoughts concerning them (cf *HPE*, p. xvii, n. 15). It should be noted in particular that, as I myself have observed, the conclusion of my re-examination of the *Letter of Darius to Gadatas* directly contradicts the extensive use I made of it in my book.²⁵

Although the dossier in this section is a partial one only,²⁶ it raises two important questions relating to the empire as a whole. One is the particular place occupied by Aramaic within the multilingual spectrum of the empire (BHach II: 169–176; see also Chapter 27 § 2). This is most obvious in the case of the Xanthos trilingual, discussions of which continue fast and furious (BHach II: 179–182), especially as the Aramaic version echoes the bilingual (Lycian-Greek) civic one, while simultaneously sounding a markedly different note. A connected problem is raised by the two monolingual Greek inscriptions (Darius-Gadatas; Droaphernes), as it is often claimed, as though self-evident, that the Greek text can be nothing more than a translation/adaptation of an Aramaic original – a thesis I reject for a complex of reasons which I explain at some length.²⁷ The second question, frequently raised, is

22 Cf. my *Leçon Inaugurale* (2000): 39–40.

23 See already RTP 95–135 (using Greek inscriptions to reconstruct the status of persons, communities and territory in Hellenistic Asia Minor), and below Chapter 27 § 1.6.

24 “Remarques sur sources épigraphiques et domination achéménide en Asie mineure” (2001): 15. It was while I was preparing that communication for the Bandırma Colloquium (15–18 August 1997), that I began to entertain serious doubts about the authenticity of the *Gadatas Letter* and announced a forthcoming study (p. 15, n. 17).

25 Cf. *HEP* Index, p. 1235, sub ML 12 = *HPE*, p. 1146; see particularly *HEP* 507–509 = *HPE* 493; cf. below Chapter 4, § 5.1: “It is never an easy matter to conclude an analysis of this type. In fact, it is somewhat daunting as any historian is reluctant to advocate the elimination of a document which, until now, has occupied such an important place in historical reconstructions (*including his own!*) from the corpus of material” (ital. P.B.).

26 See also below Chapter 19 (the use of Greek inscriptions to reconstruct exchanges between cities and satraps); Chapter 25 (the Amyzon inscription concerning Bagadates); Chapter 27 § 1.6 (*The contribution of the epigraphic sources*) and § 2.1. (*From Xanthos to Kaunos*).

27 See particularly chapter 2, n. 44; Chapter 4 § 4, particularly note 87 (with now the publication of a tablet in Old Persian by Stolper & Tavernier 2006, who (p. 8) refer to my note); BHach I: 93–4 and II: 171. On one crucial point in the debate (the ‘Aramaising’ dating formula of the

that of the diverse and evolving relations established between local sanctuaries and the Achaemenid administration, at the centre and in the satrapies (see also Chapter 6).²⁸

Differences of opinion on this question persist. In the case of the Droaphernes inscription, particular epigraphic and philological difficulties have led to interpretations that continue to be debated and may be, partly or entirely, irreconcilable.²⁹ They concern both the structure of the document itself (several texts [PB]³⁰ or one), what it actually is (a private dedication [PB] or a regulation promulgated by the

- Droaphernes inscription: below Chapter 2, n. 44), see now P. Thonemann's clarification (*Appendix: "Aramaic" numerals in Greek inscriptions*, 2009: 391–4), which has confirmed my doubts about Louis Robert's interpretation. Thonemann argues that Robert's hypothesis of an Aramaic date translated into poor Greek "is not strictly necessary. On the parallel of the Tralles inscription, the cardinal number [...] can easily be explained as an expansion of an "Aramaic" style cardinal number [...] in an original Greek text." In other words, the idea of actual Aramaic influence does not necessarily mean that one must postulate an Aramaic *original*.
- 28 See especially BHach II: 177–187, and "Histoire impériale et histoire régionale. À propos de l'histoire de Juda dans l'empire achéménide," 2000, as well as the clarifications by A. Kuhrt 2001 and 2007. On the theory of *Reichsautorisation* developed by P. Frei, see the collective volume edited by J. Watts (2001), which contains Frei's own explication, followed by six critical responses. In a recent article (2013), B. Lincoln claims, as though it were an established fact, that "the members of that group [Achaemenid History Workshops] devoted surprisingly little attention to the role of religion." He is undoubtedly thinking of studies based on a combined analysis of royal inscriptions and Avestan texts, but the way it is formulated is ambiguous – quite apart from the fact that, in my view, no "group" with a uniform approach ever existed, despite T. Harrison's somewhat artificial polemic (2011). I should also point out that, contrary to B. Eckhardt (2015: 270 n. 2), I did not "question the *Persian* origin of both documents" (the first section of the Sardis inscription was certainly originally inscribed by a high Persian or Iranian official in Sardis acting in a private capacity); what I questioned was the likelihood of an official action taken by the *Achaemenid* political authorities, which is rather different. S. Mitchell (2008: 157–9), while accepting my understanding of the text, makes an interesting suggestion on the problematical name 'Baradates'. He thinks that, at the time the texts were reinscribed, the stone-cutter slipped up, so that the name is actually the well-known one 'Bagadates' (on which, without reference to Baradates, see Schmitt 2008).
- 29 This has been confirmed by personal letters I have received from the late Paul Bernard (10th January 1999), Philippe Gauthier (18th February 1999) and Peter Hermann (27th March 1999), discussing my suggestions on the Droaphernes inscription, and expressing both selective rejections and partial agreements; see too Ph. Gauthier's comments in *Bull.Épig.* of the *REG* 112 (1999), n°469. In the article cited above (2001: 25–27, n. 15), P. Frei, unaware of my analysis, restates his view that the Greek text is constructed on the basis of an Aramaic original, and that the religious prohibitions date to the Achaemenid period (but accompanies it with the following disclaimer: "or perhaps by later sponsors"); I continue to disagree with both points. I find myself not at all convinced by the explanation-translation recently presented by P. Goukowsky 2009. He thinks that the statue to be honoured "close to the *adyton* or even within it" was one of Artaxerxes II (pp. 321–3), that we are dealing with a "perpetual edict", and that the legislator was the Great King himself (p. 330). On the statue, see also B. Jacobs 2007 (it is that of Droaphernes himself). Concerning the statue of Ariobarzanes at Ilion (Diodorus XVII.17.6), cf. also the remarks of B. Rose 2014: 154.
- 30 See now E. Dusinberre (2003: 118–120), R. Lane Fox (2006: 153) and C. Tuplin (2011: 166 and n. 104), who explicitly accept my analysis and conclusions.

satrapal authorities), and connections (or not: PB) between it and a historical situation – the so-called ‘Satraps’ Revolt.³¹ Among recent studies, the most interesting is the paper by K. Rigsby (2014): the author thinks (as I do) that Parts II and III of the inscription are to be dated to the Roman period, and that the dedication alone is dated to Achaemenid times; but, according to him, *andrias* must be understood as a divine statue, that of Zeus, and that the three parts of the inscription “are in fact related and all concern a statue of Zeus”. Vigorous discussions of the Trilingual continue unabated,³² as, of course, they do about the question of the authenticity (or ‘inauthenticity’) of the *Letter of Darius to Gadatas*. As I foresaw only too plainly when I published it,³³ my position has not persuaded everyone. Since its publication (2003), no less than three studies devoted to the inscription have taken issue with my conclusions or cast doubt on their validity.³⁴ The most recent one is by P. Lom-

31 Note here that M. Weiskopf, who had argued in favour of a link between the inscription and the ‘Satraps’ Revolt’ (*HEP* 1027 = *HPE* 1001), has since revised his position and states that he now agrees with my position: *Topoi* 12 (2002): 451–458. Since (according to L. Robert) the issue also implies a link (debatable in my view) with a famous passage of Berossus quoted by Clemens of Alexandria (*HPE* 676–680; below Chapter 2), let me mention that G. de Breucker has recently questioned my interpretation (2012: 565–566), proposing instead that it was a way of opening the Anahita-cult to non-Persians; see also Jacobs 2013, concluding that “the assumption that the consecration of statues to Anahita by Artaxerxes II was an innovation, is not justified”, but without discussing my specific discussion of this topic, nor treating the issue, also discussed by de Breucker, i. e.: Who were the intended subjects of the king’s decision?

32 See the responses from three colleagues at the end of my presentation at the Académie des Inscriptions et Belles Lettres: cf. *CRAI* 1998: 340–1 (H. Metzger, restating his disagreement), 341 (G. Le Rider), 342–347 (P. Bernard, who, on p. 346, made significant observations on the physical distribution of the three inscriptions [below chapter 3. § 3 Point 1]: “The place of honour was certainly the one reserved for the two texts, Greek and Lycian, which displayed to the eyes of the faithful the contents of the sacred law on the broad sides of the monument. The Aramaic version had been squeezed onto one of the narrow sides as an annexe. Had its function been to give the force of law to its two neighbours, one would have thought that it would have benefited from a more prominent position and so the stone would have been carved to suit that purpose, perhaps as a square pillar rather than a stele”). Among recent studies, see, e. g., G. Maddoli 2006 (a daring but risky attempt to solve the chronological problems); for disagreements with my view, see especially I. Kottsieper 2001, followed by P. Funke 2008 (although not dealing with every point). On the Lycian version, see the new edition put out in 2000 by C. Melchert (<http://www.achemenet.com/pdf/lyciens/letoon.pdf>), and his translation of lines 29–30 (“They shall defer to Pigesere. It (is) for the supreme authority to do what he decrees”), which differs slightly from the way he suggested rendering them previously: “They shall defer (authority) to Pixodaros. The supreme authority is to carry out what he commands/wishes” (*Historische Sprachforschung* 112 (1999): 75–77).

33 Cf. below Chapter 4 § 5.2: “I think it very likely that I will meet with opposition, as it is so difficult to bring proofs that will command unanimous agreement.” See on this, Ph. Gauthier’s opinion who, while expressing some reservations writes: “B.’s arguments strike me as only partly persuasive, but those adhering to the document’s authenticity will henceforth need to take them into account.” (*Bulletin Épigraphique* in *REG* 117/2 [2004], n°293).

34 One is by Lane Fox (2006), the other by Tuplin (2009), (announced in I. Delemen et al. (eds.), 2007: 15). They were recently dubbed “compelling responses” by T. Harrison (2011: 146, n. 147), although he does not bother to explain (however briefly) on what he bases his judgment. Lane Fox prepared his article independently of mine, but, in his *Postscript*, p. 169, says

bardi (2010), who analyses the terminology and semantics of the word *diathēsis* (Il.18–19) in the minutest detail. She cites in their various contexts many Greek literary and epigraphic texts of the classical, Hellenistic and Roman periods. The conclusions she draws is that the term should not be taken in the sense it had in the Hellenistic period (“state of mind”), but in the sense it had in the classical period, namely that of a “(royal) order.” On this basis, she concludes that, at the very least at this point (Il. 17–21), the letter derives from a classical period document, which must be authentic: “It is possible that there was a textual development or perhaps the creation of a dossier, which the sanctuary held in its archives, in which I am sure there existed, in Greek translation, at least the mention of a ‘disposition’ originating with Darius. [...] Perhaps only some of the main points were translated into Greek in the classical period” (p. 168). Her extensive and detailed linguistic and epigraphic knowledge, which is mobilised here, demands respect, but, contrary to her idea, I am not at all sure that such an understanding of the term *diathesis* is enough to solve the question conclusively. It seems to me that arguments I put forward in favour of a learned (late) Greek construction of the document remain relevant (below Chapter 4, especially notes 56–57). One aspect must, however, be stressed: I am convinced that the fact that there continue to be serious doubts means that historians should not treat this text as a prime piece of evidence in discussions, neither in the context of *Persian* epistolography,³⁵ nor that of “Achaemenid agricultural

that he has taken my study into account, which is as much as to say that my arguments have not convinced him to modify his text. He is firmly convinced that there was an Aramaic original but, in my opinion, the full Aramaic ‘reconstruction’ which Alison Salvesen produced at his request (p. 168–9) is illusory. (R. Schmitt, too, did so rather unwisely in 1996: cf. below Chapter 4, n. 101). Rather surprisingly he asserts that, if the (modern) translator “had found [the *Letter*] impossible to translate, [he] might have revisited [his] acceptance of it” (p. 170). That is a strange and even paradoxical argument, in my view: is there any Greek inscription which might be impossible to “translate” into Aramaic or into any other language? I very much doubt it! C. Tuplin insists that he had to respond as, he writes (p. 172), my argument was received with “a widespread instant approbation”. While not concealing the remaining uncertainties (p. 172: “Briant was right to give the question a thorough airing. The present paper seeks to continue the debate. Where the truth lies we may never know”), he confesses himself “disinclined to think that any [of the arguments] individually condemns the *Letter* or that there are a sufficiently large number of them for it to be fatally flawed” (*ibid.*). This seems to me a rather dubious approach as it avoids considering the arguments in terms of their relative importance. As for the relevant pages in L. Fried (2004: 108–119), they come to a dead end on the section I consider central to my argument, i. e. the question of an Aramaic original and the history of the text. As her prime interest is in the fortunes of Judah, she looks at other inscriptions from Asia Minor (her chapter 4, pp. 108–155) and the example of Egypt (her chapter 3, pp. 49–107) and aims to prove that “the models of local control are not supported by these data” (p. 155): on this, see my critical observations in BH *Ach* II: 178, n. 385; 179–180 and n. 391; 183–4 and nn. 395–396.

- 35 In a recent book, *Letter Writing* (2013): 36 & n. 58, P. Ceccarelli writes: “Not enough is known from its transmission to justify making use of it for a study of the formal evolution of Greek letter writing.” But it is not “Greek letter writing” that is at issue, but “Persian letter writing” – something noted by J. Muir (2009: 84; 215, n. 3), who writes (ignoring the specialist bibliography): “The earliest genuine piece of state correspondence we have from the Greek world is

policy”.³⁶ We must hope that one day a young scholar, who is a good epigraphist and specialises in Asia Minor and the Achaemenids, will reconsider the entire dossier, now accessible on the www.achemenet.com site, in order to put an end to the question and reinterpret the issues.

4 ACHAEMENID EGYPT (PART II)

Egypt, alongside Asia Minor, has for long been of particular importance to me in my researches. The place it (or rather the Delta)³⁷ occupies in the Greek narrative sources, and especially the growing quantity of material emerging from the Nile Valley and its surroundings, its typological and linguistic diversity (beautifully illustrated by the discovery and publication of the Darius statue at Susa) have been potent in stimulating my interest. There is also an older concern I have with the debate on “Egyptian nationalism” begun by specialists working on the Ptolemaic period (Chapter 5 § 1.2.3); although, recently, they have begun to pay more attention to setting the Hellenistic period revolts into a larger perspective (e. g., Vaïsse 2004: XIV, n. 14). Study of the Egyptian revolts, which made it possible for the country to detach itself for two generations from Achaemenid control, forms part of the *longue durée* (Chapter 5), as do my thoughts about the manner in which the Persian period was instrumentalised in Ptolemaic royal inscriptions (Chapter 7). A methodological question articulates this research, which applies not only to the five ‘Egyptian’ articles: namely, how can one use the Greek narratives in order to gain an understanding of what was going on inside an important province of the Achaemenid empire, while giving priority to the local sources? In order to conduct such an enquiry, it is essential to scrutinise them in detail, despite the fact that I have never claimed to be a specialist of each of the languages and scripts used in the Nile Valley (as I have explained several times: cf. below, Chapter 5, n. 2, and Chapter 20, n. 80).

not strictly a Greek letter, for originally it was probably written in Aramaic or Old Persian.” The terminological ambiguity is a striking symptom of the internal contradictions to be found in so many of the arguments: if one thinks that it is an example (even if fake) of Greek epistolography, one admits *de facto* that in no way can it have been produced by an Achaemenid royal scribe; but if one removes it from the Greek epistolographic corpus, one is compelled to demonstrate that there are sufficient indisputable and converging signs pointing to an “Achaemenid hand” (a condition which I do not think has been met).

36 In a recent study (2012: 65), B. Lincoln (who notes my article without entirely accepting my conclusions) thinks that, “even were [the text] a forgery, it would still reflect widespread understanding that the Achaemenid kings and their servants were interested in acquiring exotic trees from far-flung parts of the empire for placement in their pleasure gardens.” Formulated thus, within a discussion “on Achaemenid horticulture and imperialism” (pp. 59–85), it recalls S. Pomeroy’s (ill-considered) opinion (1994) that “regardless to its authenticity, the ‘Letter’ is true to the tradition [of the gardener-king]” (cf. below Chapter 4, n. 61; also § 1 and nn. 8–9 and 133; Chapter 11 on the topic of the ‘gardener-king’).

37 Chapter 5 § 4 (*The Delta revolts*), which now requires modification in the light of Chauveau’s study of 2004 (with note 54 below).

Achaemenid Egypt is marvellous in the way it illustrates multilingualism (Chapter 8), making it possible to analyse this feature empire-wide (*HPE* 507–511; Tavernier, forthcoming), including Persepolis (Tavernier 2008), and first and foremost in the royal inscriptions (Stolper 2005; Jacobs 2012)³⁸. The Aramaic material is crucial in furthering discussions on such matters as the strained relations between Egyptians and Judaeans in Achaemenid Elephantine (Chapter 6),³⁹ and the imperial administrative machinery revealed by the now justly famous Aramaic papyrus containing a customs register (see Part IV, Chapter 18). The recent publication of Aramaic documents from Bactria has brilliantly confirmed E. Benveniste’s intuition that from one end of the empire to the other, from Memphis to Bactra, the satrapal chancelleries used the same language, formulas, and scribes trained in the same manner.⁴⁰ The study now in process of the Persepolis Aramaic texts seems to confirm that the situation was the same in the empire’s centre (cf. Azzoni 2008; Dusinberre 2008; Azzoni & Dusinberre 2014; Azzoni & Stolper 2015).

The articles in Part II were published between 1988 and 2003. This period and the subsequent years have seen a mass of new publications, which reflect a renewal of interest among Egyptologists in the period of Persian rule.⁴¹ Evidence of this can be found in the ‘Egyptian’ articles published in the proceedings of three Achaemenid Colloquia held in Paris between 2003 and 2007, particularly the one in 2007 which was, in large part, specifically devoted to Achaemenid Egypt.⁴² Apart from studies of known documents, also used in the articles in this volume,⁴³ there has been an unprecedented growth of the demotic corpus, particularly in the form of

- 38 J. Finn’s 2011 study I find baffling; it demands a long critical review in a more appropriate place.
- 39 For recent re-examinations of the Aramaic archives at Elephantine and elsewhere: see Joisten-Pruschke 2008, Schütze 2011, Kottsieper 2013, Rohrmoser 2014; on newly published Aramaic documents from Elephantine, see also H. Lozachmeur 2006. Note also S. Bledsoe’s (2015) thoughts on the political messages contained in the Book of Ahiqar, which he analyses in terms of its production at Elephantine.
- 40 See Naveh & Shaked 2012: 39–54, who strangely do not cite Benveniste’s analysis published in *JA* 248 (1954): 43–44 (to which I referred in the Preface to S. Shaked 2004: 7–8); cf. already *L’Asie centrale et les royaumes proche-orientaux*, Paris 1984: 59–60. On this, see most recently the careful study by M. Folmer, forthcoming, as well as (on the Arshama correspondence) L. Allen’s observations (2013); on the “tallies”, see Henkelman-Folmer 2016.
- 41 For the period 1995–2000, see the inventory and analyses in BH*Ach* I (pp. 32–37, 50, 55–56, 58–59, 62, 84–85, 88–90, 91, 98–99), and II (pp. 57–63, 81, 90, 99, 126–127, 132–133, 162, 167, 184–187, 189–194); recently the synthesis by Vittmann 2003, 2011, Klotz 2015a, and Agut & Moreno-Garcia (2016): 528–677.
- 42 See the volumes of the Series *Persika* (de Boccard, Paris) no. 6 (2005): 97–128; no. 9 (2006): 375–405, and particularly, no. 14 (2009): 23–213.
- 43 D. Agut-Labordère and M. Chauveau have recently translated the *Petition of Petetise* into French (2011: 145–195); on the ‘Cambyses decree’, see D. Agut-Labordère’s studies in *Transoephratène* 19 (2005): 9–16 and *RdE* 56 (2005): 45–53. Many demotic documents are re-examined by G. Vittmann and M. Chauveau in P. Briant & M. Chauveau (éds.), 2009: 89–121, 123–131. Pharaoh Khabbabash’s rise to power (*HPE* 1017–1018) has been studied anew through a re-reading of the famous Satrap Stele: see D. Schäfer 2009, and her 2011 book, with the bibliography; see also Colburn 2015 on the Satrap Stele and the recurring Ptolemaic proc-

papyri from the Sacred Animal Necropolis in Saqqara (H. S. Smith & C. J. Martin 2009) and the 460 ostraka from Ayn Manawir.⁴⁴

Further, the Aramaic evidence can sometimes be connected to results from archaeological excavations: thus, discoveries at Syene-Elephantine (von Pilgrim 2003) have helped clarify understanding of the relations between Widranga, the temple of Khnûm and that of Yahweh (Chapter 6)⁴⁵, as several recent studies show.⁴⁶ There is now A. Rohmoser's detailed 2014 analysis, particularly her pages 240–290 (*Die Zerstörung des Jahu-Tempels und sein Wiederaufbau*). She explains in detail how the debate on the circumstances and consequences of the destruction of the Yaho temple on Widranga's order has been conducted; she demonstrates how the recent archaeological discoveries make it possible to understand the nub of the quarrel between Judaeans and Egyptians (pp. 260–265; also 85–103; 161–185); she approaches the subject from the juridical standpoint I suggested (pp. 256–9), and

lamations to the effect that they had returned cult statues looted by the Persians to Egypt (his conclusions accord with my own: cf. here ch.7).

- 44 They have been reported in many articles (cf. BHach II: 62; M. Wuttman in P. Briant (éd.), *Irrigation et drainage dans l'Antiquité* (Persika 2), 2001: 109–236; M. Chauveau, *ibid.*: 137–142; *Id.*, 2011; D. Agut-Labordère & Cl. Newton 2013; Agut-Labordère 2014). The *editio princeps* of the ostraka has been produced by Michel Chauveau and Damien Agut-Labordère within the framework of the achemenet programme and an agreement between the Collège de France and IFAO (Cairo): the texts (with index) in transliteration and (French) translation are now accessible (with photographs) on the achemenet site. – I should add that, quite recently, a new building of the Achaemenid era has been discovered at Syene by the Joint Swiss-Egyptian Mission, together with a substantial number of papyri, ostraka and sealings, now awaiting publication; the annual reports of the Mission can be found at http://www.swissinst.ch/html/forschung_neu.html.
- 45 Some of the conclusions reached in that article were already suggested in a preliminary fashion in another one published in 1987 (here Chapter 1 § 1.3), and developed in detail in Chapter 5 § 3. The main new insights in my 1996 article (Chapter 6) are on the legal and juridical plane (§ 3).
- 46 C. von Pilgrim 2003 (pp. 314–317) broadly follows my interpretation, while adding some corrections (rightly so, as A. Kuhrt 2007: 130, n. 52, who also accepts my view, notes, cf. her *Persian Empire* II: 829–831; 855–859). Von Pilgrim's work was used by R. G. Kratz 2006, who seems, however, to be unaware of the existence of my article (cf. his pp. 248–250); the same applies to I. Kottsieper 2002, H. Nutkowitz 2011 (who thinks the destructions were carried out without Widranga's knowledge), or A. Joisten-Pruschke (2008: 67–75), who is unaware of the recent studies of Greek inscriptions (p. 75–81, discussing P. Frei's theory of the *Reichsautorisation*); as for her somewhat daring attempt at a comparison with the *lan*-sacrifice of the Persepolis tablets (p. 70 and 2010: 44–45), it must now be reevaluated in the light of W. Henkelman's analyses and interpretations (2008a: 181–304); see also the well informed pages of Schütze 2011, pp. 228–241. L. Fried (2004: 102–106), unaware of von Pilgrim's recent publications, takes my article into account, without however grasping its implications. C. Tuplin (2013: 136–151) presents the most recent re-examination of the whole dossier, discussing both my interpretation (see the article translated here = ch.6) and von Pilgrim's archaeological one. Despite his doubts, I cannot find any arguments in his commentary (especially, pp. 142–143) that would demolish my interpretation, which (I stress) analyses the situation from the juridical perspective – while, of course, not excluding cultic and religious elements: see n. 36 of my article. See now Kottsieper, forthcoming.

her conclusions agree and confirm mine, both with respect to Widranga's actions, as well as the legal and political circumstances that led to his decision (pp. 278–290).

As for the topics discussed in my articles (Part II), that of Persian-Egyptian relations has now been much expanded by J. Yoyotte's detailed re-study of the Darius statue (2013). He there disagrees (n. 36) with my minimalist interpretation of the careers of several Egyptian nobles at the time of Cambyses and Darius (below Chapter 5 § 7.2–4, and *HEP* 497 = *HPE* 481–2). He counters with the example of the *sentī* (*HEP* 425 = *HPE* 413), which proves that Egyptians held high administrative positions. Nevertheless, it is perfectly possible that the *sentī* in Achaemenid times did not enjoy the powers he had in Ptolemaic times (Agut-Labordère, forthcoming). Moreover, it is the case that the surviving documentation indicates that Persians and Iranians were in the majority among those occupying high-level positions (Chapter 5 § 7.2).⁴⁷ There is also the fact that, since my first articles on Egypt, the Saqqara Stele, published in 1995 (*BHach* I: 34–35, 98–99),⁴⁸ shows that the close links between Persians/Iranians and Egyptians (Chapter 5 § 8) could lead to mixed marriages: cf. below Chapter 8 § 2.2 (“This is the first definite evidence of such a union” – to be contrasted with Chapter 5 § 8.2). This stele also played an indirect role in an article by O. Muscarella (2003). He firmly expunged from the dossier of Egypto-Persian cultural relations an intriguing archaeological and iconographic monument (the Von Bissing Stele)⁴⁹ “[which] was probably made in Egypt by a craftsman who lived in the early 20th century.” His logical conclusion runs: “For discussions on Persian-Egyptian relations and shared customs in the fifth century B. C., the remarkable funerary stela excavated at Saqqara, not the von Bissing relief, is a source” (p. 120).

The question of Egyptian revolts has received frequent attention in recent years, as one aspect in particular of the broader problem of relations (real and imagined) established between Egyptians and foreigners – one not limited to the Persian period, but one that became more urgent when Egypt and Egyptians were dominated by a foreign power, albeit ‘pharaonised’.⁵⁰ There are several studies of the subject, but none contributing, as far as I can see, much that is new. Basically, they are limited to considering the subject exclusively from the angle of Athens' interests and those of the Attic Delian League, using Greek narrative sources, whether they be

47 On the ethnic origin of high satrapal officials, see also the very full information provided by G. Vittmann 2009: 101–102 (he translates the *sentī*'s title as ‘the planner’), and M. Chauveau 2009 (pp. 127–8 on the *sentī*), together with C. Tuplin's comments, *ibid.*: 420–421, who suggests “a sort of titular *apartheid*”.

48 On this monument, see most recently E. Rehm's analysis (2005: 500–503) and that of M. Was-smuth (2010), as well as G. Vittmann's remarks (2009: 104–5, together with his drawing).

49 See the photograph in my *Darius, les Perses et l'empire* (Gallimard-Découvertes, n°159), Paris (2^d ed. 2001): 90–91.

50 See particularly G. Vittmann 2003. Relations of Egyptians with Persians are treated in the very interesting chapter V, to which should be added the next two chapters dealing, one with “Car-ians in Egypt” (chapter VI), the other with “Greeks in Egypt in the pre-Hellenistic period” (chapter VII), as well as the preceding one (III) on “Aramaic documents”.

studies concerning a single revolt,⁵¹ or attempts at a synthesis.⁵² With the exception of an interesting study by O. E. Kaper (2015) on the revolt of Pedubastis IV (one of the ‘liar kings’?), no one has looked at the material in terms of Achaemenid imperial interests, by gathering the evidence which includes the Persian, Elamite, Babylonian and local sources in all their diversity.⁵³ Note, for instance, that the only new piece of evidence on the Inaros revolt is a demotic contract from Ayn Manawir studied by M. Chauveau (2004). The text proves that the rebel was recognized in southern Egypt including the western desert (contrary to what I wrote in chapter 5 § 4).⁵⁴

5 THE GREAT KING, LAND AND WATER IN THE ACHAEMENID EMPIRE (PART III)

The series of three thematic groupings (Parts III–IV–V) opens with six studies concerning aspects of the material foundations on which Achaemenid imperial domination rested, in particular land and water. By this, I am not referring to ‘earth and water’ in the (still unclear) meaning of Herodotus’ famous phrase,⁵⁵ but rather the sphere of agriculture and animal husbandry and the conditions which enabled or obstructed their performance determined by the access countries and peoples enjoyed to water and irrigation. All will be aware that this was a fundamental aspect of social life in the Middle East, in Egypt and Central Asia, hardly peculiar to the Achaemenid period. The subject has interested me for a very long time, the reason certainly being my involvement in the lively discussions that took place in the 1970s about what has commonly been called the Asiatic Mode of Production (AMP). And it is a reference to Karl Marx that opens the introduction I provided in 2002 to a collection put together on the theme ‘Politics and the control of water in

- 51 On the Inaros revolt, see the discussion of D. Kahn 2008, whose main purpose is determining the chronology.
- 52 See M. Rottpeter 2009, whose analysis of the various *apostaseis* contains nothing new. The relevant pages of Schütze 2011: 60–68 are rather more interesting.
- 53 See in particular the book by S. Ruzicka 2012 who aims to produce a purely narrative history, organised in accordance with the classical sources and thus seriously underestimates the value of the Achaemenid-Egyptian sources for treating the question: how is possible (for example) to reduce the Aramaic documentation to “some scrappy Aramaic letters” (p. xxi)?
- 54 However, Chauveau’s proposed reading “Prince of Rebels” has to be abandoned in view of J. K. Winnicki’s criticisms in *Ancient Society* 36 (2006): 135–142: it should be read as “Chief of the Balaku tribe”. According to Kaper (2015: 125–6; 144–5), the development of the oases in Darius’ time is due to the Great King’s plan “to make sure that a revolt could never come from the oases again;” but it is worth remembering here that it is a very delicate matter to deduce political events from the demotic documents of Ayn Manawir: proof of direct action by the imperial administration in the creation of a (modest) system of underwater irrigation canals in the oasis is absent: cf. Agut & Moreno-Garcia (2016):638: “It is possible that [this system] is in fact the result of an experiment by the local Egyptian people”.
- 55 See the discussion by A. Kuhrt in *AchHist* III (1988): 87–99, and most recently the ideas of Waters 2014; note also Klinkott 2016.