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978-0-521-03653-5 - Three Historians of Alexander the Great: The So-Called
Vulgate Authors, Diodorus, Justin and Curtius

N. G. L. Hammond

Excerpt

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INTRODUCTION

The need for source analysis

The basic task for the historian of Alexander (henceforth A) is to assess the value of each of the five main accounts which have survived. It is rendered formidable not only by the length and the complexity but also by the unevenness of Arrian, *Alexandri Anabasis* (henceforth Arrian), Diodorus (henceforth D.), Justin (henceforth J.), Curtius (henceforth C.) and Plutarch, *Life of Alexander* (henceforth PA). There is only one way to discharge this task fully, and that is to analyse each individual account and determine, as far as it is possible,¹ which earlier source it was using at any given point. In this book I attempt to do so only for D., J. and C.

The reason for selecting these three is that they have often been grouped together as 'The Vulgate', and have been set apart from the other two. In itself such a grouping is innocuous; and its own inner rationale is that at some points D., J. and C. do have some features in common. But what has been damaging to the evaluation of each has been the exploitation of this grouping. Thus they have been given the label 'The Vulgate Tradition'. The implication that a single tradition is conveyed in these three works is fallacious; for they differ frequently one from the other, and often in major matters. Worse still is the label attached by E. Schwartz and F. Jacoby, 'The Cleitarchan Vulgate'; for this indicates not only that all three derive from one source but also that that one source was Cleitarchus.²

The first attempt to break the spell of 'The Vulgate' was made by W. W. Tarn. Although many of his arguments were unconvincing, he seemed to me to demonstrate that 'The Vulgate Tradition' and 'The Cleitarchan Vulgate' are both myths of a simplistic kind. Two of his conclusions may be quoted. 'How two such totally different historians as D. and

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C., with such different points of view and such different main portraits of A, ever got bracketed together is very hard to understand.' 'There never was any such thing as an Alexander-vulgate or Cleitarchan vulgate, exhibited by D., C. and J.'³ But it then became incumbent on Tarn to provide his own analysis and identify the sources of each work. Here he failed lamentably. His analysis was too superficial (he analysed only one chapter of D.), and the sources he identified included two — 'The Mercenaries Source' and 'The Peripatetic Tradition' — which were not mentioned in ancient literature and have not been accepted in modern literature either.⁴

A younger generation of writers, reacting against the work of Tarn, have thrown out the good with the bad. What is worse, they have not only resumed 'The Vulgate Tradition' but have also introduced 'The Vulgate Sources', by which is meant not, as one would expect, 'the sources of the vulgate tradition' but the 'vulgate' accounts themselves. And worst of all, some writers have lost the origin of the term 'vulgate', and they have included among 'the vulgate sources' PA, the *Metz Epitome* and almost anything which is not Arrian.⁵ The final step is to match the entire array against Arrian and see how he stands up to the assault, if at all. And as the name of Cleitarchus still clings to 'The Vulgate Tradition', Cleitarchus gets the benefit of the good things which do occur in D., C., J., PA and the minor works concerning A. He becomes quite a gifted writer! Recently advances have been made in the studies of two of our authors. In the Budé edition of D. 17 P. Goukowsky has summarised views which he had already published in specialist articles. In particular he refuted the idea that D. excerpted throughout from Cleitarchus. 'Ainsi,' he wrote, 'de nombreux détails inconnus des autres historiens de la "Vulgate" (ou même d'Arrien) peuvent être le fruit des lectures personnelles de Diodore et provenir soit des sources historiques utilisées dans d'autres sections de la Bibliothèque soit de traités techniques.' In his Commentary on C. 3–4 J. E. Atkinson has concluded from his study of these two books (out of nominally ten but really eight) that 'Curtius used several sources' and that one of these sources 'may have

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been Cleitarchus'. If either one or both of them are right, as I believe they are, then the concept of 'The Vulgate' as enunciated by Schwartz and Jacoby is mistaken – and even more so the later elaborations of it. In consequence, the term 'vulgate' is not used in this book. What I attempt is an analysis of the sources of each writer separately – D., J. and C.

The need for a detailed analysis of these three Alexander-accounts has been stated often in recent years. For instance, E. N. Borza writing of Cleitarchus and Diodorus 17 remarked that 'more is needed of the order of Hammond's study of the sixteenth book [of Diodorus]', and P. Goukowsky in considering the same topic asked for 'une analyse détaillée du livre 17'.⁶ And E. Badian in *Entretiens Hardt* 22 (1975) 301 noted that C. and D. were still practically untreated, and that comments on them tended to be either limited or arbitrary; and he expressed the need for a thorough investigation of each. That is, *inter alia*, the purpose of this book.

D., J. and C. are inevitably contrasted with Arrian, from whom they differ radically. 'The generally high quality of Arrian's history', wrote P. A. Stadter in his admirable study, 'is due in no small degree to a discriminating use of sources.'⁷ What is more, Arrian alone of Alexander-historians stated who his sources were and why he had chosen them: namely Ptolemy and Aristobulus, because they had campaigned with A and were the most trustworthy of all the authors whose works he read. For these reasons, as well as for its 'generally high quality', Arrian's work has generally been regarded as far superior in the main to the works of the other writers on A. Indeed it has often been used as a touchstone for testing the quality individually of D., J. and C. However, the trend of recent scholarship, especially in the writings of E. Badian and A. B. Bosworth, has been to try to reverse this verdict in regard to many issues, such as A's complicity in the murder of Philip, the course of the battle of the Granicus river, the cause of the fire at Persepolis and the cause of A's death. The weakness of this trend is that its advocates have not tackled the problem of identifying the sources used by D., J. and C. Until they have done so and have shown that those sources

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are more trustworthy than the known sources of Arrian, there is a danger that their judgements are unduly subjective.

Alexander's last days as an example

It will be illuminating for the reader to take now as an example the accounts of Arrian on the one hand and of D., J. and C. on the other hand which were concerned with the last days of A. For the reader will see at the outset that the differences are sharp not only between the ancient authors but also between modern scholars.

When Arrian's narrative approached the illness of A, he abbreviated and paraphrased a passage in the *King's Journal*, which he called simply *αι βασιλαιοι εφημεριδες* (7.25.1 – 26.3). Plutarch had already done likewise in writing his *Life of Alexander* (76-77.1).⁸ The similarity of the paraphrases confirms what we have no reason to doubt, that each was telling the truth and each was drawing independently on the same passage in the same *King's Journal*. At the end of his paraphrase Arrian commented as follows. 'The accounts of Ptolemy and Aristobulus', i.e. in their own works which Arrian preferred to those of other writers, 'were not far from this [account];⁹ but others recorded remarks by A in conversation with his Companions', i.e. about the succession; and yet others attributed A's death not to a natural illness but to poisoning at the instigation of Antipater.

At the end of his paraphrase Plutarch discussed the allegation of poisoning, which most writers, he said, regarded as a fabrication. Plutarch helped to confirm Arrian's point about Aristobulus' account being 'not far from' that in the *King's Journal*; for at 75.6 he cited Aristobulus as saying that having a raging fever and an excessive thirst A drank wine, 'after which he became delirious and died on the thirtieth day of the month Daesius'. It was this excessive fever and delirium which prevented him from being able to speak. However, D., J. and C. all provided remarks allegedly uttered by A in conversation with his Companions (D. 17.117.3–4; J. 12.15; C. 10.5.1–6), and in particular his

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dying words (D. 17.117.4; C. 10.5.6 'suprema haec vox fuit regis, et paulo post exstinguitur'). J. and C. present the story of the poisoning as a matter of general belief or as a fact (J. 12.14; C. 10.10.14–17), while D. is more cautious (17.117.5 – 118.2).

Thus the accounts of the *King's Journal*, Aristobulus, Ptolemy, Plutarch and Arrian are totally incompatible with those of D., J. and C. In the former group Alexander was represented as speaking only of military and naval affairs and then being speechless for four days before he died; and he neither said nor did anything about the succession. The symptom too of a continuous high fever is incompatible with death by poisoning. According to the latter group Alexander retained his power of speech till the last and made astute remarks about the succession, and in particular he gave his signet-ring to Perdikkas; and the tradition that A was poisoned is treated as being worthy of serious consideration.

Which group is to be regarded as correct? Pearson, Samuel, Hamilton, Bosworth, Brunt and Lane Fox favour the latter group. They all maintain that the *King's Journal* was a forgery made in antiquity.¹⁰ In addition, Badian and Bosworth, for instance, accept the statements of this group that A did give his royal signet-ring to Perdikkas, and Badian thinks that Perdikkas retained it until his own death. Bosworth accepts the poisoning as historical.¹¹ I am perhaps alone in regarding the first group as superior and in judging the *King's Journal* to be genuine. Let us look at the arguments. And let us bear in mind that until Pearson wrote his article in 1954 'it has been almost universally held that Arrian derived his version from Ptolemy, who had access to the original Diary [*King's Journal*]'.¹² It seems to me that Pearson's case for denying the genuineness of the *Journal* has been uncritically accepted by subsequent writers.

The King's Journal

That a *King's Journal* was kept for Philip and then for A, and that *Alexander's Journal* did record the last words and the

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last days of A is beyond question.¹³ It was from the latter that Plutarch and Arrian believed themselves to be making their paraphrases. The fact that they had *Alexander's Journal* in front of them and were practised in assessing the genuineness or otherwise of such documents might be enough in itself to convince us that they were right.¹⁴ Then we have the further fact that the accounts given by Aristobulus and Ptolemy (though lost to us) were judged by Arrian to be 'not far from' the account in *Alexander's Journal*. Could these two men, being contemporaries of A and close to him, have been taken in by a forgery which misrepresented the last words and the last days of A? That is to me inconceivable. On the other hand, if Aristobulus wrote from memory and if Ptolemy either used the genuine *Journal* or wrote from memory only, is it possible that the memory of each was faulty precisely in those reports which coincided by chance with the misrepresentations of the forged *Alexander's Journal* from which Plutarch and Arrian made their paraphrases? That too is really beyond belief.

It is, of course, an exciting idea that a modern scholar, operating only with paraphrased fragments, may be able to prove an ancient work to have been a forgery so ingeniously constructed that it deceived four ancient scholars (Plutarch, Arrian, Aelian, *VH* 3.23 and Athenaeus 10.434b, cited together in *FGrH* 117). But excitement is not enough; we need very strong arguments to support the idea. Hamilton listed those of Pearson's arguments which he found 'decisive', and it will be enough in this context to consider them. The first argument arises from the *Journal's* mention of a shrine of Sarapis, the Egyptian god of healing, whom some Companions of A consulted during the night before A died. Pearson argued that the presence of such a shrine in Babylon was an anachronism, and that in consequence the *Journal* was composed only when such a shrine existed – c. 280 in his belief – and so was a forgery. But we have to remember that Egyptians, like Jews, may have visited and resided in Babylon in the Persian period; that A was deeply impressed by the Egyptian priests of Zeus Ammon, practised Egyptian forms

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of worship in Egypt, and had Egyptian seers in his entourage from 331 onwards (C. 4.10.4); and that he intended his body to be embalmed by Egyptian priests when he lay dying at Babylon. Thus it is probable rather than improbable that a shrine of Sarapis should have existed in Babylon in 323.¹⁵ On the other hand, if a shrine of Sarapis was such a glaring anachronism as Pearson and others have supposed, would a forger have made the stupid error of introducing it into his account? Would not Peithon, Attalus, Demophon, Peucestas, Cleomenes, Menidas and/or Seleucus¹⁶ have protested that there was no such deity in Babylon and they never went near such a shrine?

The second 'decisive' argument cited by Hamilton is that Arrian seems to mention the *King's Journal* as 'a familiar literary work'. Arrian passes no such comment. It is a matter of modern taste whether one considers such sentences as 'next day he bathed again, sacrificed the appointed sacrifices and after sacrificing continued in constant fever' to be the mark of a literary work rather than a factual diary of events and sayings.¹⁷ Rareness of citation from it is another point; but this applies equally to a genuine *Journal* and to a forged *Journal* supposed to be genuine. We have to remember the difference between the research scholar and the ancient writer. The latter did not go back in the primary material, if he could avoid it by using a literary account, and particularly so in the case of a *Journal* which covered in diary form the thirteen years of A's exceedingly active reign and was packed with original documents. Next, it is argued that 'the other references to the *Journal* do not suggest an official document'. Here again we must keep a historical perspective. Modern scholars may not be interested in A's hunting, sleeping, drinking, illness and health, but there is no doubt that in the fourth century B.C. the royal hunts were of the greatest importance (cf. the fresco at Vergina and C. 8.1.14–16), that the royal banqueting and the heavy drinking at banquets with the consequent daytime sleeping were normal at court (cf. the palace of Vergina¹⁸ and the affair of Cleitus), and that the last days of such a king were important to his followers.

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Lastly, 'it is surprising, if Ptolemy used the *Journal*, that he failed to mention his use of so reliable a source'. This too applies to a forged *Journal* as much as to a genuine one; and again it rests on the tacit assumption that an ancient writer behaved like a modern writer who is continually citing and evaluating his sources. In any case we possess very little indeed of Ptolemy's own writings.

More general aspects of such a forgery may be considered. It had to be very different from the real *King's Journal* in order to make the undertaking worthwhile; and the undertaking was enormous, to construct bogus diaries for thirteen years of intense and documented activities – a work perhaps twice the length of Winston Churchill's *Second World War*. To fake spurious but plausible reports, letters, orders and documents was by itself a daunting task. Bosworth tried to evade this difficulty by postulating a truncated Diary, a mere fragment covering 'the last few months'. But this only creates difficulty. 'What is this fragment?' people would say when it was published, 'Where is the rest of the *Journal* if this fragment is from it?'¹⁹

When was the fake *Journal* produced? Pearson opted for c. 280 when men's memories might have become dim. But who was interested then? Surely the real *King's Journal* was already known and even written about by 280? Bosworth went to the other extreme. He had the forger write and publish between June 323 and winter 322–321. As this early date was incompatible with Pearson's chief argument, namely that the worship of Sarapis did not arise at Babylon until much later, Bosworth argued – as Bickerman had already done and I have done here – that such a worship was practised at the time of A's death in Babylon. Forger though he was, Bosworth's faker got that right. But could a forger have imposed his version on the minds of the Macedonians in the very year after A's death? Hundreds of leading Macedonians knew the true facts, their memories were green and they would surely have rejected a false version. At that time the corpse of A and his possessions were still in Babylon, and the real

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Journal was there. It could have been used at once to expose the forgery.²⁰

Who was the forger? Bosworth proposed Eumenes. Now Eumenes and his team of secretaries had already composed the entire *Journal* since A's accession. Did Eumenes in the twelvemonth after A's death sit down and compose a false version of A's last few months? Any secretary or anyone having access to the *Journal* could have exposed him at once as a falsifier. Pearson was more subtle. He made use of a little-known writer called Strattis of Olynthus (*FGrH* 118), who wrote three works according to the *Suda*. One of these works, as cited by Pearson, was 'five books about *The Journal of Alexander*', *περὶ τῶν Ἀλεξάνδρου ἐφημερίδων βιβλία πέντε*.²¹ This title is found also in Athenaeus 10.434b, where we are told that Eumenes of Cardia and Diodotus of Erythrae wrote up *The Journal of Alexander*, *ἐν ταῖς ἐφημερίσῳ αὐτοῦ* (i.e. Alexander). At first sight one would make two deductions: that *The Journal of Alexander* was very lengthy and that Strattis being an Olynthian wrote his commentary before c. 300, since he took his citizenship as an adult from a city which Philip had destroyed in 348. Not so Pearson. He supposes that the forger used 'a faked author's name for a fictitious diary' which he was publishing c. 280; so he chose to call himself 'Strattis of Olynthus'. This double supposition is a strain on anyone's belief. Further, why did the forger choose to call himself an Olynthian? Would not people have wondered at the time: 'Who is this Strattis, no youngster but a man of 86+, as he became a citizen at Olynthus?' It is more in accordance with the dictates of reason to believe that there was a real Strattis and that he did write five books of commentaries on the real *Journal of Alexander*, than to suppose with Pearson that an unknown man wrote five books about a (?his own) faked *Journal of Alexander* and assumed a faked name and an unlikely citizenship c. 280.²²

Let us limit ourselves to a real *Journal of Alexander*. The corpse and the possessions of the deceased king were to be taken to Aegeae, but they were intercepted and removed by Ptolemy to Memphis and then to Alexandria (Paus. 1.6.3).

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Among them surely was the *Journal*. Ptolemy himself used it in writing his history and he may have given favoured persons access to it; perhaps Strattis of Olynthus was one. The original or a copy of it went, one imagines, into the Library of Alexandria; and copies were available later for Plutarch, Arrian, Aelian and Athenaeus to consult. On the other hand, early writers such as Cleitarchus did not have access to the *Journal*.

If the conclusion is correct that what Arrian and Plutarch were paraphrasing was a passage in a genuine *King's Journal*, then the reader will see that Arrian and Plutarch in this instance are to be regarded as dependable and D., J. and C. as not. For the source of Arrian and Plutarch was a factual record of the day-by-day happenings in which the King was involved, made at the time and not intended for publication. He will see also that, since Ptolemy's account and Aristobulus' account were 'not far from' the version of the *King's Journal*, Ptolemy and Aristobulus were dependable authors in this instance, and that Arrian's choice of them as participants at the time and as trustworthy writers was justified. On the other hand, D., J. and C. provided items of fiction; not because they invented the items, but because they obtained them from one or more sources who were writing fiction or propaganda. The points which D., J. and C. have in common evidently came from a common source of this kind, one who wrote probably before the detailed accounts of Aristobulus and Ptolemy were published and so had a free field for invention. This source must have been a writer who was popular in the time of D., J. and C. As we shall see later, he was probably Cleitarchus, who was regarded by ancient authors as having a lively style but little or no regard for the truth. Thus the reader will have no hesitation in discarding as unhistorical the giving of the signet-ring to Perdikkas, the sayings of A about the succession and the alleged poisoning of A; and in rejecting the views of Badian and Bosworth, for instance, which were based on their belief that D., J. and C. were in this instance more dependable than Arrian.