

On the Date of Composition of the *PERIPLUS MARIS ERYTHRAEI*

A Study of the South Arabian Epigraphic Evidence

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1. Introductory

The *Periplus Maris Erythraei*¹ is said to have been written by a Greek merchant resident in Egypt in the time of the Roman Empire. It is a kind of commercial handbook giving details of the products of the various places along the coast, and the trading conditions in each port, on the shores of the Red Sea, Persian Gulf and Indian Ocean, then known collectively as the Erythraean Sea. As such, it is not only of very great value for the study of the history of East-West intercourse, but is also an indispensable source of knowledge of the conditions in that area at that time, a field by no means abundant in source material.

While there is no room for doubt that this book was written during the period of the Roman Empire, many different views are held by scholars, when it comes to more precisely fixing its date². Even so, since about 1870, there had, until recently, appeared no one who maintained that the date of composition lay in the 2nd or 3rd centuries. Though there were differences on points of detail, it would seem natural that considerations of the history of trade in the Indian Ocean and the handbook-like character of this book³ led to a general tendency to settle for a date about half-way through the 1st century or in its latter half⁴. However, in the last ten years and more, theories have once more been advanced, from various viewpoints, for dates in the 2nd or 3rd centuries⁵. In this article I have gone over the ground covered by recent researches, and, using South Arabian epigraphic material as my principal sources, attempted a reassessment of the date of composition of this handbook.

(Abbreviations) *ADSA*=R. LeBaron Bowen et al., *Archaeological Discoveries in South Arabia*, Baltimore, 1958; *AION*=*Annali dell'Istituto orientale di Napoli*; *AltGH*=F. Altheim, *Geschichte der Hunnen* V, Berlin, 1962; *BASOR*=*Bulletin of the American Schools of Oriental Research*; *BeSISh*=W.L. Brown & A.F.L. Beeston, *Sculptures and Inscriptions from*

Shabwa, *JRAS*, 1954, pp. 43–62; *BO*=*Bibliotheca Orientalis*; *CIH*=*Corpus Inscriptionum Semiticarum. Pars Quarta: Inscriptiones himyariticas et sabaeas continens*, 3 vols., Paris, 1889–1930; *CQ*=*The Classical Quarterly*; *DiUD*=A. Dihle, *Umstrittene Daten*, Köln, 1865; *DoSA*=B. Doe, *Southern Arabia*, London, 1971; *JA*=*Journal Asiatique*; *JAOS*=*Journal of the American Oriental Society*; *JaRSAIH*=A. Jamme, Remarks on the South-Arabian Inscriptions Hamilton 3–13, *JRAS*, 1956, pp. 146–156; *JaSIMB*=Id., *Sabaeen Inscriptions from Maḥram Bilqīs (Mārib)*, Baltimore, 1962; *JaUT*=Id., *Al-'Uqlah Texts*, Washington, 1963; *JRAS*=*Journal of the Royal Asiatic Society*; *KP*=*Der Kleine Pauly* I–IV, München; *LounRycND*=A.G. Loundine & J. Ryckmans, Nouvelles données sur la chronologie des rois de Saba' et dū-Raydān, *Le Muséon*, 1964, pp. 407–427; *PiCISA*=J. Pirenne, De la chronologie des inscriptions sud-arabes après la fouille du temple de Mārib (1951–1952), *BO*, 1969, pp. 303–311; *PiDPME*=Id., Un problème-clef pour la chronologie de l'Orient: La date du périple de la Mer Erythrée, *JA*, 1961, pp. 441–459; *PiRSAQD*=Id., *Le royaume sud-arabe de Qatabān et sa datation*, Louvain, 1961; *RE*=*Pauly-Wissowa-Kroll-Witte, Realenzyklopädie der klassischen Altertumswissenschaft*, Stuttgart; *RES*=*Répertoire d'épigraphie sémitique* V–VIII, Paris, 1928–1968; *RycCRSR*=J. Ryckmans, *La chronologie des rois de Saba' et dū-Raydān*, Istanbul, 1964; *RycEESAR*=Id., Etudes d'épigraphie sud-arabe en russe, 2, *BO*, 1968, pp. 153–156; *RycIMAM*=Id., *L'institution monarchique en Arabie Méridionale avant l'Islam*, Louvain, 1951; *RycPCH*=Id., *La persécution des chrétiens himyarites au sixième siècle*, Istanbul, 1956; *RycPRSA*=Id., Petits royaumes sud-arabes d'après les auteurs classiques, *Le Muséon*, 1957, pp. 75–96; *RycQDSA*=Id., De quelques dynasties sud-arabes, *Le Muséon*, 1967, pp. 269–300; *RycRHU*=Id., Les rois de Hadramawt mentionnés à 'Uqla, *BO*, 1964, pp. 277–282; *SchPES*=W.H. Schoff, *The Periplus of the Erythraean Sea*, London, 1912; *WisAAG*=H. von Wissmann, *Zur Archäologie und Antiken Geographie von Südarabien*, Istanbul, 1968; *WisGL*=Id., *Zur Geschichte und Landeskunde von Alt-Südarabien*, Wien, 1964; *WisHAH*=Id., Ḥimyar, Ancient History, *Le Muséon*, 1964, pp. 429–499.

2. Outline of recent arguments in favour of the 2nd or 3rd centuries

Before entering on my own argument, I would like to introduce an outline of the theories that have been put forward in recent years in favour of the 2nd or 3rd centuries, and to make clear my own view in regard to them.

We will first take the theory in favour of the first half of the 2nd

century. This theory is maintained by some specialists in the ancient history of India¹ and reposes on two points. The first is that *Μάγβαρος*² in ch. 41 of the *Periplus* is a phonetic rendering of Nahapāna, the Great Satrap of the Kshaharāta family; the second is that this Nahapāna is regarded as having occupied his position from the end of the 1st century and during the first half of the 2nd. With regard to the first point, as even J. Pirenne says³, in the present state of things there is probably no alternative to the interpretation given above. As regards the second point and the opinion on the date of Nahapāna's reign, it is one that I find it difficult to accept. In order to determine the date of his reign, it is first necessary to elucidate a number of preliminary questions. Are the dates 41-46, which appear on his inscriptions, in fact referred to the Saka era or are they merely his regnal years? Then from what historical event did the so-called Saka era originate, and to what year in the Christian era does its first year correspond? Yet, in the present state of things, views on all these points too are in confusion, and the situation is therefore such that one is far from being able to discern any unity of view on the question of the date of Nahapāna's reign either⁴. Thus the date of Nahapāna's reign is in itself still uncertain, so it seems hardly worth wasting any more talk on the fact that it is an inadequate basis for arguing about the date of the composition of the *Periplus*. My rejection of the theory of the first half of the 2nd century based on ch. 41 of the work is based on the above reasoning.

As opposed to the specialists in Indian history who uphold the theory of first half of the 2nd century, the theory for the first half of the 3rd century is maintained by specialists in ancient Arabian history. Among the studies concerned here there are those of J. Ryckmans⁵ and H. von Wissmann⁶. They find ancient South Arabian inscriptions with the names of two kings, Charibaël and Eleazos who appear respectively in chs. 23 and 27 of the *Periplus*, and they then seek to determine the date of this work on the basis of a time when kings of these names were ruling simultaneously. These studies cannot be said to be new in method⁷, but seen in the light of this article, they are of profound interest, being based on the advances in, especially, chronological studies, which have recently resulted from new achievements in the field of ancient South Arabian studies. This brief introduction will suffice at this point. The matter will be scrutinised again in chapter 3 and subsequently, where the main argument of this article is expounded.

In contrast to the theory of Ryckmans and Wissmann, in which their argument is restricted in one direction to yield the result, Pirenne⁸, though also a specialist in ancient South Arabian studies, seeks her arguments widely in many directions. First of all she criticises two points which have come to seem powerful ones among the arguments for the 1st century

theory, and she emphasizes that this theory, which has up to now had the look of being half-way to a received one, is not in fact so solidly based after all. Her first criticism is brought against the view that would claim that there are signs that Pliny's *Natural History* and the *Periplus* were composed at about the same time and that, further, the former made use of the latter, these conclusions emerging from comparative examination of various items relating to South Arabia in the two works⁹.

(1) The *Natural History* lacks the reference to the united kingdoms of Saba' and Ḥimyar (royaume de Saba' et Dou-Raydān), found in ch. 23 of the *Periplus*.

(2) There is no mention in the *Periplus* of the Qatabān kingdom which appears in the *Natural History*¹⁰.

(3) The treatment of the important South Arabian ports of Mūza, Aden and Moscha is different in the two works.

(4) There are great differences between the two works in their accounts of the frankincense trade.

She cites these four points to refute the hitherto prevailing theory of the relationship between the two works, and maintains that the composition of the *Periplus* was somewhat later than that of the *Natural History*¹¹.

However,

(1) About the king living in Sap(p)har, Pliny¹² only says, 'the king of this district', without any concrete indication of the country to which he is referring. Therefore, one cannot indeed affirm with certainty that this king like Charibaēl in the *Periplus* was the king of the two countries of Saba' and Ḥimyar, but neither can one assert that he was not king of both but only of Ḥimyar. However it may be, the object of the *Natural History* in this portion was to give an account of the development of the route to India, and it is natural rather than otherwise that there is scarcely any mention of the political situation inland; so that one may say that it is inherently unreasonable to claim that this passage was really an attempt to describe the contemporary relations between Saba' and Ḥimyar. Furthermore, the materials used for other passages in the *Natural History*¹³, touching on South Arabia, are old, and one cannot therefore suppose that they give an accurate account of the situation at the time the author was writing, and so cannot constitute a factor in the argument at this point. On this interpretation, one cannot assert definitely that it was not true that the two countries were at this time under the rule of one king, on the ground that there is no account of the united kingdoms of Saba' and Ḥimyar in the *Natural History*. In chapter 3 below it is made clear that the fact is rather the reverse: it can even be said that there is an extremely strong possibility that the occupation of Sap(p)har and rule over Ḥimyar were in the hands of the king of Saba' in Pliny's time. It is thus hard to accept Pirenne's first point as a ground for arguing against the simultaneous com-

position of the two works¹⁴.

(2) As to comparison of the two books on the subject of Qatabān, one may well borrow words used by Pirenne elsewhere, "comparaison qui ne peut être valable parce qu'elle ne tient pas compte du genre tout différent des deux auteurs".¹⁵ That is to say, "le Périples ne s'intéresse qu'aux rivages et aux ports de commerce"¹⁶: this being so, then it is rather natural that there is no mention of Qatabān, which was inland and no longer playing a role of any importance in Indian Ocean trade, and this lack of mention cannot be taken to indicate that the composition of the *Periplus* was subsequent to the fall of this kingdom, as Pirenne maintains.

(3) The same sort of thing may be said about the difference in the treatment of the ports. Whereas it was the object of the *Periplus* to indicate business conditions in the ports, that of Pliny was to give an account of the history of the development of the route to India. The reason that the latter glanced lightly, if at all, at the ports of Mūza and Moscha, looked on as important by the former, was that, although they were both important as trading posts, they did not play a big part as ports of call or watering stations for ships sailing across the Indian Ocean. Further, the reason for Aden dropping out of Pliny's account, should probably be attributed to its having been at the time no more than a poor village, as suggested in ch. 26 of the *Periplus*. In any case, it cannot be held that a difference in the date of composition of the two emerges from the difference in their treatment of these three ports.

(4) In Pliny's time the route to India was in its final stage of development, and when one considers the very large numbers of Greek merchants who put in at the Ḥaḍramaut port of Kanē, it is absolutely impossible to believe that the frankincense produced by Ḥaḍramaut was still, even in Pliny's time, being carried exclusively by the land route to the north.¹⁷ It seems reasonable to consider, in the light of the account given in chs. 27-32 of the *Periplus*, that a greater part, even if not all of it, was exported by sea. I, myself, along with other students¹⁸ of the question, am of the opinion that, in his account of the frankincense trade, Pliny does not present an accurate picture of the situation at that time, because the materials he used were out of date. Thus Pirenne's assertion is erroneous on this point too.

It is hard, then, to accept as very powerful a single one of the four points brought by Pirenne in refutation of the theory of the contemporaneity of the *Periplus* and the *Natural History*. However, as Murakawa¹⁹ points out, I do not think that comparative study of the accounts of the two works can yield a definite answer to the question of their relative priority of composition, or, provisionally supposing the *Periplus* to have been the earlier, the question whether we can or cannot say that Pliny did in fact make use of it.

The second prong of Pirenne's criticism of the 1st century theory is directed to the account of king Malichas of the Nabataeans in ch. 19 of the *Periplus*. The final words of the passage, "...Λευκὴ κόμη, δι' ἧς ἔστιν εἰς Πέτραν πρὸς Μαλίχαν, βασιλέα Ναβαταίων, <ἀνάβασις>" are, in the MS, βασιλέα ἀναβαταίως²⁰; commenting on this, Pirenne considers that this is not the generally accepted *ναβαταίων* but *ἀναβατέων*, and restores this part of the text to read, "...πρὸς Μαλίχαν βασιλέα, ἀναβατέων (Leukè, Komè, par où il y a vers Pétra, chez le roi Malichas, une montée)." The interpretation is thus that Malichas is not king of the Nabataeans but chief of a tribe which extended its power to Petra after the Nabataean kingdom had been annexed by Rome.²¹ So according to her theory, this passage in the *Periplus* cannot not only constitute a ground for the theory of 1st century composition of the work, but rather provides support for the contrary theory. I regret to say that I am not competent to pronounce on her textual criticism. However, even if one provisionally accepts her emendation as correct, it is surely not sufficient ground for instantly denying that Malichas was king of the Nabataeans. Do not the words, 'King Malichas residing in Petra' lend themselves rather more naturally, without powerful proof to the contrary, to the interpretation that he was king of the Nabataeans? It is also a very large question, as A. Dihle²² points out, whether someone no more than a tribal chieftain, in this region, subject to Roman rule, could have in fact achieved such considerable regional authority as to be called 'king (*βασιλεύς*)' by the author of the *Periplus*.

It should now be sufficient, after having dealt with the above two points with which Pirenne attacks the 1st century theory, to mention the following three points among those which she produces as grounds for her 3rd century theory.

(1) First of all, I have no quarrel with the view that regards the date of Charibaël's reign as of great importance.²³ I also share her view²⁴ as to the king in inscriptions with whom he should be identified. However, as I will explain below, I share with other students of the question the difficulty of following her chronology, based on palaeographical methods, too variously inconsistent as it is with knowledge obtained by other methods. Even if Charibaël is to be identified with *krb'l/wtr/yhn'm/mlk/sb'/wdrydn*, the latter was on the throne, according to Pirenne's chronology about 220²⁵, whereas, in fact, as I shall show in chapter 3, below, he may be supposed to have been a king of the second half of the 1st century.

(2) As I have already explained, Pirenne,²⁶ who thinks that Qatabān had already ceased to exist at the time of the composition of the *Periplus*, attaches great importance to the date of the fall of this kingdom, but the presence or absence of references to Qatabān has no great significance for the question as it now stands, and there seems no necessity to repeat the argument at this point.

(3) The third point lies in the interpretation of the political situation of Omān and the coast of the Persian Gulf. It is ordinarily supposed that Persis, which is said in ch. 33 of the *Periplus* to have possessed the Omān region, means Parthia. That is to say, Mesēnē, at the head of the Persian Gulf, which was then under the domination of Parthia, probably extended its power to the Omān region, and the situation presented in ch. 33 of this work is interpreted as a reflection of this.²⁷ Against this Pirenne says that the use of proper names in the *Periplus* is not so inaccurate,²⁸ and she goes on to criticise the accepted view by pointing out that there are absolutely no historical sources which indicate that Mesērē controlled the Omān region.²⁹ According to her, the Persis in question cannot but be Persia under the Sassanian Dynasty, and the account given in the *Periplus* simply shows that the conquest of Ardashīr extended to the Omān region.³⁰ However, we scarcely need Dihle³¹ to point out that the author's knowledge of the Persian Gulf area is universally known to have been vague, so that it seems altogether unreasonable to demand precision in his use of proper names in this area. Then again, she may well maintain that Ardashīr conquered the Omān region, but, as she offers no direct proof, there seems to be nothing to choose between this view and that which attributes control over Omān to Mesēnē. In short, might one not say that the right way of looking at this would be to accept that, either way, there is no collateral proof of what this chapter tells us and so it is useless to try to use it in discussion of the date of composition of the work; and that a correct interpretation may only prove possible *after* that date has been established?³²

I feel that I have made clear above the outline of Pirenne's theory as well as my own contrary view. Finally, I would like to look briefly at the theory put forward by F. Altheim,³³ which is by way of supplementing and complementing that of Pirenne. He cites three points on which his theory is based. Below I deal with two of these, omitting the third which consists of no more than a few corrections of Pirenne.³⁴

(1) First of all, he regards the mention in ch. 26 of the *Periplus* of the conquest (or destruction) of Eudaimōn Arabia as part of the war against Parthia, and holds that the expedition set in motion by Septimius Severus resulted in the southern extremity of the peninsula being reached; and so, he continues, the *Καίσαρ* next mentioned as the conqueror was none other than Caracalla, the actual leader of the expedition during his period of *imperator destinatus* (196–198).³⁵ If this view were correct, it would indeed seem that the composition of the *Periplus* would be 3rd century or later. Against this Dihle³⁶ argues in detail that this expedition would have been involved with northern Arabia at the outside, and that the conquest of Eudaimōn Arabia by Caracalla is impossible. But even without this refutation, it is a well known fact that there is no record of Roman troops

reaching southern Arabia apart from that of Aelius Gallus in 25–24 B.C.³⁷ The *Katōap*=Caracalla theory, cannot, of course stand up, and this item, without any collateral evidence, is the subject of many different suggestions on the part of scholars.³⁸ My own view is that, whatever interpretation one might propose, it would provide no strong ground for establishing the date of composition of the work.

(2) Next, Altheim identifies the situation in north India, of which ch. 47 of the *Periplus* gives a glimpse, as that prevailing before the conquest of the area by Kadphises II of the Kushan Dynasty; and, arguing from the view that Kanishka acceded to the throne any time between 216–218, supposes the *Periplus* to have been written early in the 3rd century.³⁹ Not being a specialist in Indian history, I find it hard to make a precipitate decision as to which of the various views on the date of Kanishka's accession⁴⁰ should be adopted, but I think this Altheim's theory is a minority view with few supporters. It is therefore inevitably most dubious to what extent one should after all accept Altheim's assertion as reliable, based as it is on such a theory.

Above is an outline of the 2nd or 3rd centuries theories which have once more been proposed in recent years. Taking the world of scholarship as a whole, they are doubtless minority views.⁴¹ But there is quite a number of scholars who support the theory of the first half of the 3rd century.⁴² It is of great interest that supporters of this theory are particularly numerous among specialists in the history of ancient South Arabia. While I have been stimulated by contact with the various theories outlined above, especially those of Ryckmans and Wissmann, I have tried in this article to reexamine this question from a standpoint of ultimately criticising these theories. As I have already said, the method followed by Ryckmans and Wissmann in their theories, involves determining the date of composition of the work by the clue provided by Charibaël and Eleazos found in inscriptions, and the method I have followed in this article is not basically different. The question here is bound up with determining to which kings in the inscriptions these two names correspond. In the third and subsequent chapters, I hope to extract from the inscriptions kings with names *krb'l* and *'l'z* (*'l'd*) (on the different spellings, see below), corresponding to Charibaël and Eleazos, and investigate minutely the qualifications of each to be Charibaël and Eleazos.

In what follows, as to the South Arabian chronology in the 1st to 3rd centuries, which is essential to my inquiry, it will be my principle to follow the theories of Ryckmans, Lundin and Wissmann,⁴³ at present the most reliable authorities for this matter.

3. Charibaēl

We can point to five krb'l in South Arabia from the 1st century onwards; which of these kings corresponds to the Charibaēl in the *Periplus*? Charibaēl is mentioned in the following terms in ch. 23:

“And after nine days more there is Saphar, the metropolis, in which lives Charibaēl, lawful king of two tribes, Homēritai and those living next to them, called Sabaioi; through continual embassies and gifts, he is a friend of the Emperors.”

All scholars agree that Saphar here means Zafār, capital of the kingdom of Ḥimyar, and that Homēritai and Sabaioi are respectively the kingdoms of Ḥimyar and Saba'. It is, then, a condition of Charibaēl in the *Periplus* that he resides at Zafār and is king, at least in name, of the two kingdoms of Saba' and Ḥimyar. But opinions are divided as to whether this Charibaēl should be seen as a king of Saba' origin or one of Ḥimyar origin. There are those, such as E. Glaser and W. F. Albright,¹ who think that he was originally a king of Saba' who had stretched out a controlling hand over Ḥimyar at this time, but the majority is agreed in seeing him as a king of Ḥimyar origin.² Whichever of these views is the right one, I would in any case like to attempt an orderly examination of the actual facts concerning these five krb'l.

First, there is of course no historical material recording the conquest of Ḥimyar, nor even any indication of an advance as far as Zafār on the part of krb'l/byn/MSR³, king of Saba', of Ja 642, 643, 643 bis, or krb'l/wtr/yhn'm/MS, king of Saba', of CIH 1, 326 and Ja 563, 564. In view of this, one had best suppose that there is almost no possibility of either being the Charibaēl of the *Periplus*. Further, krb'l/(wtr)/yhn'm/MSRḤY,⁴ king of Ḥimyar, of Ja 666, 667 and Iryani 28, is to be considered, in view of the royal title «MSRḤY», a 4th century king and is therefore also to be excluded as a candidate for our Charibaēl. We are thus left with two krb'l, and these required somewhat detailed examination.

(1) krb'l/wtr/yhn'm/MSR

He was a king of Saba' of the second half of the 1st century,⁵ grandfather of the krb'l/byn/MSR above-mentioned, and he appears on CIH 373 and many other inscriptions.⁶ As Ryckmans⁷ has definitely pointed out, there is no inscription that directly indicates that he rules over Ḥimyar. Even so, I think that this king at some period occupied Zafār, and from there ruled over both Ḥimyar and Saba'. My reasons are as follows:

(i) There is a group of South Arabian coins⁸ clearly distinguishable from the rest by the fact that there is a human head (probably that of the king) engraved on both sides, and also the name of the king in the top part and the letters «rydn» in the lower part on the reverse. Since, with the exception of one king's name (krb'l/(wtr)/yhn('m)), these are all kings of Ḥimyar, it is thought that the letters «rydn» indicate that the coins

were struck in the castle of Raydān at Zafār.⁹ The standard of these coins is visibly influenced by the denarius of the time of Nero, while their style shows almost no change (they were therefore not struck over so very long a period). These facts suggest that their date is from about 50 to 150.¹⁰ The question then arises as to the several coins in the group, inscribed, as stated above with the name, krb'l/(wtr)/yhn(')m, since there was no king in Ḥimyar in this period who bore this name. There were, on the other hand, during this same period, two krb'l/wtr/yhn'm (the k./w./y./MS previously mentioned, and the k./w./y./MSR now under consideration) in Saba'. This naturally gives rise to the supposition that one of these perhaps controlled Zafār and had the coins in question struck there. Then what reason is there to suppose that it was the latter who wielded the control? To answer this question, it is next necessary to try to examine the origin of the royal title, «MSR».

(ii) As to the question of who first used this title in Saba', it would seem almost certain that it was the krb'l/wtr/yhn'm/MSR now under consideration,¹¹ but we cannot decide so simply in the case of Ḥimyar. As investigation of the south-west mountainous area of the Yemen, which was Ḥimyar's stronghold, is still incomplete, so far not a single inscription has been found with the name of a king of Ḥimyar on it earlier than the middle years of the 1st century. The account in the *Natural History*,¹² the statement on RES 2687,¹³ generally known as the «Obne» inscription and thought to belong to the 1st century B.C., together with the existence of a so-called Ḥimyar era of which the first year was 115 (109) B.C.,¹⁴ all lead one to suppose that the kingdom of Ḥimyar itself had already been established by the end of the 2nd century B.C. But no kings have been known with some royal title before ysm/yhšdq/MSR (CIH 41,365; RES 4775; Iryani 6) of the latter half of the 1st century A.D. (about the same period as krb'l/wtr/yhn'm/MSR). In fact, in the case of Ḥimyar and within the limits of the historical sources available, this ysm/yhšdq/MSR is the first king to bear the title «MSR», but this means that the possibility exists that a king (or kings) with the title «MSR» might have already reigned in the earlier period, for which historical material is scanty. Wissmann¹⁵ has given this point his attention and makes the following conjecture as to the origin of the royal title: shortly after the incursion of Roman troops in 25–24 B.C., during this blank period for historical material, the king of Ḥimyar finally captured the Saba' capital of Mārib, and subsequently styled himself «MSR» to commemorate this event. He was soon counterattacked by Saba' forces and driven from Mārib, but he retained the title of «MSR» and his successors followed his example. The king of Saba', too, having restored his kingdom's independence, changed the title «MS», that he had hitherto borne, to «MSR» to cope with the king of Ḥimyar.

However, two questions arise in connection with this conjecture of

Wissmann's. The first is that, in the royal title, «sb'» is placed before «drydn». It is really possible that a victor, when adopting a new title, should place the name of the vanquished before his own? If Ḥimyar did subdue Saba', should not his royal title have rather been «MRS»? Is not the fact that it actually became «MSR», contrary to Wissmann's assertion, because this was first beared as a title by the king of Saba' who subdued Ḥimyar?

Further, the fact that «drydn» is used instead of «ḥmyrm» in the royal title also, again contrary to Wissmann's theory, enables us to perceive that the title «MSR» was first used by Saba'. When the Sabaeans referred to a king of Ḥimyar, as will be shown below, they ordinarily added «drydn» instead of the royal title, in the form, for example, of krb'l/drydn or šmr/drydn. May we not therefore suppose that, also when adding the Ḥimyar name to the royal title of Saba', they followed the same custom, and, instead of mlk/sb'/wḥmyrm, they would use mlk/sb'/wdrydn?

For such reasons, I consider, contrary to Wissmann, that the origin of «MSR» is to be sought in the subjugation of Ḥimyar by the king of Saba'. So, naturally enough, the conqueror to be commemorated must be krb'l/wtr/yhn'm/MSR, the first king of Saba' to bear the title «MSR». And it is also for these reasons that I considered above that this king, but not krb'l/wtr/yhn'm/MS, was the king of the same name inscribed on the coins. His principal object in attacking Ḥimyar must have been to take part himself in the trade in the Indian Ocean, now at its zenith, by acquisition of the various ports in Ḥimyar territory. Therefore, promptly after his military success, he would have removed from Mārib to Zafār, firmly established friendly relations by exchange of ambassadors and presents with Rome, as recorded in the *Periplus*, and, finally, by way of convenience in business dealings, brought his standard of coinage into conformity with that current in Rome. However, this control of Ḥimyar by the king of Saba' did not last very long, and he was soon obliged to withdraw from Zafār. It is my view that ysrm/yhšdq, then king of Ḥimyar, probably himself took the title «MSR» after driving out the Saba' forces, and that this was the beginning of the title in Ḥimyar.

I have explained above my reasons for thinking that king krb'l/wtr/yhn'm/MSR of Saba' resided in Zafār for a certain period, and ruled over both Saba' and Ḥimyar from there. In the light of these, it seems clear that this king is fully qualified to be the king Charibaēl of the *Periplus*. I would next like to examine the last remaining krb'l/drydn.

(2) krb'l/drydn

This king, regarded by Ryckmans and Wissmann as the Charibaēl of the *Periplus*, appears in this form in each of the three inscriptions Ja 578, 586, 589 (all Saba' inscriptions). As I have already explained, the title «MSR» remained in many cases a unilaterally usurped title, on the part

of either Saba' or Ḥimyar, and was therefore hardly ever recognized by the other side, there being only two examples of Saba' using «MSR» with reference to a king of Ḥimyar (CIH 353, Ja 631). Ordinarily we find as in the case of krb'l/drydn that only «drydn» is added, thus constituting a disrespectful form of address.¹⁶ Since no inscriptions from the Ḥimyar side have been discovered mentioning this king, there is at present no firm evidence that he was styled «MSR». But in view of the fact that two kings of Ḥimyar, who similarly bore the addition «drydn», were both styled «MSR», and that kings almost contemporary with him were all styled «MSR», I think it is natural that he too should have had the same title. Wissmann¹⁷ thinks that this king was on the throne about 220, and I have nothing against this.

However, while it is natural to suppose that this king had his residence at Ḥafār and ruled over Ḥimyar, can we go so far as to say that he extended his rule to Saba'? On the evidence of the three inscriptions cited above, there is no such possibility whatever. The situation was rather the reverse. The position seems to have been that the Ḥimyar army, under the banner of krb'l/drydn was waging a tireless defensive war against the attacks of 'lśrh/yḥdb/MSR and his brother y'zl/byn/MSR of Saba' at this time. This does not, however, in anyway disqualify him as a candidate for being the Charibaēl of the *Periplus*. This is because the author of the *Periplus*, as I have already mentioned, was very little acquainted with the political situation in the interior, remote from the coast as it was, and, moreover, without any direct connexion with trade, and so it is perfectly possible that he did not know that the title «MSR» of krb'l/drydn, the Ḥimyar king at Ḥafār, was no more than a usurped title, and could have been under the misapprehension that this king ruled not only over Ḥimyar but also over Saba'. Thus, on the point that he lived in Ḥafār and ruled at least nominally over both Saba' and Ḥimyar, krb'l/drydn must also be recognised to qualify as the Charibaēl of the *Periplus*.

At this point, the question is which, of the krb'l/drydn and krb'l/wtr/yhn'm/MSR, should be regarded as the Charibaēl of the *Periplus*. However, for the right answer to this, we must first examine yet another South Arabian king, Eleazos.

4. Eleazos

Ch. 27 of the *Periplus* has the following to say about Eleazos:

"After Eudaimōn Arabia there is a continuous length of coast, and a bay extending two thousand stadia or more, along which there are Nomads and Fish-Eaters living in villages; just beyond the cape projecting from this bay there is another market-town by the shore, Kanē, of the kingdom of

Eleazos, the frankincense country... Higher up inland lies the metropolis Sabbathath, in which the king lives..."

The name of Eleazos' kingdom is not given by the *Periplus*, but Sabbathath is to be identified with Šabwat¹, the capital of the Ḥaḍramaut kingdom, which appears in inscriptions, so there is no doubt that Eleazos was king of Ḥaḍramaut. It is further certain that the Greek name, Eleazos, corresponds to 'l'z or 'l'd.² Accordingly, confining the question for the time being to the 1st to 3rd centuries, we can pick out three 'l'z and one 'l'd from the epigraphic material. It is a feature of the Ryckmans-Wissmann theory that they regard the last three of these four as being the same person. As the argument develops below, it should gradually become clear how they came to reach this conclusion and why my own view is contrary. We will now first of all examine these four in order in the light of the contents of the inscriptions.

(1) 'l'z/ylt/MḤ/bn/yd"l

The only inscription which records this king's name is Hamilton 8³; since, moreover, this is a short one of only three lines, we are in no position to know anything of this king's achievements or any other details about him. However, as the script is only moderately evolved from the archaic type, Beeston⁴ conjectures that the inscription belongs perhaps to the 1st century B.C. or A.D., and goes on to suggest the possibility of this being the Eleazos, with whom we are concerned. A further point is that neither Ryckmans nor Wissmann makes any mention at all of this 'l'z when discussing the identification of Eleazos.

(2) 'l'z/ylt/MḤ

Apart from CIH 334, Iryani 13, we also know of this king's name, though in very incomplete form, from Ja 640. In the first two of these he is depicted as an antagonist of the king of Saba', š'rm/'wtr/MSR, and, in the third, as subject to this same king of Saba' and troubled with internal rebellion. There are nearly ten inscriptions,⁵ in addition to CIH 334 and Iryani 13, which mention š'rm/'wtr's expedition against Ḥaḍramaut. The expeditionary force that he led was on an unusually large scale, including as it did, in addition to the Saba' army, reinforcements from allied or subject tribes, while Ḥimyar, at this time in a subordinate status vis-à-vis Saba', also took part in the campaign. Though the Ḥaḍramaut army, under the leadership of 'l'z, riposted fiercely, it was of no avail: in the end the capital, Šabwat, and the trading port, Qana' (the Kanē of the *Periplus*) were occupied, and surrender was inevitable. We can tell the subsequent situation from Ja 640: with the weakening of the royal power, rebellions break out all over Ḥaḍramaut, and 'l'z, no longer able to hold out against these by himself, finally looks to Saba' for help.⁶ I intend to deal with the results of these rebellions later, but there is an important point to be made at this point, namely that this king was on the throne

at about the same time as š'rm/'wtr, king of Saba'. Lundin has made it clear that the latter was on the throne about the end of the 2nd century or the beginning of the 3rd, and we would therefore do well to take the same view of the former.⁷

(3) 'l'd/ylt/MḤ/bn/'mdhr

About 15 km. west of Šabwat there is a rocky hill called 'Uqla ('Uqlah), at the southern foot of which there is a group of pieces of rock bearing inscriptions. The first person to discover and record these was H. St. J. B. Philby,⁸ who visited the region in 1936, and the copies of these that he brought back with him were arranged by Beeston and published in two lots.⁹ Later, in 1961-62, Jamme visited the area and made an improved catalogue of the inscriptions surviving there, including some which had been left out by Philby. He then published his results in a single volume.¹⁰ This may be regarded as the most reliable book on the 'Uqla inscriptions at present, and I propose to base my further argument on it.

It seems, from the contents of the inscriptions, that this was the place where the chieftains of Ḥaḍramaut tribes or the ambassadors of neighbouring countries presented themselves, and where exceptionally important ceremonies, centred on the king, were performed, and a large number of inscriptions bearing the names of kings survives. When arranged according to the kings' names, these fall into two main groups, first Ja 925 (=RES 4908) and others relating to 'l'd/ylt, then Ja 949 (=RES 4912) and others relating to yd''l/byn and his sons. So far as the epigraphic evidence goes, it is impossible to point to any consanguineous relationship between 'l'd/ylt and the yd''l/byn, father and sons, and they appear to be of different lineage. The first question that then presents itself is that of their chronological relationship. On the evidence of the disposition of the inscriptions and the type of the script, the personal names which appear in the texts together with the similar nature of their contents, it is agreed among scholars that the two were chronologically continuous, but on the question of their priority opinions are divided between Jamme,¹¹ who would put 'l'd/ylt first, and Ryckmans¹² and Wissmann,¹³ who prefer the yd''l/byn, father and sons. Limitations of space preclude the introduction of the details of the argument here, so I will confine myself to the conclusion and say that while Wissmann's refutation of Jamme's view is hardly at all effective, that of Ryckmans is most richly persuasive, and I myself ungrudgingly accept his contentions on this question as they stand. In short, after yd''l/byn/MḤ/bn/rbšms, there follows the period of his sons 'lrym/ydm/MḤ and yd''b/gyln/MḤ, immediately after whom comes 'l'd/ylt/MḤ/bn/'mdhr.

Having thus made clear the chronological relationship of the kings in the 'Uqla inscriptions, it is next necessary to investigate their absolute dates, and for this the most valuable clues are provided by the references to Ḥaḍramaut kings in the following Saba' inscriptions:

- (i) Ja 629, Iryani 5 (=Condé 3) . . . s'dšmsm/'sr'/MSR and his son mr̄tdm/yh̄md/MSR are making war against yd'1/MḤ.
- (ii) Ja 643, 643 bis . . . krb'1/by'n/MSR is making war against yd'1/MḤ.
- (iii) Na 19 . . . That there be established an alliance between 'lhn/nhfn/MS and yd'1/MḤ is the prayer of the vassal of the former.¹⁴
- (iv) CIH (155), 308 . . . 'lhn/nhfn/MS and his son š'rm/'wtr/MS have concluded a peace with yd''b/ġyln/MḤ.
- (v) CIH 334, Iryani 13 . . . š'rm/'wtr/MSR is making war against '1'z/yłt/MḤ.

We know about the dates of the reigns of the Saba' kings who appear in these inscriptions,¹⁵ so we are in a position to make an estimate, very rough though it be, of the dates of the Ḥaḍramaut kings, as follows: yd'1/MḤ about 130–160; yd''b/ġyln/MḤ, who probably succeeded the former, about 160–190; and the last one, '1'z/yłt/MḤ, as I have already said, from about 190 to the beginning of the 3rd century. Thus, even though we do not know whether there was any blood relationship, we can point to a line of kings in Ḥaḍramaut, yd'1, yd''b/ġyln and '1'z/yłt, stretching from the 2nd century to the beginning of the 3rd.

The next question that naturally presents itself concerns the difference between the above three kings, of whom we know from a series of Saba' inscriptions, and the kings previously extracted from the 'Uqla inscriptions. Neither Ryckmans nor Wissmann¹⁶ has any hesitation in regarding these two groups as being the same, but I myself, for reasons which I will now state, have reached a conclusion the reverse of theirs.

(i) First there is the question whether, from the palaeographic point of view, the series of 'Uqla inscriptions should be regarded as belonging to the 2nd century. Lack of space forbids my going into details, with which I propose to deal in a further article,¹⁷ and I will simply give the conclusion at this point. The script, especially that of the ⟨w⟩ and ⟨f⟩, used in the 'Uqla inscriptions¹⁸ and the Ḥaḍramaut one of the same period (Ingrams I)¹⁹, is clearly of a later type, and in particular resembles that in Ḥimyar inscriptions²⁰ of the second half of the 2nd century and later.

(ii) It was pointed out by Beeston²¹ that the Ḥaḍramaut dialect found in the 'Uqla inscriptions is already beginning to give way, when compared with its original form, then Jamme,²² after his new and minute examination of these inscriptions, discovered that the proper nouns and grammar are very strongly influenced by the Saba' dialect. The most likely conjuncture, whereby the Saba' dialect came to exert so marked an influence on that of Ḥaḍramaut, is probably the time when Saba' and Ḥaḍramaut became directly limitrophe, as a result of the destruc-

tion of Qatabān. Then, if, following Wissmann, one provisionally accepts the 'Uqla inscriptions as belonging to the 2nd century, the influence of the Saba' dialect would appear in the first half of the 2nd century, and that would be too early. This is because Ḥaḍramaut finally destroyed Qatabān in the middle of the 2nd century,²³ so that one would expect the influence of the Saba' dialect to become marked at the earliest from the end of the 2nd century to the beginning of the 3rd.

(iii) One of the 'Uqla inscriptions shows the relations between Ḥaḍramaut and Ḥimyar at the time. This is Ja 923 (=RES 4909) and it records the attendance at the 'Uqla ceremony, presided over by 'l'd, of two Ḥimyarites despatched to Ḥaḍramaut by the Ḥimyar king ṭ'rn/y'b/yhn'm /MSR. That is, according to this inscription, these two kingdoms were on good terms with each other at that time. On the other hand, this was completely at variance with the situation in the time of 'l'z of CIH 334, Iryani 13. Ḥimyar was at the time under the control of Saba' and, far from being on friendly terms with Ḥaḍramaut, had, on the contrary, joined the expeditionary force of king š'rm/'wtr of Saba' in an invasion of Ḥaḍramaut territory. Thus relations between Ḥaḍramaut and Ḥimyar were entirely different at the time of 'l'd of the 'Uqla inscriptions and 'l'z of CIH 334, Iryani 13.

(iv) Again, there is scarcely any possibility of the existence of a king ṭ'rn/y'b/yhn'm/MSR of Ḥimyar at the same time as king 'l'z of CIH 334, Iryani 13 (end of 2nd century to beginning of 3rd). At this time there are no inscriptions whatever indicating a king so named, while there are, on the contrary, a great many showing the existence of kings otherwise named.²⁴ This is a further demonstration that it is wrong to identify this 'l'z with the 'l'd of the 'Uqla inscriptions. On the other hand, if it were half way through the 3rd century, there would be no particular objection to supposing the existence of this king.

The above points lead me to believe that the three kings who appear in the series of Saba' inscriptions and those of the same names in the 'Uqla inscriptions should not be regarded as being the same; and that the former were kings of the 2nd century while the latter belonged to the 3rd. From all this I imagine the situation of Ḥaḍramaut, from the 2nd to the 3rd century, to have been more or less as follows.

Just as yd'l/byn with his sons and 'l'd/yłt of the 'Uqla inscriptions were of different lineage, so was 'l'z/yłt of CIH 334, Iryani 13, probably of different lineage from his predecessors, yd'l and yd'b/gyln. Now, š'rm/'wtr of Saba' was on friendly terms with yd'b but turned round and attacked 'l'z with extraordinary ferocity. Taking this fact together with my belief that

these two Ḥaḍramaut kings were of different lineage, we can tell that the accession of 'I'z was a case of usurpation. It would seem that š'rm could not let pass the fall of the family of yd''b with whom there had been friendly relations since his father's reign.²⁵ Or it may rather be more accurate to suppose that he planned to take advantage of the internal disorder of Ḥaḍramaut to strike a blow for the aggrandizement of his own power. However this may be, this expedition ended very successfully. As has already been said, rebellions broke out on every hand against 'I'z, conspicuously weakened by the seizure of Šabwat, the capital, and Qana'. It cannot be supposed that he could long have retained his royal authority in such circumstances. He would have been overthrown, at the latest, at just about the turn of the 2nd to the 3rd century. And so I think that the man who now succeeded to the Ḥaḍramaut throne was surely none other than yd''l/byn of the 'Uqla inscriptions. This is based on the supposition that, judging from this king's name, he was of the blood of the family that had been driven from the throne by 'I'z, and also on the interpretation of his restoring the castle and fortifications of Šabwat, in Ja 949 (=RES 4912),²⁶ being the restoration of the capital, which had been sacked and destroyed by the Saba' army in the time of 'I'z. It would probably be satisfactory to estimate the period during which this king and his sons were on the throne as about the first fifty years of the 3rd century. In the meantime, Saba', under the rule of the family of 'Išrḥ/yḥḍb/MSR, who succeeded to the throne of š'rm, had been very busily engaged in struggles with Ḥimyar and Abyssinia,²⁷ thus giving Ḥaḍramaut the opportunity to recover from the wounds of its defeat. Ḥaḍramaut cleverly threw off the control of Saba' by linking up with Abyssinia and Ḥimyar,²⁸ and gradually restored its power; and in the time of 'I'd/yłt, in the middle of the 3rd century, as we can tell from RES 3958, which will be examined below, even went so far as to get Beihān, formerly the territory of Qatabān, again under its control.

It has been established above that the 'Uqla inscriptions belong to the 3rd century and that the 'I'd/yłt who appears in the last of them was on the throne from the middle period on. I now propose to move on to the fourth 'I'z.

(4) 'I'z/yłt/MḤ/bn/'mdhr

In RES 3958, which is based on Glaser's squeeze (Gl 1430=1619), the part corresponding to the name of the father of 'I'z is left blank, because of the lack of clarity of the squeeze, but Jamme,²⁹ who later investigated Wādī Beihān, rediscovered the inscription carved on a rock at the foot of Mount Qarneim, and verified that this king's father's name was 'mdhr.

The point most deserving attention in this inscription is the date, 'year 144', given in the fourteenth line. The view has hitherto prevailed

that this date is based on the Ḥimyar era,³⁰ and even now Wissmann³¹ follows this. However,

(i) The first year of the Ḥimyar era is 115 (109) B.C., but all the inscriptions on which dates are recorded according to this era are, with two exceptions (RES 3958, 4197 bis), of the 3rd century or later. There is a gap of 150 years or more between the dates recorded on those two inscriptions and those on the others, and, in the case that the former are also held to be based on the Ḥimyar era, this gap is a blank to the point of being unnatural. Would it not perhaps be a more natural explanation that dating by the Ḥimyar era actually came into use in the 3rd century, and that the dates on the two questionable inscriptions were based on some other era?

(ii) This inscription clearly indicates the situation prevailing after the destruction of Qatabān, and the final destruction of this kingdom by Ḥaḍramaut took place in the middle of the 2nd century, as already stated. This is clearly incompatible with taking the date of this inscription to be A.D. 29 (35).

(iii) According to Albright,³² the type of the script shows this inscription to be considerably later than A.D. 29.

(iv) As Beeston³³ also points out, in the text the main part is drafted in the Saba' dialect in this inscription (and RES 4197 bis), while only the date part is in the Ḥaḍramaut dialect. Taking this in conjunction with the fact that Beihān was under the control of Ḥaḍramaut at the time, we may take this to be evidence that this dating was not based on an era peculiar to the region but on one in use in Ḥaḍramaut.³⁹

Such reasoning leads me to the view that the dates recorded in RES 3958 and RES 4197 bis are not based on the Ḥimyar era but on another era used in Ḥaḍramaut, indicating a date at least later than the destruction of Qatabān in the middle of the 2nd century. But what would this other era have been? One clue is afforded by the 'Uqla inscriptions previously examined. This is the very strong possibility that, in the light not only of his name but also his father's name and the period of his reign, the mid-3rd century Ḥaḍramaut king 'l'd/yłt/bn/'mḍhr is the same man as the 'l'z of RES 3958. Nor am I at variance with the views of Ryckmans and Wissmann³⁴ on the identity of these two 'l'z('l'd). This being so, it follows that the 'year 144' in question, recorded in RES 3958, is based on an era which starts at the beginning of the 2nd century. Ryckmans³⁵ then proceeds to conjecture as follows: at this time, all over South Arabia, there was the very strong influence of nomads who had penetrated from the north.³⁶ Might it not then be that, through them, the so-called Bosra (Bostra) era,³⁷ of which the first year was A.D. 105/106 and which was in general use in

and around the Roman Province of Arabia, had penetrated as far as Ḥaḍramaut?—One cannot readily pronounce on whether the era in use in North Arabia was so easily transmitted to the south, and it also remains questionable whether the intermediary would have been nomads.³⁸ But in the present state of the problem this explanation of Ryckmans' may be regarded as the best, so the 'year 144' of RES 3958 would correspond with A.D. 248/249, and the 'year 172' of RES 4197 bis with A.D. 276/277. The results of our examination of the 'l'z ('l'd) which appear in the inscriptions may be summarized as follows:

- (i) 'l'z/yłt/MḤ/bn/yd'1 (Hamilton 8) of 1st century B.C. or A.D. . . . suggested by Beeston as possibly the Eleazos of the *Periplus*.
- (ii) 'l'z/yłt/MḤ (CIH 334, Iryani 13, Ja 640) of end of 2nd century to beginning of 3rd . . . contemporary with š'rm/wtr/MSR, king of Saba'.
- (iii) 'l'z ('l'd)/yłt/MḤ/bn/'mdhr (RES 3958, 'Uqla inscriptions) of mid-3rd century or later . . . contemporary with t'rn/y'b/yhn'm/MSR, king of Ḥimyar.

5. The date of composition of the *Periplus*

In the course of our investigations in chapters 3 and 4, we found two krb'1 and three 'l'z ('l'd) as candidates for Charibaēl and Eleazos respectively. In the present chapter, I propose to look for a period when both were reigning simultaneously and thence deduce the date of composition of the *Periplus*. For the convenience of explanation I will look at the question from the angle of krb'1.

(1) krb'1/wtr/yhn'm/MSR

This king of Saba' was on the throne in the second half of the 1st century. According to Beeston, the 'l'z/yłt/MḤ/bn/yd'1 of Hamilton 8 reigned about 1st century B.C. or A.D. It is therefore possible, within these limits to regard them as having acceded to their respective thrones at about the same time in the latter half of the 1st century. However, the number of Ḥaḍramaut inscriptions available to us is limited, with the result that the names of that country's kings and the facts relating to their succession are extremely obscure to us compared with those of Saba' or Ḥimyar, and, especially for the 1st century and earlier, we have only fragmentary knowledge of the names of a number of these kings, including the above 'l'z/yłt/bn/yd'1. Accordingly, it is only in default of an otherwise more suitable candidate in our present state of knowledge that we can make a connection between this 'l'z of Hamilton 8 and krb'1/wtr/yhn'm/MSR, and it remains wholly possible that a better qualified fourth 'l'z may emerge from future excavations and investigations. However, whether we accept this 'l'z of Hamilton 8 or not, we can anyway affirm, first, that it is wholly

possible that a king named 'I'z reigned in Ḥadramaut at the same time as krb'l/wtr/yhn'm/MSR, and, secondly, that, therefore, this krb'l is fully qualified to be the Charibaēl of the *Periplus*.

(2) krb'l/drydn

Now, both Ryckmans and Wissmann first identify the 'I'z/yłt/MḤ of CIH 334, Iryani 13 and the 'I'z('I'd)/yłt/MḤ/bn/'mdhr of RES 3958 and the 'Uqla inscriptions as being the same person, and they then use the fact that this person was of the same period as krb'l/drydn as the principal ground for their assertion that the *Periplus* was written in the first half of the 3rd century. But for me, having made a distinction between these two 'I'z('I'd) in the preceding chapter, it is necessary to discuss the respective relationships between each of them and krb'l/drydn.

(i) krb'l/drydn and 'I'z/yłt/MḤ of CIH 334, Iryani 13

In order to be in a position to say anything of the relations between these two, it is essential to be as precise as possible about the dates of their reigns.

First, krb'l/drydn: the date reckoned on the Christian era of 212,¹ recorded in CIH 314+954, in mentioning his predecessor, šmr/drydn (=šmr/yhr'š/MSR), gives a clue for the date of his accession. Further, three inscriptions, Ja 578, 586, 589, which mention this king, all attribute him to the period when the brothers 'Išrh/yḥdb/MSR and y'zl/byn/MSR shared the throne of Saba', and RES 4646 confirms that the joint rule of these two kings did not last beyond 233.² One cannot directly conclude from the lack of any inscription mentioning krb'l/drydn in the period of the single rule of 'Išrh that the former was no longer on the throne by this time, but we certainly have here a clue for the date of his abdication.

Next, 'I'z/yłt/MḤ: it is clear in CIH 334, Iryani 13 and Ja 640 that this king's dates were at least partly concurrent with those of š'rm/'wtr. As it has been related above, this king suffered heavy defeat at the hands of the Saba' army led by š'rm/'wtr, and he was then troubled by internal rebellions, and was replaced by yd''l/byn, while the restoration of the fortifications and the castle of Šabwat, believed to have been destroyed by the invaders, was not yet undertaken. One cannot, in view of this, suppose that he remained so very long on the throne after his defeat. On the other hand, we can tell from Ja 633 that š'rm/'wtr was still flourishing in the period 200–206, but it is necessary to insert another king of Saba', lḥy'tt/yrhm/MSR (Ja 631), between him and the occupation of Mārib by the Ḥimyar king šmr/yhr'š/MSR in 209 (Ja 653). Taking these facts into account, it is hard to suppose that he was still on the throne so very long after the opening of the 3rd century, and the Ḥadramaut expedition was probably carried out at the end of the 2nd century. So the life of 'I'z was probably over very early in the 3rd century at latest, and it would surely be proper to suppose that there is almost no possibility of an overlap, however partial,

between the dates of his reign and those of krb'l/drydn, who had not yet come to the throne in 212.

- (ii) krb'l/drydn and 'l'z('l'd)/ylt/MḤ/bn/'mdhr of RES 3958 and the 'Uqla inscriptions

The 'l'z of CIH 334, Iryani 13 and this 'l'z('l'd) are separated by yd'l/byn and his sons. If this interval is roughly estimated at about 50 years, (that is, two generations), then the later 'l'z('l'd) is dated in the middle of the 3rd century or later, and this date would fit well with that of A.D. 248/249, if we reckon the 'year 144' of RES 3958 according to the Bosra (Bostra) era. This date is probably that of the beginning of his reign. This means that it is almost unthinkable, in this case too, that the date of reign of this 'l'z('l'd) could coincide, even partially, with that of krb'l/drydn, who does not appear at all in any inscriptions later than 233, as has already been shown.

It is clear from the foregoing that the reign of krb'l/drydn is situated just half way along the 50 year interval that separates the 'l'z of CIH 334, Iryani 13 and the 'l'z('l'd) of RES 3958 and the 'Uqla inscriptions, so that there is no possibility of its overlapping with that of either of these. This being so, it is evident that krb'l/drydn cannot be regarded as the Charibaël of the *Periplus*, nor either of the above two 'l'z('l'd) as the Eleazos.

Above we have investigated the possibility or otherwise of the existence of a king called 'l'z('l'd) in Ḥaḍramaut at the same time as either of the two krb'l was on the throne. It has emerged, as a result, that that possibility exists in the case of krb'l/wtr/yhn'm/MSR, whereas it is hardly possible to accept that it does so in the case of krb'l/drydn. The conclusion which this enables us to reach is that composition of the *Periplus* in the latter half of the 1st century is wholly acceptable, and the possibility of its composition in the 3rd century is exceedingly slight.

- (3) The invasion of South Arabia by the Abyssinians

As I have already explained, I have, in this article, tried to establish the date of composition of the *Periplus* by looking in the epigraphic material for the Charibaël and Eleazos found in that work. I think that I can be regarded as having achieved my object by the end of the preceding section. I now propose to conclude this chapter by adducing reasons, from a different angle from that which I have adopted hitherto, why it cannot be supposed that the work was written in the 3rd century, especially in its first half.

Epigraphic material tells us that from the middle of the 2nd century to the 3rd the region of the Arabian peninsula, from the Red Sea coast to the mountainous area of the Yemen, was subject to fierce attacks by Abyssinians.³ They established a fortified bridgehead in the Red Sea coastal region now known as Tihāma, and thence directed their attacks principally against the towns of Ḥimyar and Saba' in the mountainous area of the

Yemen. One can grasp the extreme violence of their incursions at this time by looking at only a portion of the many inscriptions which tell of the war against them—now they would cross the mountains and penetrate the town of Nagrān⁴ in the interior (Ja 577, 635), now they would occupy, if only temporarily, the Ḥimyar capital Ṣafār (Ja 631), or, again, they would control Śawwā (Ja 585), the important Ḥimyar town which appears in ch. 22 of the *Periplus* as Sauē.

On the other hand there is not a single fragment of a reference which gives a glimpse of the Abyssinian invasion in that part of the *Periplus* which gives an account of these areas. Not only were Saphar (=Ṣafār) and Sauē of course under the complete control of Charibaēl; so were such ports as Mūza or Okēlis, which faced the Red Sea. Nor were there any signs that there had been Abyssinian fortifications or colonies anywhere in the Red Sea coastal region. Nor can Wissmann overlook so very great a difference between the information given by the epigraphic material and that of the *Periplus*. To explain the complete absence of references to the Abyssinian incursions in the latter, he conjectures⁵ that, at the beginning of the 3rd century, which would be the period immediately preceding the composition of the *Periplus*, according to his theory, there was probably a big political upheaval in the Abyssinian home country in Africa, and that, because of this, the Abyssinian forces were temporarily obliged to retire from Arabia. It is indeed true that in the first half of the 3rd century there is a temporary cessation of references to the war against the Abyssinians to be seen in the epigraphic material, and this accords with what he says. Even so, no historical material has so far been discovered which would support his conjecture of political upheaval in Abyssinia. Furthermore, as has been related above, 'lṣrḥ/yḥḍb/MSR and his brother y'zl/byn/MSR, contemporary kings of Saba' with krb'l/drydn (Wissmann's Charibaēl), were obliged to fight energetically to drive out the Abyssinians who had invaded Nagrān and Śawwā; in the light of this, it is virtually impossible to believe that, immediately afterwards, these Abyssinians should suddenly vanish from Arabia and leave not so much as a trace behind. Even granting that no material trace was left, the memory of them must have long remained in people's minds. Moreover, the root cause of the warfare between these two areas, confronting one another across the Red Sea, must have been, whatever other elements there may have been, rivalry in connexion with Red Sea trade. Accepting for the moment that the *Periplus* was written in the first half of the 3rd century, is it conceivable that its author, who displays so very strong an interest in the regions along the trade route, should have heard nothing and written nothing of these momentous events?

Looking at the matter in this way, I think that there is no explanation for the complete lack of mention in the *Periplus* of Abyssinian incursions

into the Arabian peninsula, other than that it was written before such incursions became conspicuous. In short, however late one may put the composition of this work, it is inconceivable that it was later than the middle of the 2nd century, when the Abyssinian invasion of South Arabia increased in ferocity. Though this can provide no direct support for my theory of the latter half of the 1st century, it may be said to be a fully effective argument for the refutation of the theory of the first half of the 3rd century held by Wissmann and others.

6. Finally

In this article I have attempted a reexamination of the date of composition of the *Periplus Maris Erythraei*, criticizing theories, which have once again been put forward in recent years, in favour of the 2nd or 3rd centuries, in particular the theories of Ryckmans and Wissmann.

I first searched the epigraphic material for kings who might be considered to correspond to the Charibaēl and Eleazos of the *Periplus*, and I have found two krb'l as candidates for the former and three 'l'z('l'd) for the latter. On then looking for a period when two kings so named respectively were on their thrones simultaneously, it became clear that there was such a possibility in the latter half of the 1st century, whereas it was very hard to accept such a possibility in the 3rd century, of which there was question. From this I was enabled to conclude that the *Periplus* should be regarded as not having been written in the 3rd century, but in the latter half of the 1st, as it had, until recently, come to be believed. And I obtained indirect support for this conclusion from a comparative examination of the accounts of the *Periplus* and of the epigraphic material with reference to the Abyssinian invasion of South Arabia.

However, the use of exclusively South Arabian epigraphic material in this article leads to no closer limits than these, and to obtain a more precise dating it is necessary to make use of information obtained from the study of other areas. What may be considered the most reliable material here is the dates of the reign of Malichas, king of the Nabataeans, previously mentioned. If the *Periplus* was written in the latter half of the 1st century, there can be no doubt that this Malichas was Malichas II. Then taking the dates of his reign (about 40–70) together with those of krb'l/wtr/yhn'm/MSR, who corresponds to Charibaēl, (about 60–90) we obtain the period 60–70. As has been said, there is some material which shows the latter's reign as much as ten years earlier than this. But, as the *Periplus* shows, he had subdued the greater part of Ḥimyar territory, including Zafār, and no small amount of time must have elapsed since accession before he had achieved this. In the light of this, there can, I think, be hardly any possibility of this work having been written earlier

than 60. For this I would regard 60–70 as the period during which the composition of this book took place.

NOTES

1. Introductory

- (1) *Periplus Maris Erythraei*. Surviving MSS consist of one, said to be of the 9th or 10th centuries, in the library of Heidelberg University (*Codex Palatinus Graecus*, 398), and a second, said to be of the 14th or 15th centuries, in the British Museum Library (*Add. Mss.* 19391); it is thought that the latter was either copied from the former or from the same original. There are several critical editions. The most faithful of these to the MS and the one now in general use is that of HJ. Frisk, *Le périple de la Mer Erythrée, suivi d'une étude sur la tradition et la langue*, Göteborg, 1927. There are also several annotated editions. Of these *SchPES* is extremely detailed and is the most widely used today. But Schoff, for whom the Frisk text was not yet available, relied for the most part on the Müller text (C. Müller, *Geographi Graeci Minores* I, Paris, 1855, pp. 257–305). In Japan, too, there is the splendid annotated translation by Kentarō Murakawa, *Erythrā-kai annai-ki* (エリユトウラー海案内記) (*Seikatsu-sha*, 1946). Murakawa used the Frisk text as his basic text, making use when necessary, for purposes of corrections or additional material of Müller and Fabricius (B. Fabricius, *Der Periplus des Erythraeischen Meeres von einer Unbekannten*, Leipzig, 1883). Cf. *SchPES*, pp. 17–21.
- (2) For the history of the theories on the dating of the *Periplus*, see: Fabricius, *op. cit.*, pp. 23–27; *SchPES*, pp. 7–15, 290–293, and Murakawa, *op. cit.*, pp. 7–15; and on more recent studies, see *KP*, IV, cols. 641–642, s.v. “Periplus Maris Erythraei”; J. Innes Miller, *The Spice Trade of the Roman Empire 29 B.C.–A.D. 641*, Oxford, 1969, p. 16, n. 2; H. Bengtson, *Griechische Geschichte von den Anfängen bis in die Römische Kaiserzeit*, 4th ed., München, 1969, p. 524.
- (3) The accounts in chs. 26 and 57 are those which best show at what stage in Indian Ocean trade it was written. Ch. 57 shows what an extraordinarily large contribution was made to the development of trade in the Indian Ocean by the discovery of the route across the ocean using the south-west monsoon (the so-called Hippalus). Ch. 26 shows that the reason for the decline of the once flourishing staging port of Eudaimōn Arabia (Aden) is to be sought in the Egyptian merchants becoming able to travel directly to anywhere overseas. Egyptian merchants had hitherto been content to do business with Indian and Arabian merchants in the ports on the south-west coast of Arabia or in those of the African coast opposite. The discovery of the use of the south-west monsoon at this juncture enabled them to travel directly to India, and this revolution in the pattern of trade is what lies behind the two accounts cited. (Of course, one must not forget, either, that the increasing demand for eastern goods in the Mediterranean world after the establishment of the Roman Empire was yet another factor.) We can see fully from the tone of the two accounts that this revolution had taken place not so very long before the writer's own time. The very fact that Egyptian merchants' expeditions to India were of recent date would itself account for the need of a commercial handbook such as this one. Considerations such as these make it in the highest degree natural that the 1st century theory of the composition of the book should gain general support. Incidentally, the account in ch. 57 of the route to Limyrikē in south-west India corresponds to stage four in the route to India described by Pliny (*Nat. Hist.*, VI, xxvi, 101–106). This stage would have been accomplished

about the middle of the 1st century, on the evidence of Pliny's tale of the freedman of Annius Plocamus drifting to India (VI, xxiv, 84-85); but in view of the results of Sir Mortimer Wheeler's excavations of Arikamedu, the date was perhaps somewhat earlier than this (cf. M. Wheeler, Arikamedu: an Indo-Roman Trading Station on the East Coast of India, *Ancient India*, II, 1946, pp. 17-124; id., *Rome beyond the imperial frontiers*, London, 1954, p. 148).

- (4) The strongest ground for this theory is the reference in ch. 19 of the *Periplus* to King Malichas of the Nabataeans. This king is generally identified as Malchos II, who appears in Josephus, *De Bello Judaico*, III, 4, 2, and was on the throne from about 40 to 70 (there are various theories as to the exact dates of his reign. Cf. *RE*, XIV-1, col. 857, s.v. "Malchos II"; XVI-2, col. 1459, s.v. "Nabataioi"; *KP*, III, col. 927, s.v. "Malchos".) Cf. J. Kennedy, Some notes on the Periplus of the Erythraean Sea, *JRAS*, 1916, pp. 835-836; id., Eastern Kings contemporary with the Periplus, *JRAS*, 1918, p. 112; *SchPES*, pp. 11, 103; Tkač, *RE*, IA-2, 1920, cols. 1462-1463, s.v. "Saba"; M.P. Charlesworth, Some notes on the Periplus Maris Erythraei, *CQ*, XXII, 1928, pp. 93-94; J.G.C. Anderson, *The Cambridge Ancient History*, X (2nd ed.), 1952, p. 88, and Murakawa, *op. cit.*, p. 14.
- (5) I have grouped the theories together for the sake of clarity of exposition, but in fact there is a theory for the first half of the 2nd century, and one for the first half of the 3rd. As will emerge later, there is no connection between them.

2. Outline of recent arguments in favour of the 2nd or 3rd centuries

- (1) J.A.B. Palmer, Periplus Maris Erythraei: the Indian evidence as to the date, *CQ*, XLI, 1947, pp. 137-140; D. W. MacDowall, The Early Western Satraps and the Date of the Periplus, *Numismatic Chronicle*, 1964, p. 271 (according to *KP*, IV, col. 641).
- (2) *Μαυβάνου* given by Frisk, *op. cit.*, p. 14, not *Μαυβάρου*, *Μαυβάνου* or *Μαυβάρου* is confirmed by A. Maricq as faithful transcription of Heidelberg MS (*PiRSAQD*, p. 184, n. 63). It is hard to understand why Palmer, while claiming to follow the Frisk text, gives the erroneous *Μαυβάνου*.
- (3) *PiDPME*, p. 455; p. 459, n. 31.
- (4) Cf. A.M. Boyer, Nahapāna et l'ère Śaka, *JA*, juill.-août 1897, pp. 120-151; E.J. Rapson, The Coinage of the Mahākshatrapas and Kshatrapas, *JRAS*, 1899, pp. 357-404; *SchPES*, pp. 197-200; R. Banerji, Nahapāna and the Saka Era, *JRAS*, 1917, pp. 273-289; V.A. Smith, *The Early History of India*, 4th ed., Oxford, 1934, pp. 220 ff.; R.C. Majumdar et al., *An Advanced History of India*, London, 1950, pp. 118 ff., 1046; K.A. Nilakanta Sastri ed., *A Comprehensive History of India* II, Calcutta, 1957, pp. xviii, 231 ff., 274 ff., 841-842; id., *A History of South India*, 3rd ed., Oxford, 1966, p. 95; A. Maricq, Nahapāna fondateur de l'ère Śaka?, *PiRSAQD*, pp. 203-209.
- (5) J. Ryckmans, Chronologie des rois de Saba' et dū-Raydān, *Oriens Antiquus*, 1964, p. 78 et p. 80, n. 49; *RycCRSR*, p. 14 et Tabl. II. But he too, in *RycPRSA*, p. 77 and p. 79, n. 11., follows the theory of 1st century composition based on the date of the reign of King Malichas of the Nabataeans.
- (6) *WisGL*, pp. 72-77; *WisHAH*, pp. 477-484.
- (7) Cf. Tkač, *RE*, IA-2, cols. 1464-1465.
- (8) *PiDPME*, pp. 441-459; *PiRSAQD*, pp. 167-209.
- (9) *Periplus*, chs. 21-32; *Nat. Hist.*, VI, xxvi, 104.
- (10) *Ibid.*, XXX, xxxii, 63-64; xxxv, 68-69.
- (11) *PiRSAQD*, pp. 167-179.
- (12) *Nat. Hist.*, VI, xxvi, 104.
- (13) *Ibid.*, VI, xxxii, 153-162.

- (14) It may be remarked that the united kingdoms of Saba' and Ḥimyar had no real existence until the end of the 3rd century, when Ḥimyar finally annexed Saba'. Until then they were separate countries, each with its own king. Though from time to time one of them would control the other (which means that the author of the *Periplus* must have happened to pass through on such an occasion), this was never more than a purely temporary phenomenon.
- (18) R. LeBaron Bowen, Ancient trade routes in South Arabia, *ADSA*, p. 40; *RycPRSA*, pp. 87-88.
- (19) Murakawa, *op. cit.*, p. 12. Cf. Tkač, *RE*, IA-2, cols. 1462, 1466.
- (20) Frisk, *op. cit.*, p. 6 and note.
- (21) *PiRSAQD*, pp. 187-193.
- (22) *DiUD*, pp. 13-16.
- (23) *PiRSAQD*, p. 193.
- (24) *PiDPME*, p. 450.
- (25) *Ibid.*
- (26) *PiRSAQD*, p. 200.
- (27) Cf. Boyer, *op. cit.*, p. 132, note; *SchPES*, p. 147, and Murakawa, *op. cit.*, p. 173.
- (28) *PiDPME*, p. 446.
- (29) *Ibid.*, p. 453.
- (30) *Ibid.*, pp. 453-454.
- (31) *DiUD*, pp. 32-34.
- (32) In this connection, Pirenne (*PiDPME*, p. 454) interprets $\pi\alpha\rho'\delta\delta\acute{\omega}\nu$ in ch. 37 of the *Periplus* as $\Pi\alpha\rho\alpha\delta\omega\nu$, and adduces the theory in favour of 3rd century composition of the work on this basis, and Altheim (*AltGH*, pp. 14-15) supports this, but I prefer to follow the common reading, $\Pi\alpha\rho\sigma\iota\delta\acute{\omega}\nu$. Cf. Frisk, *op. cit.*, p. 12; *SchPES*, p. 161; *DiUD*, p. 34, n. 34, and Murakawa, *op. cit.*, p. 185.
- (33) *AltGH*, pp. 11-15; cf. F. Altheim & R. Stiehl, *Die Araber in der Alten Welt* I, Berlin, 1964, pp. 42 ff.
- (34) See note 32.
- (35) *AltGH*, pp. 12-13.
- (36) *DiUD*, pp. 16-25.
- (37) Strabo, *Geogr.*, XVI, iv, 22-24; *Nat. Hist.*, VI, xxxii, 160; Dio Cassius, *Rom. Hist.*, LIII, xxix, 3-8.
- (38) In my view Dihle's interpretation is the best. (*DiUD*, pp. 22-25).
- (39) *AltGH*, pp. 13-14.
- (40) On the date of Kanishka's accession, see Majumdar, *op. cit.* pp. 120-121; Nilakanta Sastri, *A Comprehensive History*, pp. 233 ff., 841; and *Ajia Rekishi Jiten* (Encyclopaedia of Asian History, vol. II, Heibon-sha 1959) p. 208.
- (41) Bengtson, *op. cit.*, p. 524; *DiUD*, pp. 12-13, 35; A. Grohmann, *Arabien*, in: *Kulturgeschichte des Alten Orients* (Handbuch der Altertumswissenschaft, III, 1, 3, 3, 4), München, 1963, p. 28; *JaSIMB*, p. 391; Miller, *op. cit.*, pp. 16-18; D.C. Sircar, *Numismatic Chronicle*, 1966, p. 241 (according to *KP*, IV, col. 641); all these continue to follow the 1st century theory.
- (42) H. de Contenson, Les premiers rois d'Axoum d'après les découvertes récentes, *JA*, 1960, pp. 76 ff.; Ju. M. Kobiščanov, *Aksum*, Moscow, 1966 (according to *RycEESAR*, p. 156); *DoSA*, p. 12.
- (43) J. Ryckmans, *Chronologie, Oriens Antiquus*, 1964, pp. 67-90; *RycCRSR*; *LounRycND*; *RycRHU*; *RycQDSA*; A.G., Lundin, Dopolnenija k spisku sabejskih eponimov, *Vestnik Drevnej Istorii* 97 (1966) 3, pp. 82-91 (cf. *RycEESAR*, pp. 155-156); *WisGL*, pp. 43-77 and Tabls. III, IIIa; *WisHAH*; *WisAAG*, pp. 1-13. (In *WisAAG*, p. 13, there is a table which displays the views of these three most comprehensively; in this article I have followed this table as a guide, except where I consider something to be particularly questionable.) But in his most recent work, Ryckmans entertains some doubts about their views hitherto (J. Ryckmans, *Himyaritica* 4, *Le Muséon*, 1974, pp. 517-521).

3. Charibaël

- (1) E. Glaser (*Skizze der Geschichte und Geographie Arabiens II*, Berlin 1890, p. 167) regards *krb'l/wtr/yhn'm/MSR* as Charibaël; W.F. Albright (*JAOS*, 1953, p. 38) regards *krb'l/wtr/yhn'm/MS* as Charibaël.
- (2) Cf. Tkač, *RE*, IA-2, cols. 1461-1462; *SchPES*, p. 109. and Murakawa, *op. cit.*, pp. 157-158.
- (3) The following abbreviations are used for the royal titles: *MS*=*mlk/sb'*; *MSR*=*mlk/sb' /wdrydn*; *MSRHY*=*mlk/sb' /wdrydn/whqrm(wt)/wymnt*; *MH*=*mlk/hqrm(wt)*. Of these, «*MSR*» requires some explanation. The «*rydn* (Raydān)» which appears in this title is the name of the Ḥimyar king's castle at Zafār (cf. *RycIMAM*, p. 161; *WisHAH*, p. 448) and when, accompanying a pronoun, it becomes «*drydn*», it expresses a meaning such as 'people of Raydān' (that is to say the Ḥimyar tribe), or 'the territory occupied by those people', or 'the state made by those people', and is ordinarily used in inscriptions with the same meaning as «*hmyrm*» (cf. *RycIMAM*, pp. 159-161. W.F. Albright, in *op. cit.*, p. 39, gives a different interpretation, which is mistaken owing to his ignoring the difference between «*rydn*» and «*drydn*»). Accordingly, «*MSR*» comes to mean 'king of Saba' and Ḥimyar'. However, there is a problem: apart from a few short periods, from the latter half of the 1st century, when the first king to bear the title «*MSR*» appears in the inscriptions, to the end of the 3rd century, when Ḥimyar finally annexed Saba', the respective kings of Saba' and Ḥimyar almost always styled themselves «*MSR*» while coexisting in a relationship of mutual antagonism. In fact, although this title originated in the historical fact of the king of one side having subjugated and ruled the other (see below), it was rather rare, until the destruction of Saba' at the end of the 3rd century, for the title to be used by a king actually ruling over both Saba' and Ḥimyar, and in most cases it is thought to have been unilaterally usurped by each side (cf. *WisHAH*, p. 451). As regards *krb'l/byn/MSR*, there can be no doubt that he was king of Saba', as suggested by its appearance in the names of four kings of the same family, each of which was a very generally traditional name of kings of Saba' from early times, and by the places of discovery and the contents of inscriptions on which their names appear.
- (4) Wissmann entirely overlooks the existence of this king (ex. *WisAAG*, p. 13).
- (5) About 60-90 (cf. *Ibid.*). But if the date of 67, given to CIH 609 by Lundin (*Dopolnenija*, p. 88), is accurate, in view of the fact that his son, *hlk'mr*, was already adult at this time, it is perfectly possible to regard his accession as having taken place some 10 years earlier. Only, as is pointed out in *RycEESAR*, pp. 155-156, Lundin's chronology, accurate for the 2nd and 3rd centuries, is not so reliable for the 1st century.
- (6) CIH 373, 609, (750), 791; RES 3895, (4132), (4716), 4771; Ry 540, 544; Ja 879. Cf. *JaSIMB*, pp. 344-346; *WisAAG*, p. 11.
- (7) *RycPRSA*, pp. 81-82. Cf. *RycCRSR*, pp. 12-14; *PiRSAQD*, p. 21, n. 14.
- (8) Cf. G.F. Hill, *Catalogue of the Greek Coins of Arabia, Mesopotamia and Persia*, Brit. Mus., London, 1922, pp. xlv-lxxxiv, 45-76, Pls. VII-XI.
- (9) *Ibid.*, p. lxxviii.
- (10) *Ibid.*, pp. lxxv-lxxvi, lxxxii. Against this, Pirenne (*PiRSAQD*, p. 62) maintains that these coins belong to the period 100-300. Pirenne, who attributes the cessation of the minting of coins at Raydān to the Abyssinian invasion, considered, on the basis of Ry 535 (=Ja 576), that this event occurred in about 300, and counted back from there to arrive at the above dating. However, she misinterpreted Ry 535, as was pointed out by A. Jamme (On a drastic current reduction of South-Arabian chronology, *BASOR*, 145, 1957, pp. 29-30; cf. *RycCRSR*, pp. 17 ff.), and as she herself has subsequently admitted (*PiCISA*, p. 307). It may be added that the Abyssinian incursions into South Arabia were already becoming very pronounced after the middle of the 2nd century. (See below)

- (11) Jamme (*JaSIMB*, p. 344) and Ryckmans (*RycCRSR*, p. 11) maintain that this king's father; ḍmr'ly/byn was also «MSR». But in the inscription (CIH 373), there is no royal title added to ḍmr'ly/byn. The first bearer of the new title in the historical sources was undoubtedly krb'l/wtr/yhn'm/MSR, and I would like to regard the latter as the one who first adopted the new title.
- (12) In *Nat. Hist.*, VI, xxxii, 158., there is the entry, 'the Homeritae with the town of Mesala.' Much of the information before and behind this section was taken from Juba II, who was himself much indebted to Uranius; in view of the time at which the latter lived, we may regard the state of affairs in South Arabia depicted here as belonging to the beginning of the 1st century B.C. (according to *PIRSAQD* pp. 144 ff.). Section 161 of the same volume and chapter of the *Natural History* states, in connection with Aelius Gallus' expedition to South Arabia, that Ḥimyar was 'the most numerous tribe' of all the tribes in South Arabia. This statement and the next item in RES 2687 make it clear that Ḥimyar was already becoming a considerable power in South Arabia in the 1st century B.C.
- (13) An inscription discovered at Qalat in Ḥaḍramaut, believed to be of the 1st century B.C. both on palaeographical grounds and on the grounds of its contents. The author was a vassal of the king of Ḥaḍramaut, and records the construction of fortifications to parry the incursions of Ḥimyar forces. The Ḥimyar forces had moved along the coast and penetrated to about the middle of Ḥaḍramaut territory; in the light of this, the power relationships between Ḥimyar and Ḥaḍramaut were somewhat different in the 1st century B.C. from what they were in the 1st to 3rd centuries A.D. which are described below.
- (14) On *WisHAH* pp. 429-430, Wissmann conjectures that by the first year of this era, which corresponds to 115 (109) B.C., the nucleus of the kingdom of Ḥimyar had probably been formed. As to the year to which the first year of this era corresponds, there are two rival views, 115 B.C. and 109 B.C. In this article I have imitated Wissmann in showing the latter date in brackets. Cf. *RycPCH*, pp. 1-4, 18-24.
- (15) *WisHAH*, p. 451.
- (16) Ex. ḍmr'ly/drydn (=ḍmr'ly/yhbr/MSR) (G1 1228), šmr/drydn (=šmr/yhr'š/MSR) (*Ja* 576, 577; CIH 314+954).
- (17) *WisHAH*, pp. 477-478.

4. Eleazos

- (1) Now called Šabwa. Cf. *DoSA*. pp. 228-233.
- (2) Written «'l'z» in Saba' inscriptions (CIH 334, *Ja* 640, RES 3958) and on an old Ḥaḍramaut inscription (Hamilton 8) but spelt «'l'd» in later Ḥaḍramaut inscriptions ('Uqla inscriptions). Ryckmans (*RycRHU*, p. 281) says that: "dans l'onomastique récente du Ḥaḍramaut, notamment celle des textes de 'Uqla, le «z» primitif, conservé dans le dialecte ancien et dans les autres dialectes sud-arabes, est régulièrement transcrit par «d».
- (3) *BeSISh*, pp. 55-56, Pl. XVIII, fig. 2; cf. *JaRSAIH*, pp. 150-151.
- (4) *BeSISh*, p. 56. He sees the *Periplus* as being written about A.D. 50.
- (5) CIH 334; *Ja* 632, 636, 637, 741 (=756); Fakhry 75, 102; Ry 533, Iryani 13. Cf. *WisHAH*, p. 474; J. Ryckmans, *Himyaritica* 3, *Le Muséon*, 1974, pp. 247-256.
- (6) In view of this, it seems that š'rm'/wtr, rather than driving out 'l'z/yłt, chose to exercise control over Ḥaḍramaut by using him as a puppet.
- (7) *LounRycND*, p. 411; cf. *WisAAG*, p. 13. Ryckmans (*RycCRSR*, pp. 19-20; p. 25, n. 53; Tabl. II) erroneously identifies the Ḥimyar king [šm]r/yhr'š/MSR, who appears with yrm'/ymn (grandfather of š'rm'/wtr) in CIH 353, with the Ḥimyar king of the same name at the beginning of the 3rd century; he therefore inevitably dates š'rm'/wtr

- and 1/2 40 or 50 years later than was the case. On the other hand, Wissmann (*WisHAH*, pp. 456-457) distinguishes these two šmr/yhr'š, dating that of CIH 353 in the middle of the 2nd century, and Lundin confirms this view in the above-cited article.
- (8) H. St. J.B. Philby, *Sheba's Daughters*, London, 1939, pp. 313-316. Cf. *JaUT*, pp. 2-3; *DoSA*, pp. 233-235.
 - (9) A.F.L. Beeston, The Philby Collection of Old-South-Arabian Inscriptions, *Le Muséon*, 1938, pp. 311-333; id., Appendix on the Inscriptions Discovered by Mr. Philby, in: Philby, *op. cit.*, pp. 441-456. Later, the former of these were published as RES 4852-4902 and the latter as RES 4908-4917; after both were given some revision by G. Ryckmans, they were brought together in *RES VII*, Paris 1950.
 - (10) *JaUT*.
 - (11) *Ibid.*, pp. 7-31.
 - (12) *RycRHU*, pp. 277-280.
 - (13) *WisHAH*, p. 468; p. 483, n. 143.
 - (14) Na 19. Not seen by me. Following *ibid.*, p. 466.
 - (15) Following *WisAAG*, p. 13. Ryckmans' chronology differs in this part from that of Wissmann; this, as shown in note 7 above, is due to his erroneous interpretation of CIH 353.
 - (16) *RycRHU*, pp. 280-282; *WisHAH*, pp. 464 ff. But, as already stated above, Ryckmans' chronology in this part is some 40-50 years later than the true chronology, and it is therefore not possible to deal with his arguments on the same footing as Wissmann's. In general here I am directing my argument to the latter, while also mentioning the former when necessary and appropriate.
 - (17) Yūzō Shitomi, Kodai Minami-Arabia-shi no kuronorōjī ni kansuru ikkōsatsu (On the chronology of Ancient South Arabia) (*Orient=Bulletin of the Society for Near Eastern Studies in Japan*, XIX-1, 1976, pp. 33-54).
 - (18) *JaUT*, Pls. I-IV.
 - (19) Following H. St. J.B. Philby, Three new inscriptions from Hadhramaut, *JRAS*, 1945, Pl. X. It is the inscription of yd' 1/byñ/MḤ/bn/rbšms, the script of which is of the same type as that of the 'Uqla inscriptions.
 - (20) Cf. NaNAG 13-14 (J. Ryckmans, *Eretz Israel IX*, 1969, p. 103, Fig. 1), CIH 40 (*CIH I*, Tabl. XI; cf. *RycCRSR*, pp. 22-23), RES 4196 (*JaSIMB*, Pl. I), CIH 46 (*CIH I*, Tabl. XI), G1 1594 (*JaSIMB*, Pl. I; cf. *PiCISA*, p. 309), CIH 448 (G. Garbini, *Iscrizioni sabee da Hakhir*, *AION*, 1971, Tabls. I-IIa), RES 4230 (*JaSIMB*, Pl. J), CIH 569 (*CIH II*, Tabl. XXX), etc. Further, both Ryckmans (*RycCRSR*, p. 25) and Wissmann (*WisAAG*, p. 13) identify the t'rn/y'b of CIH 569 with the king of the same name (early 2nd century) of CIH 457 and RES 4708, 4775, but the «b» and the «w» of the former are clearly of a later period than those of the latter. My own view is that this king should be identified with t'rn/yhn'm/MSR who appears in an inscription discovered at Mosna', of the year 434 of the Ḥimyar era (A.D. 319 or 325) (M. al-Iryani & G. Garbini, A Sabaeen Rock-Engraved Inscription at Mosna', *AION*, 1970, pp. 405-406, Pls. II-III). (For my reasons, see Shitomi, *op. cit.*, p. 40)
 - (21) A.F.L. Beeston, *A Descriptive Grammar of Epigraphic South Arabian*, London, 1962, p. 8; p. 71, n. 12.
 - (22) *JaUT*, pp. 23-31.
 - (23) Cf. *WisAAG*, p. 13.
 - (24) First, there is l'zzm/yhnf/yḥdq/MSR, who was on the throne at the end of the 2nd century at the same time as š'rm/'wtr/MSR of Saba', next there are ysrm/yhn'm/MSR and his son šmr/yhr'š/MSR, who appear in inscriptions of the beginning of the 3rd century (Ja 653, CIH 314+954, RES 4196, etc.), and then their successor, about 220, krb'1/drydn, already examined. Moreover, in the blank in the table of *WisAAG* p. 13, should be inserted 'mdn/byñ/yḥqbḍ/MSR (RES 3433, 5098+5099, also known from coins struck at Raydān), who appears in this table with a question mark in the mid-3rd

- century, between the mid-2nd century šmr/yhrš/MSR (CIH 353, RES 4336) and the late 2nd century l'zzm/yhnf/yhšdq/MSR. (Cf. *RycQDSA*, pp. 298-299)
- (25) Not only friendly relations but, according to Iryani 13, there would seem to have been relationship by marriage (cf. J. Ryckmans, *Himyaritica* 3, p. 252).
- (26) Cf. A. Jamme, *South-Arabian Inscriptions, Ancient Near Eastern Texts Relating to the Old Testament* (J.B. Pritchard ed.), 3rd ed., Princeton, 1969, p. 669. But in the matter of «bytn/šqr» in line 2, Ryckmans' interpretation (*Himyaritica* 3, p. 252) is correct.
- (27) Cf. *WisHAH*, pp. 476-478. I deal below with the war with Abyssinia.
- (28) Ja 577, 923; RES 3884.
- (29) A. Jamme, *Pièces épigraphiques de Heïd bin 'Aqîl, la nécropole de Timna' (Hagr Kohlân)*, Louvain, 1952, p. 1, n. 3. But this inscription, rediscovered in 1950, has not yet been published, which is very awkward. So when I say that it is the same as RES 3958, the statement is entirely based on what Jamme says and it does not mean that I have seen this inscription with my own eyes. Cf. W.F. Albright, *The Chronology of Ancient South Arabia in the Light of the First Campaign of Excavation of Qataban*, *BASOR* 119, 1950, p. 14; id., *JAOS* 73, 1953, pp. 38-39; *BeSISh*, p. 56; A. Jamme, *A New Chronology of the Qatabanian Kingdom*, *BASOR* 120, 1950, p. 27; *JaRSAlH*, p. 151; *RycCRSR*, p. 25; *WisHAH*, p. 482, n. 139.
- (30) Cf. Tkač, *RE*, IA-2, cols. 1464-1465; *RES*, VII, p. 13. Provisionally supposing this to be based on the Ĥimyar era, the corresponding year is A.D. 29 (35). Tkač takes this date to be a clue and considers the *Periplus* to have been written about 40-45 (*op. cit.*, col. 1465). But he follows Glaser in regarding 118 B.C. as the first year of the Ĥimyar era, and calculates the date of RES 3958 to be A.D. 26, but the error of this view is clear from *RycPCH*, p. 2).
- (31) But he regards the 'year 144' of the inscription as an abbreviated form of '344=A.D. 229 (235)', and he takes the closeness of the reign date as ground for regarding the 'l'z/yłt of this inscription (and the king of the same name in the 'Uqla inscriptions, whom he sees as the same as this king-see below) and the 'l'z of CIH 334, previously cited, as the same man (*WisGL*, Tabl. IIIa). It is, indeed, as will be explained below, too absurd to regard RES 3958 as being of the 1st century, and the correct view is to conclude that it belongs to the 3rd century, but this conclusion is by no means based on the reasons he gives. It is also contrary to the facts to identify the two 'l'z of RES 3958 (and the 'Uqla inscriptions) and of CIH 334 as being the same person (see below).
- (32) W.F. Albright, *JAOS* 73, 1953, p. 38.
- (33) A.F.L. Beeston, *Epigraphic South Arabian Calendars and Datings*, London, 1956, p. 35.
- (34) *RycCRSR*, p. 25; *RycRHU*, p. 281; *WisGL*, Tabl. IIIa; *WisHAH*, p. 482. On the «z» and «d» of these two being no obstacle to their identification as the same person, see note 2.
- (35) *RycCRSR*, p. 25, n. 53; *RycRHU*, p. 282.
- (36) «'rb», ordinarily interpreted as 'nomads' or 'beduin' appears in South Arabian epigraphy from the latter half of the 1st century, and, as will be shown below, they exercised, together with the Abyssinians, great influence on South Arabia from the 2nd century on.
- (37) Cf. *RE*, I, cols. 641-642, s.v. "Aera"; II, col. 359, s.v. "Arabia"; *KP*, I, col. 935, s.v. "Bostra"; A. Grohmann, *I. Arabische Chronologie, II. Arabische Papyruskunde* (Handbuch der Orientalistik. Abtl. I, Erg.-Bd. 2, Halbbd. 1), Leiden & Köln, 1966, pp. 4-5.
- (38) Even if not so prosperous as what it had been, there must have been north-south overland trade at this time besides sea-borne trade, and we may perhaps suppose that this era may have been among the northern cultural features imported by way of the overland trade route.
- (39) Cf. *RycPCH*, p. 23.

5. The date of composition of the *Periplus*

- (1) Cf. *LounRycND*, p. 411. I follow Lundin for the dating of the inscriptions cited below.
- (2) The only name to be found in this inscription bearing the date corresponding to A.D. 233 is 'lṣṣ/yḥḍb/MSR.
- (3) Cf. *WisGL*, pp. 65-70; *WisHAH*, pp. 472-477; A.K. Irvine, On the Identity of Habashat in the South Arabian Inscriptions, *Journal of Semitic Studies*, 1965, pp. 178-196.
- (4) Now Najrān. In epigraphic South Arabic «g» was a stop, not an affricate (cf. A.F.L. Beeston, *A Descriptive Grammar of Epigraphic South Arabian*, London, 1962 p. 12).
- (5) *WisHAH*, pp. 479-480.