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Dr. Wissowa on the Argei

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the deflecting force of cognate words which began with *pot-* succeeded finally in detaching *potus* from the now isolated *bibo*, and made it possible on the one hand for the prodigious *bibitus* to spring up, and on the other for Priscian to declare that the participle of *poto* was *potus*.

To sum up: *potum*, *potus*, *poturus*, is for classical Latin the proper supine and participles of *bibo* (and of *poto* if used in the simple sense of drinking); *potatum*, *potatus*, *potaturus* of *poto* in its own frequentative sense. Thus 'mecum i *potatum*' Plaut. Pseud. 1327; 'tamquam leuia quaedam uina nihil ualent in aqua, sic Stoicorum ista magis *gustata* quam *potata* delectant' Cic. Tusc. 5 § 14 (*potata* would have been impossible); '*potaturus* est apud me' Ter. Phorm. 837.

Of the two exceptions to the classical usage that I have noticed in the texts of the writers of classical times one is Celsus 4. 19 ad fin. where *potata* was condemned for a gloss by Targa and is omitted in Darem-*of pota* (= *potum do*) for which see Rönisch *Italia u. Vulgata*, p. 376.

berg's recension; it is certainly in disagreement with the usage of Celsus as already pointed out. The second is Valerius Maximus 2. 4. 5 'calefactamque aquam pueris *bibendam* dedit qua *potata*' e.q.s. Here there is very good MS. authority for *pota*. Valerius uses *potauit* in its proper sense at 3. 6. 6. In Pliny N.H. 20. 136 'ad crapulae grauedines decocuntur folia *poturis*' (*potaturis* E, d) either variant is correct as Latin.

I have set forth the facts relating to the classical usage of these two indispensable verbs because they are widely ignored. I do not claim that they are altogether unknown. For example, after I had discovered for myself the true supine of *bibo* I found in M. Brenouf's *Les Hellénismes dans la Syntaxe Latine* p. 268 and n. (2) 'Le supine n'était pas usité pour certains verbes; ainsi *bibitum* qui ne fut employé que très tard... Il était remplacé par *potum* ou *potatum*. Col. 12. 51. 3 dabitur *potatum* imbecillis bubus' Virg. Buc. 7. 11,' quoted above.

J. P. POSTGATE

¹ I have not been able to find this place.

DR. WISSOWA ON THE ARGEI.

(IN PAULY'S *Real-Encyclopädie*, Ed. 2, Vol. I. Pp. 689 *fol.*)

THIS important article, if (as seems to be the case in Germany) its conclusions be accepted, goes far to solve an old and provoking difficulty in Roman religious antiquities. The section on this subject in my 'Roman Festivals' was written, and indeed rewritten, before the article reached me; and had I fully grasped the importance of Dr. Wissowa's treatment of the subject I should doubtless have written it a third time. This, however, I failed to do until after my book was in print. Now, after most carefully considering Dr. Wissowa's arguments and conclusions, I feel compelled to make a few observations on them.

It will be remembered that there are two rites at Rome connected with the name Argei; (1) in all probability a procession round 24 (or perhaps 27) sacella or sacraria Argeorum, on March 16 and 17; (2) the casting of 27 (or possibly 24, or according to Dionysius 30) *simulacra hominum* made of rushes into the Tiber from the pons sublicius on May 14 (according to Ovid) or

May 15 (according to Dionysius).¹ Of the first of these rites we know nothing certainly, though in Varro L.L. 5. 45-54 we have fragments of what seems to be an itinerary for the use of a procession going round the sacella. Of the second we have tolerably explicit accounts; we know that the Pontifices (and according to Dionysius the *στρατηγοί*) were present at the ceremony, also the Flaminica Dialis in mourning, and that the Vestals themselves cast the simulacra into the river. The connexion between the two rites is not absolutely certain, but has generally been assumed as a fact since Jordan wrote his chapter on the Argei in his *Römische Topographie* (II. 237 *fol.*). Nor is it certain that the number of sacella and the number of simulacra was the same; this cannot be proved from the text of Varro (cp. L.L. 5. 45 and 7. 44). Dr. Wissowa concludes that the number in each case was 27,

¹ The references will be found in full in Wissowa's article, and the most important in my *Roman Festivals*, p. 111 *fol.*

following Mommsen's somewhat cautious note in *Staatsrecht* III. 125; this is the conclusion which best suits his own views,¹ and we may provisionally accept it, remembering, however, that it is not a certainty.

Thus it would seem that we have no very secure foundation for conjectures as to the real meaning of either of the rites. But since the appearance, first of Jordan's chapter, and secondly of that of Dr. Mannhardt in his *Antike Wald- und Feldkulte*, some new light seemed to be thrown on the mystery. We began to believe (1) that the sacella were the centres of some ancient 'Cultusordnung' of the districts which became the four Servian regions: (2) that the casting of the simulacra into the Tiber was a rite of very primitive character, possibly a rain-spell, which may be compared with many strikingly similar ceremonies now familiar to anthropologists. Dr. Wissowa's explanation is altogether different, and as startling as it is interesting. It may be briefly stated thus. Both rites are of late date, probably of the third century B.C., and of Greek origin: they are not survivals of primitive custom or worship, as the 'comparative anthropologists'² would persuade us. Probably (he writes) at some date between the first and second Punic war, a Sibylline oracle directed that, in order to assuage a famine or pestilence, twenty-seven Greeks (the traditional enemies of Rome, as he calls them) should be sacrificed by being cast into the Tiber. If I understand him rightly, he believes that these victims were first detained for a while at certain points in the four Servian regions, so as to spread their healing influence throughout the city (this would be the origin of the March rite): and that two months later they were taken in procession to the *pons sublicius*, and there sacrificed by drowning. He conjectures that the actual sacrifice only occurred on the first occasion, and that substitutes, in the form of the *simulacra*, took their place regularly in subsequent years.

The evidence brought together for this revolutionary theory is somewhat complicated: but the leading points in it seem to be as follows.

1. Argei really = 'Αργεῖοι, as the Romans themselves believed. The history of the

revival of this etymology is this: Mommsen, when writing the third volume of his *Staatsrecht*, applied to Willamowitz-Möhlendorf for an opinion about the word, and part of the latter scholar's reply is quoted on p. 123 of that volume. One sentence only seems to contain anything like a definite pronouncement; it runs thus 'Ein Name für das Hellenenvolk ist das Wort nie gewesen, höchstens in Anschluss an Homer von Dichtern, zum Beispiel von Ennius im *Medea*-prolog, so verwendet worden.' Next H. Diels, in his valuable book on the Sibylline oracles (p. 44, note) amplified this sentence while endeavouring to show that the name Αργεῖοι came to Rome through the Sibylline oracles: this is the amplification: 'Willamowitz (bei Mommsen *l.c.*) hat richtig vorgehoben dass der Name (*i.e.* Argei) griechischen Ursprungs ist ('Αργεῖοι), und bei den Griechen nur in der von Homer abhängigen Poesie synekdochisch für Ἑλληνες eintreten kann. Geht man von diesem Fundament aus, so ist, mein' ich, der Schluss zwingend, dass nur auf dem Wege griechischen Orakelpoesie, die ja mit epischen Materiale wirtschaftet, die Argei in den Römischen Cult, und von da in die Sprache übergegangen sein kann.' Dr. Wissowa now goes yet a step further, quoting Diels as having proved the equation Argei = 'Αργεῖοι, and approves his conclusion that there *must* have been an oracle in the third century ordering the sacrifice of twenty-seven Argei = Greek captives. In spite of these developments, a cautious inquirer will be apt to think that we are after all much as we were about the etymology of the name. I am quite unable to see that either Willamowitz, Diels, or Wissowa have proved anything either as to the history or the etymology of the word Argei. No parallel instance of its use has been discovered, in the Sibylline oracles or elsewhere; and the equation with 'Αργεῖοι is as hypothetical as ever. The etymology may be a little more likely than others, (see however H. Nettleship, *Contributions to Latin Lexicography*, s.v.), but an uncertain etymology is of no practical use without a history of the word — and that history is in this case not forthcoming.

2. Dr. Wissowa's own chief contribution to the argument is the following. The Argean rites are not to be found in the so-called Numan calendar, *i.e.* in the *Fasti* of which so many fragments survive; they are therefore, he argues, of later origin, *i.e.* later in all probability than the Decemvirate. Now we used to explain this absence from the

¹ He holds with Diels (*Sibyllinische Blätter*, p. 42 foll.) that the number 27 should be accepted as having special religious significance at Rome as well as in Greece and elsewhere.

² The inverted commas are Wissowa's, and speak his well-known contempt for the species.

Calendar by assuming that they were of the same character as the Septimontium, Paganalia, &c., which are also absent from the Calendar yet undoubtedly ancient; and that in the well-known definition of *publica sacra* in Festus 245 they are referred to as well as and together with the Septimontium, Paganalia, and Fornacalia. 'Publica sacra quae publico sumptu pro populo fiunt, quaeque pro montibus, pagis, curiis, *sacellis*.' The last words of this passage have generally been taken to refer to a different class of rites from those which were *publico sumptu*; the latter are those noted in the Calendar in large letters, while the other class comprises those divisions of the state which worshipped collectively,—pagi, montes, curiae, and lastly *sacella*, by which word the Argean *sacella* seem to be indicated. (Marquardt, Staatsverwaltung iii. 120: Mommsen, Staatsrecht iii. 123 foll.) Dr. Wissowa is naturally concerned to show that we have been all wrong in referring *sacella* in this passage to the *sacella Argeorum*, and that we have here no explanation at all of the absence of the latter from the Calendar. His argument seems to me a weak one. Varro, he says, uses both *sacellum* and *sacrarium* of the Argean chapels: but the latter he uses twice, the former only once: *sacellum* cannot therefore be the technical word in use at Rome for the Argean chapels, and these latter cannot be referred to in the passage of Festus. That *sacrarium* was the right word has indeed been shown by Jordan (Top. ii. 280). *Sacellum*, according to him, was a general word for an unroofed shrine, which might come into use in a loose way for the same thing indicated by *sacrarium*, which was properly a place in which sacred objects were deposited. I confess I do not see why Festus, or Verrius Flaccus before him, should not have used *sacella* for the Argean chapels in this general sense. Livy's expression is *loca sacris faciendis* (l. 21. 5): he avoids, as Dr. Wissowa says, a technical expression: rather, he does not use it because he does not know it: Varro himself vacillates: why should we expect to find the technical term in Festus alone? Too much surely should not be built on the use of a word in and after the Augustan age for an almost forgotten thing.

But whoever will prove that the *sacella* in Festus's gloss are not the *sacella Argeorum*, must also attempt to show that we have no reason to believe that the latter were the centres of an ancient religious division of the city. This question turns

almost entirely on the passage in Varro L. L. 5. 45 which has been already referred to. Prof. Robinson Ellis has been kind enough to obtain for me from Florence the exact reading of the Laurentian MS.: I find it is correctly quoted by Dr. Wissowa from the second Spengel edition. Varro is discussing the names of the seven hills; he deals with the Capitoline and the Aventine (with the Velabrum), and then goes on thus:—*Reliqua urbis loca olim discreta, cum Argeorum sacraria in septem et viginti partis urbi sunt disposita. Argeos putant e principibus qui cum Hercule Argivo venerunt Romam et in Saturnia subsederunt. E quis prima est scripta regio Suburana, secunda Esquilina, tertia Collina, quarta Palatina.* He then proceeds to take the four regiones one by one, with the names of the hills included in them, some of which, forgotten in his own day, he quotes from the document which he calls *Argeorum sacra* (or *sacrificia*).

There is certainly nothing in this passage as it stands to convince anyone that the *sacraria* indicated local divisions, except the words '*cum Argeorum sacraria in septem et viginti partis urbi sunt disposita*'; and these words as they stand in the MS. do not suit with what follows: for after the parenthetical sentence about the Argei (if indeed he ever wrote that sentence) Varro goes on '*e quis prima est scripta*,' etc., where the '*e quis*' cannot refer to 27 partes urbis, for the regiones were four only. Either something has fallen out before the '*e quis*,' or we must read with Spengel and Wissowa himself '*Reliqua urbis loca olim discreta, cum Argeorum sacraria septem et viginti in quatuor partes urbis sunt disposita.*'

This correction would be satisfactory enough, if we could be sure that the rest of the passage stands as Varro wrote it,—but this is exactly what I myself find it very difficult to believe. How are we to account for the insertion of the sentence '*Argeos dictos putant*,' etc., between '*partes*' and the relative? Why, again, should Varro here use the word *partes* for the four Servian regions, which elsewhere he calls by their proper name?¹ Still, though I have doubts about Spengel's improvement of the sentence, I agree with Dr. Wissowa that the general meaning of the passage, so far as we can discover it, taken in connexion with the whole of the context, does not suggest that

¹ After a fresh reading of his chapters 45–46, I am inclined to think that Varro here uses *partes* of four districts of the city before they became technically organised as *regiones*. '*E quis (partibus) prima est scripta regio Suburana*,' &c., he adds.

the *sacraria* were necessarily the centres of religious divisions of the city. O. Müller did not so understand it, as may be seen by his note: and even in England Burn, who was a scholar and knew his literary authorities well, was not misled: 'Varro means,' he writes (*Rome and the Campagna*, p. 39, note 1), 'that the rest of the city was divided already into districts (*i.e.* regiones) at the time the Argean chapels were instituted.' But on the other hand the passage is too doubtful to admit of being used to prove that the *sacraria* Argeorum were *not* the *sacella* of which Festus speaks: from his words we are compelled to believe that there were in Rome some *sacella* which were the religious centres of some local divisions: and if they were not the Argean *sacella*, what were they? No attempt is made to answer this question except by referring to a passage in Cic. *de Leg. Agr.* ii. 14, 35,¹ which seems to me to have no bearing on the difficulty whatever.

3. A third argument used by Dr. Wissowa, but one on which he does not seem to lay much stress, is that two out of the three days of the Argeorum *sacra* (supposing that the third was May 14 and not 15 as Dionysius reports), are days of even numbers, and that thus the rule is violated, which holds good with a single exception² throughout the Roman calendar, that State festivals must take place on days of odd numbers. This, he argues, indicates that the Argeorum *sacra* were of later date than that of the drawing up of the calendar. He is here on ground which is peculiarly his own, and where I confess I hardly feel equal to following him. But I may venture to ask two questions: (1) can we be sure that the ancient moveable feasts which were undoubtedly older than the calendar, *e.g.* Ambarvalia and Compitalia, never took place on days of even numbers? and (2) are not the days which immediately precede Ides and other festivals closely connected with these in character (so Mommsen, *C.I.L.*, vol. i., ed. 2, p. 290), and would there not be less objection, as in the case of March 14, to retaining these as the days of ancient festivals, even after the formation of a regular calendar, than to fixing them on ordinary days of even number? March 16

precedes the Liberalia (anciently Agonia), and May 14 precedes the Ides. Again, it seems to me that those who believe that the *sacra* Argeorum belong to the same class of festivals as the Septimontium, etc. can as legitimately argue that, as not belonging to the State festivals proper, they might never have been subject to the rule of odd numbers, as Dr. Wissowa can argue that from the point of view which he adopts as to the date of the *sacra*, they ought to have been subject to that rule. So with another argument which he uses in passing, viz. that if the Argeorum *sacra* were older than the calendar, they would have had an influence on the character of the days: March 14 is marked C and May 14 F in the calendar, while all State festival days are N or NP. But the days on which the Compitalia were usually held (Jan. 3 and 4) are marked C: so also those on which the Paganalia were held (Ovid *Fasti* l. 658 foll.), and the date of the Ambarvalia was usually May 29, as we have reason to believe, which is also marked C.

These are Dr. Wissowa's chief arguments for his theory of the late and Sibylline origin of the Argean rites; and I confess that I find them far from convincing. I must add that there are also one or two difficulties which seem to me fatal to it. Why were not the decemviri sacris faciundis, who had charge of the Sibylline oracles and superintended the rites ordained by them, present at the ceremony? The *personnel* of the ceremony at the bridge emphatically suggests an ancient Roman origin: Pontifices, Vestals, and Flaminica Dialis in mourning. Dr. Wissowa has simply made no attempt to get rid of this objection to his view; it is in fact impossible to do so. He frankly acknowledges that the Flaminica is not known to have put on mourning for any but ancient Roman festivals (see my *Roman Festivals*, 115). Dionysius (i. 38. 3) says that the *σπαρτηγοί* were present, *i.e.* according to Jordan (*Top.* i. 1, 288) the praetor urbanus, who certainly did preside at some ceremonies of the *ritus graecus*: but so did he also at the Compitalia, as we learn from Gellius 10. 24. 3. Almost in despair, as it would seem, for his theory, Dr. Wissowa appeals to the fact that human sacrifice was foreign to the Roman character and practice, and insists that the drowning of twenty-seven persons must be Greek in origin: to which we might answer that those who have once been convinced that Mannhardt's explanation from parallel primitive customs

¹ 'Sunt enim loca publica urbis, sunt sacella, quae post restitutam tribuniciam potestatem nemo attigit, quae majores in urbe partim periculi perflugia esse voluerunt. Haec lege tribunicia Xviri vendunt.' I know of no clue to the identification of the *sacella* here mentioned.

² This exception he has tried to explain in his work *de Feriis*, ix. foll. See my *Roman Festivals*, p. 44.

throws more light on the mystery than anything that has as yet been written about it, do not need to believe that it originated in human sacrifice at all.

Another difficulty, almost as fatal in my opinion, is this. If twenty-seven Greeks were really sacrificed at some date between the first and second Punic war, when records were beginning to be constant and reliable, why has every trace been lost of such an astonishing circumstance? It is exactly what the Epitomist of Livy would naturally have picked out to record, and that it would be in Livy's own work we may be absolutely sure. The much less striking sacrifice of a pair of Gauls and a pair of Greeks in 226 B.C. and again in 216 B.C., is frequently referred to (see Wissowa's references, p. 699); yet of the supposed oracle and its twenty-seven victims there is not even a hint to be found in all later literature. All memory of it must have vanished with astonishing celerity: for no Roman scholar ever alludes to it in attempting to explain the *Argeorum sacra*. Many such attempts were made, and Dr. Wissowa rightly brushes them aside: but it does not seem to occur to him to ask why the true

explanation should have been utterly forgotten of a rite so recent in origin as he believes it to have been.

I cannot but think it a pity that this eminent scholar should so absolutely decline to learn anything from the despised comparative anthropologists. I venture to say that if he had really studied, and not merely referred to, Mannhardt's *Baumkultus* and *Wald- und Feldkulte*, he would have been saved the trouble of constructing an entirely new theory on very doubtful foundations. He need not have agreed with Mannhardt's conclusions and explanations; but he might have recognised that they cover the known facts far better than one which, like that I have been examining, flies in the face of the evidence afforded by the leading features in the cult. It certainly cannot be said of Mannhardt's explanation, as assuredly it must be said of Dr. Wissowa's, that it leaves out of account not only the *personnel* of the procession to the *pons sublicius*, but the importance of the bridge, and of the river itself, in the general character of the ritual.

W. WARDE FOWLER.

A HOROSCOPE FROM EGYPT.

IN Messrs. Grenfell and Hunt's *Oxyrhynchus Papyri*, II. we have at p. 137 a horoscope, which after communication with Dr. Rambaut they believe to be inexact. Not feeling satisfied with some of the assumptions made in their commentary, I referred the case to the Astronomer Royal and now Mr. P. H. Cowell of Greenwich Observatory having very kindly determined the position of all the planets, fixes the date of the horoscope in A.D. 14. The horoscope is entirely correct. He writes that Saturn was on the boundary then between Sagittarius and Capricorn, just leaving Sagittarius not to return for twenty-eight years: the Moon entered Taurus on September 29 and left it October 1. Two or three interesting consequences may be noticed. (1) The missing year of Tiberius' reign will be the first; he succeeded on August 14, and September 29 in this document is counted in his reign. This is evidence that has been needed for some time for finally determining how in Egypt the emperors' reigns were delimited. (2) The date according to the

old Egyptian reckoning comes out correctly as the night between September 29 and 30. (3) The date by the Augustan fixed calendar makes that calendar begin as it was intended by its patron to do, on August 30. Investigation is needed to ascertain whether this is not in reality the case in other instances, in which case Dr. G. F. Unger's rule (*I. Müller Hand. d. Kl. Altertums-Wissenschaft* vol. i. p. 778) will need revision. (4) The night is made to begin at sunset, which would have been on the night in question at Alexandria almost exactly at 6.0 p.m. The birth we are told was *ὥρα τεταμένη τῆς νυκτὸς* and the Sun was in the Sign next to that which was setting. The time therefore would be about 9.0 p.m. The next Egyptian day began about 3 a.m. (5) The Julian year diverged from the Egyptian year at the exact rate, of course, of one day in four years: it is conceivable that the Augustan year was based on the Sothic or fixed Egyptian year, which diverges from the vague Egyptian at a rather greater rate. If so, in every 112 years or