

and again, without a dative, in 1362.<sup>3</sup> That ἐγγελαῖν does not denote laughter at Pentheus' appearance has indeed been suggested by at least two scholars, but somewhat doubtfully. Jeanne Roux, in her edition of *Bacchae* with translation and commentary (Paris, 1972), translates 842 'Tout plutôt que d'être ridiculisé par les Bacchantes', but in her commentary she rightly observes 'Penthée ne redoute pas que les bacchantes se moquent de son costume, mais qu'elles bravent impunément son autorité'. See also E.-R. Schwinge, *Die Verwendung der Stichomythie in den Dramen des Euripides* (Heidelberg, 1965), p. 395. Neither scholar cites the passages from *Medea* which confirm this sense for ἐγγελαῖν.

In this scene, from 810 onwards, Dionysus begins to dominate the mind of Pentheus, perhaps, as Dodds and others have suggested, by appealing to the Dionysiac urge within him, and when he refers to the expedition to spy on the Bacchantes as a reconnaissance he may be, as Dodds puts it, 'rationalising his unacknowledged lust to pry into the women's doings'. All through this part of the stichomythia he wavers between curiosity, bluster and acquiescence. There is, however, a contrast between this scene and the next (912–70), where he is completely controlled by Dionysus and no longer has a mind or will of his own. In this scene the domination is not yet complete, as is shown by 851–3. Pentheus is still attempting to assert his independence, and I take it that at 842 his line of thought is ostensibly something like this: I still find repugnant the idea of dressing as a woman, but anything would be better than allowing the Bacchantes to triumph over me, and a reconnaissance in disguise could be the first move against them. On this view no emendation is needed, apart from the trifling change required by Pierson's ἐγγελαῖν.

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<sup>3</sup> In *S. El.* 807 ἀλλ' ἐγγελάωσα φροῦδος Jebb translates 'She left us with a laugh'. Clytemnestra's pretence of grief at the report of Orestes' death is no doubt hypocritical, but I feel some doubt about whether she would be described as literally 'laughing'. Perhaps the sense is rather 'triumphant', 'exulting'. Cf. in the same play 277 ἐγγελάωσα τοῖς ποιουμένοις, which Kells, in his note *ad loc.*, translates 'exulting in her deeds'.

## NEW DEVELOPMENTS IN THE PROBLEM OF THE ATHENIAN PLAGUE

### I. Two New Candidates

The first of these is not a single disease but a group of three: 'all the clinical and epidemiological evidence described by Thucydides' (henceforward T) 'can be attributed to infection with influenza virus complicated by a toxin-producing strain of noninvasive staphylococcus' (A. D. Langmuir, T. D. Worthen, J. Solomon, C. G. Ray and E. Petersen, *New England Journal of Medicine* [henceforward *NEJM*] 313 [1985], 1027–30). This initial analysis is in fact supplemented (1028) by bullous impetigo in an attempt to explain the marked skin symptoms which are not ascribable to the other two diseases: streptococci produce flushes of the skin that end in desquamation – something which Langmuir *et al.* admit T would have described if present.

The authors (henceforward L) themselves admit (1029) that certain features do not square with T: (a) influenza epidemics produce a high proportion of pneumonia cases but T does not record such symptoms and L admits that T's careful observation would not have missed them. (b) The high mortality of about 33% (based on T's

figures and accepted by L) far outstrips that of the most damaging of modern influenza epidemics (1917–19). (c) The severe gastro-intestinal symptoms described by T are not produced by influenza. L therefore introduces a staphylococcus infection, which can be very lethal in individual cases. But staphylococcal epidemics do not seem to have been recorded, though L claims that such infections have been associated with pandemics and epidemics of other diseases. L however acknowledges that the symptoms of such infections may vary substantially in different cases and therefore feels able to postulate a variety not yet observed, arguing that new discoveries about staphylococci are now abundant. L reiterates this point in *NEJM* 315 (1986), 1172 in reply to my discussion which immediately preceded it. Finally they needed to postulate the presence of impetigo along with the other two diseases in order to explain the skin symptoms, as already noted. Although individual patients may suffer simultaneously from two or three of these conditions, L's theory requires that all occur together in all the thousands of cases at Athens since they showed the same symptoms. This is of course possible but in the absence of any subsequent record of such a complex clinical situation it hardly breaks the claim that no recorded plague matches that in T,<sup>1</sup> even though this may happen one day.

The assumption of a combination of three distinct diseases (with symptoms which may have been separately familiar) also raises a severe problem in explaining the acquisition of immunity which T reports. This suggests a single disease and a totally unfamiliar one, as T also says.

In *NEJM* 314 (1986), 855 D. M. Morens and M. C. Chu argue that staphylococci do not produce explosive epidemics of the kind proposed by L nor could infections be absorbed from the surface of the skin into the lungs as L suggests. They also stress the absence of pneumonia, the unsuitable time of year for influenza (summer) and the epizootic aspect.

Morens and Chu floated a second candidate for T's plague but do so with great reservations. It is Rift Valley Fever (from Central Africa) which was only recorded c. 1930 for the first time but clearly may have existed earlier. The authors frankly admit that while some of the Fever's symptoms fit T strikingly well, it totally lacks other crucial ones – notably blisters, open sores and loss of extremities. L, in *NEJM* 314 (1986), 856, adds that the respiratory symptoms of the Fever are not sufficient to match T, whilst jaundice and haemorrhages, prominent in the Fever, are absent in T.

It thus seems that L's combination of diseases joins the many past candidates which seem to fit in certain respects but not in all, and that the Rift Valley Fever is in the same category.

## II. *The Effects of the Plague on Birds and Animals*

Some progress has unexpectedly been possible in this area. T says that vultures disappeared while dogs which ate diseased corpses perished. He is clearly reporting that *only* carnivorous creatures suffered: dogs, if they refrained from eating corpses, survived, and T could hardly have omitted to mention if there had been serious losses among horses required by the 1,000 cavalrymen – over 2,000 if we allow for remounts. As dogs and horses live in close association with their masters it seems clear that the plague was not communicated between humans and non-humans by touch or breath.

<sup>1</sup> *CQ* 29 (1979), 282–301. Dr J. C. F. Poole, my co-author, died in 1985. Dr J. Potter has kindly put me in touch with the necessary reading and experts in the medical area.

L argued that T's reference to victims dying ὡς περ τὰ πρόβατα showed that sheep were also dying: but they do not claim that the sheep were carnivorous. They were misled by the idomatic use of the definite article into a rendering of the Greek to mean 'as *the* sheep did' instead of 'like sheep' – a rendering not favoured by classical translators nor supported by the context, since the phrase occurs not in the passage about the effect on animals but the one about the psychological effects on men – dejection and despair which sapped resistance and left them easy prey.<sup>2</sup> 'Like sheep' is a simile for submission.

T thought that the effect of the plague on dogs and vultures might provide a decisive clue to the disease: but corpses have not usually been left exposed in many parts of the world where scientific observation is possible. So the issue seemed unhelpful until it occurred to me that Parsees have a rule (only relaxed in dire circumstances) that corpses should be exposed to vultures. It seemed unlikely that any careful records had been kept of what happened to the vultures when Bombay (the most populous Parsee centre) was afflicted by plague, but with the help of experts I discovered that most meticulous records have been kept at least from the eighteenth century A.D.<sup>3</sup> These showed that the influenza epidemic of 1917–19 produced rises in corpses from the annual rate of about 1035 (1917) to 1446 (1918) and 1561 (1919). No vultures died from eating their flesh, though there was a marked shortage of human officiators.

The truth is that vultures are remarkably invulnerable to infection from diseased flesh of all forms of creatures. Dr D. Houston, a specialist in the study of the diet of vultures, assures me that all vultures have as their main food the flesh of creatures which have died from diseases, not freshly killed prey.<sup>4</sup> They have therefore become highly pathogen-resistant through high acidity in their stomachs and are only likely to succumb to powerful infections if they happen to have minor cuts in their crops – something which would only happen to individual birds, and could not explain a total disappearance of the vulture population.

<sup>2</sup> The movements of sheep in the period 431–28 B.C. are therefore irrelevant to the identification of the disease, but non-classical students of the subject must be warned about the suggestion (*NEJM* 315 [1986], 1172–3) that the sheep and cattle, evacuated to Euboea in 431, might have been brought back after the end of the Spring invasion – only to be lost (as L concedes) by the rapid Spartan invasion of Spring 430. L's only ground for this strange assumption seems to be that the leaving of them in Euboea for the duration 'seems unlikely'. Much more unlikely in fact is an unnecessary return of animals highly vulnerable to a hoplite or cavalry invasion, since animals are much slower to move than troops and there was no guarantee of only one invasion each year. It must be remembered that the Athenian infantry were not allowed out of the Walls to provide protection, that meat was not a part of normal Greek diet and that there was no grazing space for more than a few sacrificial animals inside the Walls.

<sup>3</sup> Dr J. R. Russell and Professor J. Hinnells put me in touch with Desturji Doctor Firoze Kotwal of Bombay who kindly dealt with my enquiries. The record referred to is *Parsee Prakesh*, ed. R. B. Paymaster (Bombay, 1936), Vol. 5, Pt. 5, p. 426.

<sup>4</sup> D. C. Houston and J. E. Cooper, *Journal of Wildlife Diseases* 11 (July 1985), 306–13. They list many frightful diseases which the vultures' stomachs can destroy including, significantly, streptococci (310). It is of course theoretically possible (since T saw no corpses of vultures) that they might have left Attica because they had become aware of danger and wished to avoid it. But learning aversion is a slow process (*Behavioural Biology* 17 [1976], 61–72, 87–98), and it is easy to understand how vultures that had eaten diseased flesh would not have died on the spot, since the bird's digestion of food may take 5 hours and the disease itself would take time to develop. In any case if vultures took warning of danger this would show that they were vulnerable to the disease, which is the crucial point.

### III. *Medical Silence on Contagion*

Recently our suggestion in *CQ* 29 (1979), 296–300 about the silence of medical writers in the ancient world on contagion has been questioned.<sup>5</sup> We are somewhat unjustly accused of claiming to be the first to note T's primacy in making a written record of it: but against this see loc. cit. 295 n. 52. Nor did we claim that T was alone in his awareness of contagion: we cited historians and poets and agreed that lay people believed in it, merely pointing out that no ancient medical writers mention it.<sup>6</sup> This was clearly due to a dogmatic belief in the miasma theory. We pointed out that this silence could hardly be explained as due to the theoretical nature of the treatises or by the suggestion that they were concerned 'not with the epidemic illnesses but with the pathology of the individual' – since one work is specifically devoted to *Epidemics* and there are many references to them elsewhere in the Corpus, whilst awareness of the risk of contagion is surely highly relevant to the pathology of the individual. It clearly would have been more helpful to warn people of the danger of human contact than to recommend them to seek better airs or to abstain from exercise so as to breathe in less bad air. Nor does it help defenders of the Hippocratic School to argue that nothing in the Corpus is actually inconsistent with knowledge of contagion,<sup>7</sup> since failure to mention a fact so vital both for knowledge and for survival is surely incomprehensible. The absence of such a statement, even in parts of the Corpus now lost, is confirmed by the silence of Galen and later writers. Even a famous Arab doctor, well versed in classical scientific writings (but probably not in Thucydides) discovered contagion for himself during a plague in Spain in the late Middle Ages.<sup>8</sup> Finally, as we noted, the distinguished scientist William Harvey who, like all doctors up to the 17th century, was brought up on Hippocrates and Galen, broke out by his discovery of the circulation of the blood but was still loyal enough to attack Fracastoro's *De Contagione*. All these doctors must have regarded contagion as a popular superstition like the Evil Eye or colds from draughts. In the Plague of London (1666) Judges still carried posies against evil airs, as immortalized in a nursery rhyme.

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<sup>5</sup> J. Solomon, *Maia* 37 (1985), 121–3. His suggestion that the miasma theory is more scientific than T's careful study of the disease is hard to swallow, unless it is held that any theory is better than none. All scientists may at times have to make intuitive jumps but these have to be tested against evidence and abandoned if they fail. Even if doctors discounted the inferences made by laymen (and recorded by T) from experience in Athens, it is surprising that the miasma theory was not abandoned when at the siege of Potidaea during the Plague the besiegers were beset by it while the besieged were unaffected. They shared the same air, but were separated by a wall. It should be noted that doctors also ignored T's clear report of acquired immunity – a phenomenon which was only officially recognised by them in the 18th century.

<sup>6</sup> The one possible passage in the Hippocratic Corpus which is adduced was discussed by Dr Poole and myself (loc. cit. 298 with nn. 55–6).

<sup>7</sup> Cf. S. Hornblower, *The Greek World 479–323 B.C.* (London, 1983), p. 303 n. 3.

<sup>8</sup> Cf. *The Legacy of Islam*, ed. Arnold and Guillaume (Oxford, 1931), p. 340.

### A DELICACY IN PLATO'S *PHAEDO*

Plato's striking figure of the 'child in us' at *Phaedo* 77e5 takes on an added lustre when viewed in the light of the theory of explanation Socrates develops between 100b1 and 105c7.

Socrates' theory aims to explain why certain objects have certain properties: why