Commentary Two
Careful with that axe, Eugenius

I. Background

The preceding two chapters represent the elaboration of an idea that I first had as an undergraduate. In the course of writing essays on intrusive burials in Iron-Age Britain and on the earliest Anglo-Saxon graves, I chanced upon the idea of using Gerald Dunning’s three criteria for identifying imported pottery\(^1\) to try and spot the graves of incomers. This seemed to yield interesting results. Eighteen months or so later, as a new D.Phil. student sitting in on Tania Dickinson’s legendary special subject on the Anglo-Saxon Migrations, I thought it would be interesting, in discussing fifth-century metalwork, to apply the test to the earliest so-called Germanic burials in Gaul, as described in Horst-Wolfgang Böhme’s monumental study of ‘Germanic’ graves between the Elbe and the Loire.\(^2\) This appeared to cast doubt upon the ‘Germanic’ nature of these graves and suggested an interesting avenue for future research, albeit one tangential to the thrust of my doctoral work on the region of Metz, wherein almost no such graves are found.\(^3\)

A year thereafter, I was invited to participate in a conference which Hugh Elton and John Drinkwater were planning, on fifth-century Gaul. There being little specifically ‘fifth-century’ about my Metz work, the idea of resuming my inquiry into the supposed ‘early Germanic’ graves sprang to mind. As I read such of the work on these burials as I could find in York I was struck by how illogical it was. That there was nothing intrinsically ‘Germanic’ about the graves seemed to be staring the authors in the face. On occasion they appeared to notice this but all continued to fall back on the ‘Germanic’ explanation. Bailey


\(^{3}\) For which, see below, p. 163.
Young’s 1980 article in the *Bulletin de Liaison de l’Association Française d’Archéologie Mérovingienne* showed this very clearly. It was as though Young, having assembled all the pieces of the ‘social’ explanation, demolishing the ‘Germanic’ interpretation, felt at the last minute that he could not go through with so radical a final step. It was perhaps time for someone with more youthful bravado to take Young’s argument to the next stage.

As the concluding sentence of chapter 2 makes clear, I presented my paper at the conference very much as an intellectual exercise: a ‘what if?’ scenario, challenging people to show me where I had gone wrong or to provide evidence that made the ‘Germanic’ interpretation plausible. In so doing they would strengthen the traditional views and make the archaeology of the Frankish settlement rather easier. Yet, the paper was very well received at the conference, by the smattering of archaeologists present as well as by the historians. The latter, not having been ‘indoctrinated’ into the traditional views as students, were perhaps better placed to look dispassionately at the data and their degree of fit with the proposed interpretations. The sense seemed to be ‘you must be right; why has no one spotted this before?’ Actually they had but I was at this point unfamiliar with the work of De Laet and Van Doorselaer.

I had, then (again, as the concluding sentence of chapter 2 makes clear), no particular vested interest in proving that my argument was correct. I was perfectly willing to be shown that the traditional interpretations were solid, so I could get on with trying to examine the routes, mechanisms and processes of Frankish settlement in Gaul (something that did concern my thesis). Disappointingly, in the immediate aftermath of the conference’s publication there did not seem to be much response at all and those that did appear were insufficient to make me reconsider my point of view. Two responses, by Martin Welch and Simon Burnell, are considered in chapter 3. Another discussion of the topic was presented by Dick Whittaker in his study of the late Roman frontier. Although a very interesting analysis, Whittaker still

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5 ALRF, p. 169 (above, p. 110), n. 15.