

INTRODUCTION

KEITH HOPKINS: SIGHTING SHOTS

Building the Wigwam

Much research is a waste of time; the marginal productivity of each extra footnote is low. Besides, time spent on detail stops one from doing something more useful.

‘Some sociological approaches to Roman history’ (1974)¹

Such provocative openings are at best only half-truths. They are gaffly aphorisms deliberately intended to sting a late-afternoon graduate seminar into sharp response (here Oxford in January 1974). Keith Hopkins delighted, like any skilled soapbox rhetorician, in wittily, repeatedly and unrepentantly overstating his case. His target was a perceived lack of intellectual enterprise amongst his fellow ancient historians (and, of course, only amongst *some* ancient historians – but this is polemic).² In reviewing the third edition of the *Oxford Classical Dictionary* for the *Times Literary Supplement* in February 1997, H. offered qualified praise for ‘the inclusion of several hundred synoptic or analytical pieces’, singling out a handful of new contributions for their ‘exemplary clarity, intellectual economy and interest. ... They provided the necessary information, but subordinated incidental facts to general problems.’ But he also fretted about the growth in the number of entries which he dismissed as ‘unnecessary fillers of insignificant omissions made in the previous editions: yet more rarely mentioned Greek colonies, minor characters from Roman Republican and imperial history, forgettable towns in the Roman imperial provinces’. He noted, for example, that the articles on the thirteen successive Hellenistic

¹ 1974a: 1; see too 1990: 624–5; Hopkins and Burton 1983b: 132 caption to table 3.2.

² For an approved list, see 1978b: 252 (a tactic not later repeated).

Sociological Studies in Roman History

kings of Cappadocia all named Ariarathes or Ariobarzanes – ‘about whom we know very little’ – occupied two columns: taking up ‘1/1600 of the whole classical world’.³

For H., the *Oxford Classical Dictionary* unhappily exemplified what he found least attractive about the priorities of many of his colleagues: an obsessive interest in detail (‘perhaps that is the masochism of our profession’⁴); a privileging of proper nouns (delightfully damned as ‘the upper-case mentality of so many British classicists’⁵); and an involuted concentration on a limited cache of evidence only interrupted by fits of donnish excitement about occasional new discoveries which helped ‘foster the illusion of academic progress’.⁶ In H.’s sharply drawn caricature, classics was an inward-looking discipline in which too many scholars were so enamoured of each other’s ability to extract data from often technically difficult and refractory texts (in long-dead languages in which there was a steadily shrinking competence) that they neglected to interrogate the contexts in which that information was embedded. ‘For too many ancient historians history is an account of the “evidence”, not of the humans or of the society in which the evidence was created.’⁷

There was nothing strikingly new in this critique. Thirty-five years before H.’s provocative graduate seminar, the historian, philosopher and archaeologist R. G. Collingwood had recalled in his *Autobiography* that, as an undergraduate in Oxford before the First World War, he had experienced Roman history in the process of transformation (his heroes were Haverfield and Mommsen), but that Greek history ‘was left high and dry by the tide of new methods’. Shunned by the cleverest students, classical Greece remained the dull preserve of ‘the scissors-and-paste man’.

At bottom, his business was to know what ‘the authorities’ had said about the subject he was interested in, and to his authorities’ statements he was tied by the

³ 1997b.

⁴ Hopkins 1987: 121 [essay 4: 146]; see too 1978c: 185.

⁵ 1997b; see too 1972a: 356, 1978c: 185; Hopkins and Burton 1983a: 41 n. 16.

⁶ 1994a.

⁷ 1978c: 186; and see especially the critique in 1972a and the ‘Sieve syndrome – or a fixation on pebbles’ in 1990: 623–5.

Introduction: Keith Hopkins: Sighting Shots

leg, however long the rope and however flowery the turf over which it allowed him to circle. If his interest led him towards a subject on which there were no authorities, it led him into a desert where nothing was except the sands of ignorance and the mirage of imagination.⁸

H. – an enthusiastic quoter of Collingwood⁹ – did not echo his optimistic view of any continuing improvement in the state of Roman history: ‘alas, scissors-and-paste people still dominate the ancient history profession, and some even pride themselves for it’.¹⁰

To be sure, neither Collingwood nor H. was simply dismissive of facts or ‘authorities’. Rather, H.’s anxieties are best illustrated by the sharp contrast (drawn in a number of essays in this volume) between induction and deduction. On the inductivist side stand historians whose sedimentary understanding of the ancient world consists of a careful and continuous accumulation of data. ‘In this view, the ancient sources provide our major authority for the description and interpretation of events; description of the evidence should therefore be comprehensive, and discrepancies in the evidence resolved.’¹¹ For H., the result was a too frequently misplaced confidence in an understanding of the past unruffled by a stifling preponderance of texts written by and for a narrow, highly educated élite (and, in turn, refracted by the sometimes frankly eccentric preoccupations of monk-librarians in medieval scriptoria). ‘Credit goes to the ancient historian who makes the best pattern out of the largest number of pieces and cites the most obscure sources relevantly.’¹² Ancient history was still tightly tied by the leg: constructed and constrained by the surviving ancient evidence (as though ‘the sources by and large faithfully reported the world in which they lived’¹³), and dangerously prone to glib over-generalisation (‘the Romans thought’, ‘the Jews believed’)

⁸ Collingwood 1939: 79–80, see too 82–3.

⁹ 1978c: 182–3, 1995–6: 44, 1998: 191 n. 10 [essay 12: 439 n. 10].

¹⁰ 1995–6: 65 n. 8.

¹¹ 1972a: 355; see too 1983b: p. xiii.

¹² 1978c: 182.

¹³ 1978c: 183.

Sociological Studies in Roman History

on the basis of a slight handful of supposedly authenticating citations.¹⁴

By contrast, deductivists (with H. as their self-appointed poster boy) emphasised the importance of working out patterns which might help more broadly to contextualise the ancient data. For H. (who rightly prided himself on his own culinary abilities), this was a difference in approach which separated ‘a pre-packed meal from a factory’ and ‘a crafted confection from a chef. The ingredients are partly the same, the results significantly different.’¹⁵ In ‘On the probable age structure of the Roman population’ (1966) [essay 3] and its reprise ‘Graveyards for historians’ (1987) [essay 4], H. used the recently available United Nations model life tables (an aggregate of a vast amount of census data from a wide range of developing countries) to argue that a reconstruction of the Roman population based on the age of death given in funerary inscriptions – 43,000 examples in the western half of the empire¹⁶ – resulted in a distribution of age and mortality unrecognisable in any other pre-industrial society. ‘It is inconceivable that the *pattern* of Roman mortality should be so unlike that of all other known populations.’¹⁷ As Walter Scheidel makes clear in his thoughtful response to these two essays:

H. did not simply seek to answer an empirical question: instead, his contribution focuses on the feasibility of exploring Roman population history – on the nature of the evidence itself. His studies are as much an exercise in applied epistemology as demographic analysis.¹⁸

Tested by reference to an external standard¹⁹ (the UN model life tables), recorded ages on Roman funerary inscriptions were ‘demographically ludicrous’.²⁰ ‘Instead, and this point seems worth stressing, Roman tombstones provide us with a biased

¹⁴ 1964–5: 311, 1978c: 183–5, 1983b: 8 n. 13 (quoted below p. 36), 1983d, 1991a: 482 n. 14, 484 n. 18 [essay 8: 318 n. 14 and 321 n. 18], 1993: 8 n. 8 [essay 11: 403 n. 8], 2000a: 5; Hopkins and Burton 1983b: 156 n. 49; and see below n. 57.

¹⁵ 1998: 191 [essay 12: 439].

¹⁶ 1987: 113 [essay 4: 135].

¹⁷ 1966a: 255 [essay 3: 121].

¹⁸ [154].

¹⁹ To follow H.’s carefully chosen language at 1966a: 264 [essay 3: 134].

²⁰ 1987: 119 [essay 4: 143].

Introduction: Keith Hopkins: Sighting Shots

set of commemorations. Commemorative practice is useful for analysing Roman commemorative practice ... useless for understanding Roman patterns of death.²¹

H.'s concern with pattern and plausibility is elegantly exemplified by 'Christian number and its implications' (1998) [essay 12]. Selecting the end-point was straightforward: an acceptance of the conventional estimate of the number of Christians in the Roman empire at the beginning of the fourth century AD (at the time of the Emperor Constantine's public profession of Christianity) at around 6 million – roughly 7–10 per cent of the empire's population.²² But early Christianity had started small. The number of 'first' Christians in AD 40 (soon after the crucifixion) was set at 1,000. On the assumption of a constant growth rate between 40 and 300, there were then only 7,400 Christians in 100, 100,000 by 180 and 1 million by the mid-third century.²³ H.'s utter lack of commitment to any of these arbitrary figures is crucial. They are neither necessarily correct nor true.²⁴ For a start, there is no compelling reason to assume that Christianity grew at a constant rate. Perhaps it advanced rapidly in its early decades: but then it must have slowed later. Perhaps growth fluctuated or sometimes even declined. 'Drawing a single path of consistent growth is merely an intellectual economy in the face of competing probabilities, and in the absence of reliable data.'²⁵ Rather than provide any definite answer, H.'s calculations aimed to circumscribe a problem.

This approach to evidence and argumentation is perhaps at its clearest in 'Taxes and trade in the Roman empire (200 BC–AD 400)' (1980) [essay 6], which explored the relationship between the flow of taxes and trade in a unified and increasingly

²¹ 1987: 115 [essay 4: 137].

²² 1998: 191–2 [essay 12: 439–40].

²³ 1998: 193 fig. 1 [essay 12: 441 fig. 12.1]. Straight-line growth between 40 and 6,000,000 over 250 years works out at 3.35 per cent (compounded) per annum.

²⁴ 1983a: 85 [essay 7: 272]: 'Without any commitment as to its truth, let us tentatively estimate ...'

²⁵ 1998: 194 [essay 12: 442]; for a useful parallel, see the discussion of coin loss in 1980a: 107 [essay 6: 226]: 'I should stress that assuming a constant rate of loss is a heuristic device, not a description of reality', and 1995–6: 53.

Sociological Studies in Roman History

monetised Mediterranean world. ‘For the sake of clarity, I have canvassed several probabilities in the form of propositions, but the evidence is so sparse that it is difficult to *prove* that each proposition is right.’²⁶ It might be more helpful to say that not all of the seven propositions advanced were equally persuasive. While H. – revisiting the arguments in ‘Rome, taxes, rents and trade’ (1995–6) and ‘Rent, taxes, trade and the city of Rome’ (2000) – was eager to defend, modify and improve individual propositions, he remained chiefly concerned with the overall pattern. His objective was to establish a framework (like the steady-line growth of Christianity) ‘against which the fragments of evidence can be tested, or around which they can be fitted’.²⁷ H. was (ruefully) aware that the piecemeal evidence would only support each proposition more or less convincingly than the others. Hence the emphasis on achieving plausible coherence: ‘all these arguments, and the evidence from which they are derived, are partial, but they draw strength from their inter-relationship. They back each other up. ... This is what I have called a wigwam argument, in which weak arguments prop each other up.’²⁸

It seems a fair claim that the style and shape of H.’s arguments are (in his own formulation) ‘distinctively different from the norm’²⁹ – at least it is a claim put to the test by the essays collected in this volume and those published by H. (some the result of collaboration) in *Conquerors and Slaves* (Sociological Studies in Roman History 1) (1978) and *Death and Renewal* (Sociological Studies in Roman History 2) (1983).³⁰ But to

²⁶ 1980a: 101 [essay 6: 213].

²⁷ 1998: 194 [essay 12: 442] and see below pp. 15–16.

²⁸ 1980a: 116 and n. 43 [essay 6: 241 and n. 43] with B. D. Shaw 1982: 23 and Jongman [264]. See too the formulation in 1978b: 19–20: ‘Unfortunately there is hardly any sound evidence with which this generalisation can be validated; yet it seems more attractive than any alternative I can think of. There are several pieces of evidence, each insufficient or untrustworthy in itself, which seem collectively to confirm it. I call this the wigwam argument: each pole would fall down by itself, but together the poles stand up, by leaning on each other’; and 1995–6: 42 (quoted below p. 15).

²⁹ 1995–6: 42.

³⁰ *Conquerors and Slaves* (1978b) consisted of five essays: (1) ‘Conquerors and slaves: the impact of conquering an empire on the political economy of Italy’; (2) ‘The growth and practice of slavery in Roman times’; (3) ‘Between slavery and freedom: on freeing slaves at Delphi’ (in collaboration with P. J. Roscoe); (4) ‘The

Introduction: Keith Hopkins: Sighting Shots

insist on viewing his argumentative strategies, as H. frequently demanded, as taking sides in a methodological tug-of-war between inductive and deductive approaches to ancient history-writing is ultimately unprofitable. Perhaps (and more kindly) it should be understood instead as a deliberately swashbuckling stance which, to plunder H.'s own phrases, offered a convenient 'simplification of a complex reality'³¹ – a handy 'intellectual economy in the face of competing probabilities'.³² But simplicity and economy have their costs. In wittily ridiculing the perceived deficiencies of conventional scholarship, H. risked trivialising his own close commitment to the ancient evidence. As Wim Jongman rightly remarks (in his commentary on 'Taxes and trade in the Roman empire (200 BC–AD 400)' [essay 6]): 'if there is one thing that characterises H.'s work, it is precisely the passion for facts and empirical detail.'³³ For example, the fulcrum of 'On the probable age structure of the Roman population' [essay 3] was a study of seven thousand funerary inscriptions from Rome, Italy and North Africa.³⁴ The third essay in *Conquerors and Slaves* (written in collaboration with P. J. Roscoe) – 'Between slavery and freedom: on freeing slaves at Delphi' – analysed inscriptions which recorded twelve hundred legally binding acts of slave manumission executed between 201 BC and AD 100.³⁵ One of the most persuasive propositions in 'Taxes and trade in the Roman empire' [essay 6] (proposition 6: 'the integration of the monetary economy in the high empire') was supported by 'the analysis of over 90,000 silver coins found in five regions of the Roman empire: southern Germany, northern Italy,

political power of eunuchs' (a lightly revised version of 1963a); (5) 'Divine emperors or the symbolic unity of the Roman empire'. *Death and Renewal* (1983b) was made up of four essays (two co-authored with Graham Burton): (1) 'Murderous games'; (2) 'Political succession in the late Republic (249–50 BC)' (= Hopkins and Burton 1983a); (3) 'Ambition and withdrawal: the senatorial aristocracy under the emperors' (= Hopkins and Burton 1983b); (4) 'Death in Rome' (in collaboration with Melinda Letts).

³¹ 1995–6: 41.

³² 1998: 194 [essay 12: 442].

³³ Jongman [267]; see too Woolf [528–9].

³⁴ 1966a: 259 n. 22 [essay 3: 124 n. 20].

³⁵ 1978b: 133–71 with the data conveniently summarised at 140 table 3.1.

Sociological Studies in Roman History

Britain and Gaul, the Balkans and a garrison town in Syria'.³⁶ The lengthy and detailed demographic analysis of senatorial career-patterns in the late Republic and early empire in *Death and Renewal* (co-authored with Graham Burton) was based on the close study of five hundred men elected (or appointed) to the consulship.³⁷ A final example: one of the central arguments in 'The political economy of the Roman empire' (2009) [essay 13] was driven by H.'s own enthusiasm for new scientific research on deep-drilled core-samples from Greenland, glacial lakes in Sweden and Swiss peat bogs. (By measuring a marked rise in windborne pollutants, especially lead, in the first and second centuries AD – and to levels not reached again until the eighteenth century – these ice-cores could be argued to capture the atmospheric results of a surge in the production of silver in the Roman mines in Spain. For H., the increased availability of silver, mostly for the imperial mint, was an index both of the prosperity that came with empire and of the importance of monetisation in the development of a more integrated Mediterranean-wide economy.³⁸) To this scatter of illustrations of close engagement with ancient data, add, for good measure, H.'s learned excursions into the heavily fortified technical fields of textual criticism ('A textual emendation in a fragment of Musonius Rufus: a note on contraception' (1965) [essay 2]), papyrology ('Conquest by book' (1991) [essay 10]) and epigraphy.³⁹

Softening (but not conceding) his position in 'Rome, taxes, rents and trade', H. offered 'a slightly shame-faced, perhaps also two-faced apologia'.

Actually, there is ample room for both compromise and overlap between model-builders and inductivists; their positions are more complementary than opposed. After all, deduction like induction is partly a rhetorical pose. The

³⁶ 1980a: 112 [essay 6: 235]; see too 1978a: 39–40 n. 11 [essay 5: 165–6 n. 11].

³⁷ Hopkins and Burton 1983a and b; see particularly 1983b: 130–3 on the research design.

³⁸ 2009: 197–8 [essay 13: 518–20] with Woolf [528]; see too 2004: p. xix.

³⁹ See, for example, 1978a: 41–2 n. 13 [essay 5: 167 n. 13], 1980a: 105 n. 16 [essay 6: 222 n. 16], 1983a: 108 n. 34 [essay 7: 295 n. 34] on *IG XII suppl.* 348; or 1995–6: 55 on Hyginus. H. as expert is cleverly assessed in Morley 2006: 30–2.

Introduction: Keith Hopkins: Sighting Shots

model-builder, if he/she is going to have any chance of success, has to know much of what the sources tell us. The pure inductivist, if he/she is going to be understood outside a narrow range of committed specialists, has to think through what the implications are of his detailed arguments.⁴⁰

The shift in language is also important. To be sure, H.'s ancient history-writing was, first and foremost, propositional and problem driven. He described this approach as 'matrix thinking'⁴¹ (a term used once, and swiftly discarded) or, more satisfactorily, as model-building. What mattered most was the 'fall' of the data; that is (as 'On the probable age structure of the Roman population' [essay 3] exemplifies), H. was chiefly concerned to plot the ancient evidence against a wider set of possibilities – or, perhaps better, probabilities. 'Models allow us to construct whole pictures, into which the surviving fragments of ancient source material can be plausibly fitted.'⁴² From that point of view, H.'s Roman history was at base as inescapably anchored to a bedrock of traditional source-citation as the ancient history-writing to which he objected.⁴³ Whatever the distinctive difference of his preferred style of argument, H. shared with his colleagues a steadfast concern to understand the often poor and patchy evidence for the ancient world by testing its strengths, deficiencies and limitations.

One key concern was to establish the field of play. That is most clearly illustrated by the use of UN model life tables in 'On the probable age structure of the Roman population' [essay 3] or the initial postulate of straight-line growth in 'Christian number and its implications' [essay 12]: 'the straight line ... is like a set of goal posts in a game of football; arbitrarily placed, but good to measure the game against'.⁴⁴ It was equally important to establish the boundary conditions – the parameters – of any proposition. This might usefully be done by a series of 'crude and speculative' calculations. 'My

⁴⁰ 1995–6: 41–2; see also 1998: 186 n. 2, 194 [essay 12: 433 n. 2, 442–3] and the optimism (later abandoned) of 1972a: 356. Note too the comments of Harris 2005: 99.

⁴¹ 1995–6: 66 n. 14.

⁴² 1995–6: 41; see too 1983a: 95 [essay 7: 287–8], 1998: 194 [essay 12: 442] quoted above p. 6.

⁴³ Millar 1979: 170; B. D. Shaw 1982: 30, 49–50; Osborne 2004a: 7; Harris 2005: 93.

⁴⁴ 1998: 194 [essay 12: 443].

Sociological Studies in Roman History

objective here is not accuracy, but a rough order of magnitude to tease out implications which might remain obscured without rough figures.⁴⁵ In ‘Taxes and trade’ [essay 6], H.’s estimate of the gross domestic product of the Roman empire started with a benchmark proposition: ‘we can make a *minimum* estimate by multiplying the size of the population by the amount of food necessary to keep that population at the minimum level of subsistence’.⁴⁶ These ‘sighting shots’⁴⁷ – ‘a first fix’,⁴⁸ ‘guesstimates’,⁴⁹ ‘a crude guess’,⁵⁰ ‘provisional, simplifying calculations’,⁵¹ ‘closer to guesses than to fact’⁵² – were the building blocks of H.’s parametric reasoning. ‘We make a simplifying assumption, to see where it leads us, without facing up initially to all the complexities of the real world. It is as though, in order to guess the weight of an elephant, you first imagine it to be a solid cube.’⁵³

This pattern of reasoning was also important on a smaller scale. H. was always wary of validating the exceptional because widely scattered data when averaged yielded a plausible result (that ‘is like confirming reports about giants and pygmies by reference to average height in England’⁵⁴); or of being too quick to allow an outlier to defeat a general proposition (‘I call this the Mt Everest gambit’⁵⁵ or the ‘Irish dwarf syndrome’⁵⁶). Above all, H. was always reluctant to assume (at least as a starting-point) that the Roman empire had broken loose from the broad material constraints faced by any pre-modern

⁴⁵ 1983a: 88 [essay 7: 276–7]; see too 1978b: 32 n. 41, 56, 110 n. 23, 1991b: 134 [essay 10: 364].

⁴⁶ 1980a: 117–18 [essay 6: 244].

⁴⁷ 1983a: 100 [essay 7: 296], 1991b: 134 [essay 10: 364], 1978b: 3 n. 6.

⁴⁸ 1983a: 85 [essay 7: 271].

⁴⁹ 1995–6: 73 n. 73, 1998: 193 n. 17 [essay 12: 442 n. 17].

⁵⁰ 1978b: 69p.

⁵¹ 1995–6: 45.

⁵² 1983a: 95 [essay 7: 288].

⁵³ 1995–6: 42, 1998: 192 [essay 12: 440].

⁵⁴ 1966a: 251 [essay 3: 114]; see too the careful explanation (for classicists) of mode and median in 1964–5: 309 n. 1; also 1966a: 249 [essay 3: 110]; but not always: ‘variance explained is the correlation coefficient squared’, 1980a: 111 [essay 6: 230] with Morley 2006: 31 n. 18.

⁵⁵ 1995–6: 65 n. 5; see too Hopkins and Burton 1983a: 41.

⁵⁶ 1990: 625; dwarves are repurposed in 1995–6: 43.