to moderno che – non ho qui il tempo di argomentare dettagliatamente – si forma nel XVIII secolo, tanto che H. Dilly si chiedeva nel 1979, «l’arte ha più di duecento anni?». Diceva E. Buschor nel 1942 – e non era un rivoluzionario – «Kunst ist nicht immer Kunst gewesen». In effetti il concetto di arte applicato al mondo antico è del tutto anacronistico, è una caratterizzazione di una totalità di testimonianze visive che sono oggetto dell’indagine dell’archeologo; parimenti si usa il termine artista per definire un pittore, un ceramografo, uno scultore, un fonditore, un vasaio. Quindi quando si parla di arte antica usiamo termini che di per sé sono imprecisi. Lavorare in questa direzione mi sembra l’unico modo di fare un po’ di chiarezza terminologica e quindi anche metodologica. Alcuni dei contributi qui raccolti mi sembra che vadano in questa direzione e il volume, pur nella sua eterogeneità è pieno di spunti, quindi meritevole, anche in considerazione del fatto che si tratta di un’iniziativa curata da giovani studiosi.

Roma  

Marcello Barbanera


Celtic art has long had an association with classical studies. The first major study was by Paul Jacobsthal,² by training a classical art-historian, while the development of Celtic art styles was closely linked to connections with the Mediterranean worlds of Greece, Etruria, and finally Rome. But until recently studies of Celtic art have been a specialist sub-discipline, ignored by many Iron Age archaeologists. This has been particularly true in Britain. Here, most Celtic art came from hoards or stray finds, often from rivers or peat-bogs. The British Iron Age had no strong tradition of burial with grave goods. This is in strong contrast to the core areas of Celtic art on the Continent, such as the Aisne-Marne of northern France or the Hunsrück-Eifel of western Germany, where burial finds were common. Burials were the key evidence for Iron Age studies in these areas but in Britain settlement archaeology led the way, and the ‘stray finds’ of Celtic art have been largely ignored.

Recent years have seen renewed attention to Celtic art, with attempts to integrate the British art evidence into the archaeology of the Iron Age. This book is therefore very timely. It is the outcome of a three-year project, led by Professor Chris Gosden (Oxford University) with the support of Dr Duncan Garrow (hereafter G&G), and funded by the Arts and Humanities Research Council, which was designed to put Celtic art in Britain into its archaeological context. There were two other major outcomes of the project – a valuable set of conference proceedings³ and a programme of scientific dating.⁴

¹ This is a revised version of a review originally prepared for the Prehistoric Society’s website.
³ D. Garrow/C. Gosden/J.D. Hill (eds), ‘Rethinking Celtic Art’ (Oxford 2008).
This volume provides a stimulating study which sets fresh agendas in the field. Its title gives a clue to the approach – ‘Technologies of Enchantment’ was a phrase coined by the anthropologist Alfred Gell to describe the way in which complex art could enchant an onlooker by the complexity of its technology and decoration.1 This is no art-historical study but an anthropologically-informed approach which seeks to put British Celtic art into wider social contexts. It builds on a database of over 2500 pieces of decorative metalwork,2 in marked contrast to the hundred or so items which form the focus of most studies (p. 55 and appendix 1). This database has increased enormously in recent years through the systematic recording of metal-detected finds in England and Wales by the Portable Antiquities Scheme.3

Chapters 1 and 2 set out the basis of G&G’s approach, with summaries of current trends in British Iron Age archaeology and approaches to Celtic art. Their starting point is a concern with ontologies, which they define as the nature of being. How did Iron Age people see and create their world, how did they perceive artefacts and how did objects shape these perceptions? They ask not what did art mean but what did objects do? – how were they used and perceived, and how did this change over time? The study sits very much within the current mainstream of British Iron Age studies, which emphasise a contextual approach, considering objects in their setting. They are also very critical of concepts such as elites and hierarchies, which are seen as traditional outmoded interpretations based on a perceived shared ‘Celtic’ past and taken from classical sources and inappropriate analogy.4 Theirs is an Iron Age of dispersed communities, where objects played a key role in building and re-making relationships.

Chapter 3 considers the methodology and lays out basic data on contexts, dates and distributions in a useful series of charts and maps.5 G&G stress that Celtic art was a pan-British phenomenon, though this is only partly true. The overall distribution is broad, but northern Scotland and south-west England are virtual blanks; and when this is broken down by date, the distributions look a lot more regional, as G&G note (figs 3.11–12). There is a complex story of development in here.

G&G make some key points in these chapters. A critical one is their comparison with Bronze Age finds. Bronze Age material culture was based on quantity (repeated, essentially similar material, such as hoards of axes), whereas the later Iron Age was based on quality – fewer, more individual items. This contrast is reflected in the styles: typology works well in the Bronze Age but badly in the

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2 Available online at: http://www.britishmuseum.org/research/research_projects/project_archive/technologies_of_enchantment/the_celtic_art_database.aspx
3 www.finds.org.uk
5 There is the occasional error, and future researchers should check the data before building on the detail.

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Iron Age because, they argue, styles accumulated rather than replacing one another, with elements of old styles surviving alongside new ones. This makes typological dating very difficult. Their solution is to look closely at the associations and contexts of decorated objects in order to build a chronology, supported wherever possible by new radiocarbon dates, the details of which are published elsewhere. This used Bayesian statistical analysis to assess several hypotheses on the development of Celtic art, from which they argued a most likely scenario — although other options were feasible. Here their preferred option is adopted largely without further comment: an early style of art (rarer, highly individual, and typically asymmetrical and complex), and a later style (more symmetrical, less complex, and produced in greater numbers), with a chronological gap between the two, c. 20 BC – AD 40. The broad division makes a lot of sense and ties into other evidence of changing societies in the late pre-Roman Iron Age leading to larger-scale power structures, especially in south-east England. But the late Iron Age gap they suggest is far from certain. The data in their 2010 paper could also support a continuous sequence with no break, and dates for really detailed modelling are still sparse. Many of the finds one would instinctively put into this period (such as decorated mirrors) are not independently dated, and the model may reflect an absence of evidence or changing habits of deposition rather than a genuine gap. Other kinds of art also developed over this time, notably coinage, while there were also regional changes in where such art was being developed, away from south-east England and more widely across the country. It is a topic which requires more evidence – but in presenting a theory, they have provided something to test against.

An important point to emerge from the dating audit is how much Celtic art was a phenomenon of the Roman period. The outline of this has been clear for some time, but they show how extensive this was – there is far more Celtic art from Roman Britain (especially in the first and second centuries) than from the Iron Age.

The remaining chapters adopt a life-cycle approach, from manufacture (chapter 4) through the lives of particular objects (chapter 5) to deposition (chapters 6–8). In each case G&G present a series of worked examples, carefully chosen to bring out key points in the argument and to present significant objects and contexts.

Chapter 4 includes stimulating observations on the nature of metal, developing an earlier discussion (pp. 14–20). In particular, they consider the relationship between iron and copper alloy, as part of their concern with reconstructing ontologies and avoiding modern concepts. Were iron and copper alloy seen as different metals, or was there more significance to different alloys, or to wrought versus cast metal? Here as elsewhere in the book, the idea is nice but the data are uncooperative; G&G try hard to find alternative patterns, but these are not entirely convincing. They make a good case that different depositional contexts

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2 Garrow et al. 2010 (note 4).
were preferred for cast objects (dry) versus sheet objects (wet), although this is rather generalised and would need closer regional analysis to be sure these patterns were genuinely widespread. But was it really the technology which led to this decision, or was it the nature of the object? For instance, if cauldrons were selected for deposition in water then this would create such a pattern, as cauldrons can only be made from sheet metal. One suspects it was the object rather than its technology which led to selection for deposition.

The discussion of manufacturing is one of the weakest areas of the book. This was not a focus of the project, and they largely synthesise published work. But there has been no major published survey since the 1980s, which focussed strongly on small areas of southern England. The patterns that they rely on are thus not convincing. It is also hard to tie metal-working debris into Celtic art unless moulds survive – how can one be sure that decorated material was being manufactured rather than brooches, fittings or rivets?

Much more useful and original is their discussion of the effects of different decorative styles (pp 100–7). They analyse the complexity of different motifs – a subjective area, but one which needs to be addressed – and offer valuable insights into processes of perceiving such complex objects. This stresses how a viewer’s understanding changes as their eye takes in different aspects of a complex surface. Tentative conclusions tend to become harder as the narrative progresses – the concluding discussion (pp. 107–111) is firmer in its views than the preceding analysis – but these approaches show considerable potential for further work on how art was perceived.

Chapter 5 takes three key artefact types – torcs, swords, and coins – and considers their different histories. They raise numerous interesting points – arguing, for instance, that swords were often long-lived, carrying a history with them. The discussion of coins stresses how different they were in their designs, regional distributions and clear Continental links from other decorated material. They were also more common, and G&G suggest they created new links between different people rather than reinforcing the existing (more exclusive?) networks where other forms of art operated.

Chapters 6–8 look at the different contexts of deposition for decorated objects: hoards, burials, and settlements. Each contains well-written and interesting arguments and perspectives. Chapter 6 emphasises how much of the material comes from late-phase hoards, and how much more diverse these later hoards were than the earlier ones: quantity was becoming a dominant feature over quality once more. But some of the aspects they consider cry out for more regional studies rather than a national overview – for instance, in the landscape setting, which is known to be regionally varied.

G&G range over different scales, from nationwide to a close focus on particular hoards or burials. These detailed studies are some of the most stimulating

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parts of the book. Here they consider what the objects did and who was involved in bringing them together. In this they look for relations between communities rather than elite individuals. They note subtle variations in a hoard of horse and chariot harness from Polden Hills (Somerset), for instance, and suggest this relates to several different groups bringing the material together. This is convincing for large hoards – but they argue the same approach should be used with burials, seeing them as an accumulation of material from different groups or symbolising different connections. For instance, they argue that the warrior from Deal (Kent) was buried with an accumulation of material, some of which may have been old when buried, and suggest this represents a community deliberately creating a ‘founding figure’ or ancestor in death. The emphasis here is on the maintenance and building of relations between dispersed groups rather than with individual or elite status, though they do consider the latter in their study of the late Iron Age burial from Baldock (Hertfordshire). But there is a problem here. The arguments are stimulating and informative, but they are very much set within current models of Iron Age society which seek to find small-scale Iron Age communities until the disruptiveness of the late Iron Age in south-east England – so rich burials like (late Iron Age) Baldock are inevitably interpreted differently from earlier rich burials because of wider social models. Yet there are other ways to view rich burials. The work of Diepeveen-Jansen on early La Tène burials in France and Germany¹ may have retained the terminology of elites, but her elites were small-scale and unstable – essentially small family groups contesting status in wider, rather ill-formed communities, where individual prowess could earn respect and allow access to special, unusual items decorated with this special, unusual art. This seems to this reviewer a better model for early Celtic art in Britain than the community-focussed version of G&G.

Chapter 8, on settlements, contains new insights into supposedly well-known material. Two of the three case studies are in Wessex, in southern England: the deliberate burial of moulds and crucibles at Gussage All Saints in a pit prominently placed near the entrance, and the burning of a chariot at Bury Hill. These are seen as events which marked each site’s claim to local fame. Their third study considers Celtic-style material from the Roman fort of Newstead in the Scottish Borders. They emphasise how this ‘Celtic art’ was part of lives on the Roman frontier, and was found both inside and outside the fort – not ‘soldier and civilian’, but something which touched the lives of many different people in frontier society. The study of this continuing role for Celtic art in Roman Britain is a key area for further work.²

The concluding chapter draws the story together, reiterating their view that Celtic art was based in relationships between communities: art-objects shaped and modified relations between different groups in an unstable, non-hierarchical

world. The move away from the uncritical assumptions of elites and hierarchies is very welcome, but this much more communal focus downplays the role of differences between individuals – for instance in access to or command of resources such as material, knowledge or connections. Even if these were fleeting, existing in the being and efforts of one person rather than persisting in the family or group, they were significant in the lives of wearers and viewers. Celtic art would have made these differences in knowledge and connections clear, because it was difficult to understand and because it showed wider links. These are both areas which G&G consciously avoid – the Continental links of Celtic art, and the ‘meaning’ of art. During the early phases of Celtic art (5th–3rd centuries BC), British styles show close connections to the Continent, and this knowledge of a wider world must have been significant for certain groups or individuals. The meaning of the art must also have been important. This was not just decoration: it carried messages, quite plausibly with a religious role,1 again marking wider connections across Europe. Here G&G rather avoid their title – if these objects are «technologies of enchantment», then where did the enchantment lie? This surely was a restricted, not a community art – complex and specialised, restricted to certain kinds of objects, and different from the ‘everyday’ decoration found on pottery, glass jewellery or bone objects. Knowledge of its connections and meanings must surely have been used by some people to indicate a difference from others.

In these debates lie the seeds for future work. This is a highly engaging, highly important book which is full of interesting views and avenues for further work. It is a genuinely archaeological view of Celtic art – embedding this decorative material in a social context, and forcing archaeologists to pay attention to it. It is distressingly expensive, though, as Oxford University Press books tend to be – one must hope for a paperback edition to bring it into the hands of the students who need to be reading it. It should also be required reading for anyone working with Celtic art far beyond the bounds of Britain; and for classicists, it should give a new view of ‘barbarians’ and a fresh insight into the ways local art styles were used within the Roman province of Britannia.

Edinburgh

Fraser Hunter

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Il libro di Elisabeth Marlowe ‘Shaky ground. Context, Connoisseurship and the History of Roman Art’, pubblicato da Bloomsbury nel 2013, affronta, con competenza e solide basi epistemologiche, uno degli argomenti più spinosi delle discipline antichistiche, quello cioè della modalità con cui scrivere (o riscrivere) la storia dell’arte classica. Infatti, mentre, come lucidamente affermava uno scettico come Ph. Bruneau intorno alla metà degli anni ’70 (‘Situation méthodologique de

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