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<th><strong>Title</strong></th>
<th>Response to Cassidy et al., 'A dynastic elite in monumental Neolithic society'</th>
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<td><strong>Authors(s)</strong></td>
<td>Flechner, Roy</td>
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As a historian I am unable to comment on the science behind this article, so I will take it as given that the scientific analysis is sound and that it supports the Research Team's interesting conclusions, for which congratulations are due. However, insofar as the historical and social-scientific interpretations are concerned, the article leaves much to be desired.

To begin with, the Research Team itself draws attention to the problem of interpreting archaeological evidence by means of a document written thousands of years later, a document which makes no claims for objectivity. Since the publication of the article this problem has been addressed in various on-line forums, so I will not labour the point here.

I wish rather to raise another methodological issue, in connection with the hazards of researching outside one's discipline without having the necessary expertise for it and without collaborating with others who do.

The article proposes that incest has only ever been considered socially accepted among polygamous elite groups, which treated heads of royal families as 'god-kings', and that the prerogative of incest was a means of asserting these groups' distinctiveness and superiority. There are indeed convincing attestations of what is sometimes referred to by the jargon as 'royal incest', and the Research Team mentions ancient Egypt, Hawaii, and the Inca empire as cases in point; but the evidence for this practice, its contexts and functions is much more debatable than the clear-cut way in which the Research Team presents it. The scientific literature that the Research Team cites on this point is poorly chosen and self-contradictory: the earliest article is by Goggin and Sturtevant, from 1964. Sturtevant was engaged in a bitter debate with the authors of another article quoted, by van den Berghe and Mesher: he refuted aspects of their work, and they of his. The article by van den Berghe and Mesher (1981) is in fact not even an article but a brief acrimonious response to critical comments made by Sturtevant on an article of theirs from 1980. The Research Team cites another article (by Gates, 2005), which offers no independent data but is merely a derivative citing Goggin and Sturtevant. It is quoted from a collection of articles, one of which (by Scheidel) argues against the notion that incest was confined to elites in Egypt, demonstrating instead that 'many couples in Roman Egypt arranged marriages between their own children'. This article contradicts the Research Team's premises and is not cited (perhaps the Research Team didn't read it). Strangely, the Research Team references another article, by Huebner (2007), which also makes the point about wide-spread brother-sister marriage in
ancient Egypt. But the team references it in support only of 'royal or dynastic incest'. (The only unproblematic references that I could find relate to articles concerning Hawaii.)

Ultimately, therefore, the Research Team's propositions on elite incest go back to a database constructed by Goggin and Sturtevant in 1964, and to a database by van den Bergh and Mesher from 1980 (which is not explicitly acknowledged in the article by Cassidy et al., but is referenced in van den Bergh and Mesher's publication from 1981). There is no indication that the Research Team investigated how secure these old databases are or how these databases are received by present-day anthropologists. Nor is there any engagement with the question of the extent to which these databases—which draw primarily on African, South American, North American, and Pacific examples—shed light on a prehistoric society from the westernmost fringes of Europe.

These inauspicious premises do not stop the Research Team from drawing far-reaching conclusions about the role of incest among elites in prehistoric Ireland. All this, we must remind ourselves, is based on the remains of a single individual found in a passage tomb, about whom and about which we know next to nothing. The site of the remains (i.e. the passage tomb) leads the Research Team to make bold analogies with other places in which passage tombs are found: 'We propose that a comparable set of social dynamics was in operation in Ireland by the Middle Neolithic, and—given that solstice-aligned passage tombs similar to Newgrange were constructed in Wales, Orkney and Brittany—may have occurred outside the island as well'. This is a false syllogism. It is the equivalent of saying: 'some cats are white, some refrigerators are white, therefore some cats are refrigerators'. To top it all, I quote the following circular argument: 'The Brú na Bóinne passage tombs appear in Medieval mythology that relates their construction to magical manipulations of the solar cycle by a tribe of gods, which has led to unresolved speculation about the durability of oral traditions across millennia. Although such longevity seems unlikely, our results strongly resonate with mythology that was first recorded in the eleventh century AD, in which a builder-king restarts the daily solar cycle by copulating with his sister'. Either the Research Team's findings reinforce the legendary narrative (from the eleventh-century Metrical Dindshenchas) or vice versa. You can't have it both ways. Had the Research Team read van den Bergh and Mesher's 1980 article, it would have learned about the method by which they constructed their database, which explicitly excluded 'legends or myths of incest' (p. 305) because they are not reliable.

It is possible that once the Research Team addresses the methodological concerns that I raised then the article's conclusions regarding incest will still stand. But the Research Team will have to work hard, very hard, to get there,
and it will have to do this by engaging closely with a wealth of pertinent literature across disciplines, preferably by collaborating with historians, anthropologists, and specialists in medieval Irish literature. This interdisciplinary collaboration should really have taken place before publication, and it is something that the editors of a prestigious journal ought to have directed the authors to do. In the present-day academic climate interdisciplinary studies are common, and one would expect the editors to assign an article to peer-reviewers from as many disciplines as the article covers. Only good will come of it.

**BIBLIOGRAPHY**


