

Reply to Pierre Moret

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I would like to thank Pierre Moret for his praises —which I certainly do not deserve— as well as for his valuable comments and criticisms. An intellectual discussion with such a prominent scholar is a satisfying experience, even if I do not wholly agree with his opinions. His reactions are related to three different topics: the definition of the geographical area covered in my article; the role of population increase in social evolution and the role of migrations. I will group my responses under these three headings:

a) The definition of the area

I think the reporter is right when he points out the ambiguities regarding this issue. This is a charge that I can only plead guilty to, but I also want to plead mitigating circumstances. Establishing the geographical limits in this kind of studies is not usually straightforward. P. Moret suggests that, given the problems in ascertaining the limits of protohistoric territories, «natural» regions should be the basic criterion («...n'importe laquelle de ces combinaisons est légitime, pourvu qu'elle s'appuie sur les articulations et sur les limites d'une région naturelle»). To this I object: «natural» regions cannot be easily defined and, even if they could, this view seems grounded on some sort of geographical determinism, an intellectual position that I do not share. I understand that the reporter thinks that either I should not have included the Catalan area of the Ebro valley in the article, or, conversely, that I should have studied this entire «natural region». In my opinion, this is wide of the mark: the inclusion of the Lleida (Lérida) region has made it possible to argue that similar processes took place in different areas and at different times due to similar underlying causes. I think this is a relevant issue, while taking into account the entire Ebro river valley was obviously well beyond my scope. Moreover, if we take Moret's position to its ultimate con-

sequence, we can also point out that there is no neat geographical (nor cultural or linguistic) limit between the far southern Catalan territories and Northern Valencia. Should I also have included the Maestrat? Why not the Plana de Castelló? Certainly, natural regions are not so straightforwardly defined, nor might they be easily equated with archaeological, cultural or historical areas. More explicitly, P. Moret states that he would have preferred that I had taken into account the whole area covered by my figure 1. I maintain that, from either a geographical, cultural, linguistic or historical point of view, this is not a more consistently defined region. Why, then, «Catalonia», instead of the «*Nord-Est ibérique*»? Because, although «Catalonia» is not an univocal concept from either historical, or cultural, or geographical points of view. I honestly think that it is not less precise than the alternative suggested by the reporter.

That being said, I believe P. Moret is right when he points out the archaeological and historical relevance of the lower Aragon area. I am not reluctant, therefore, to admit that my contribution would have been better founded if I had also taken into account the evidence that he himself and other scholars have been bringing to light in that region during the last few years. However, I think I should say the same about the North of Valencia and, since I do not believe that the Pyrenees constitute any kind of a natural or cultural border, the Rosselló (Roussillon) and Western Languedoc. Relevant information has been gathered lately in both areas, but, again, boundaries must be drawn somewhere...

b) The role of population increase

Since it has to do with the core of the explanation that I propose for social change, this matter is more important, and deserves a lengthy comment. Firstly, I must say that my position is consistent with cultural materialism and with the model of social evolution that has been developed by A. W. Johnson and T. Earle (2000²).

As I understand his statement, Moret claims that population increase may not be considered as the «prime-mover» of social evolution, since it is in itself caused by other forces, like technological advance, migration, shifts in personal statuses and changes in reproductive patterns, among other unspecified ones. Given that I do not dispute the possibility that population increase could be caused by migration, I think it is worthless to discuss this issue now. As for shifts in personal status, it seems clear to me that they are the consequence of social change (or they go together with it), not its cause. The same holds true for changes in reproductive patterns (usually in the restriction of births, not in the encouragement of population growth). Consequently, I will (briefly) concentrate on the crucial question of whether population increase or technological change drives social evolution. Assuredly, this is to some extent a chicken-and-egg debate, since both possibilities are ever present. I am quite sure that the introduction of technological improvements *can* trigger demographical expansion. That being said, I also think that, *as a rule*, populations grow until the carrying capacity of the environment is reached, and then try to find new solutions to scarcity. These generally involve technological improvement, which is closely related to the expansion of the political economy. If the existing technology does not provide

a base on which to build new productivity, social change will not occur and population will drop. In other words, some knowledge of a given technology is necessary to develop new means of production that are capable of coping with population growth. But, as Kim has put it, «the spread of technology should not be confused with the spread of knowledge or an idea» (Kim, 2001, 444). Since the initial costs and risks of the adoption of a new technology are usually high, there have to be powerful reasons to start that process. Assuredly, the increase of the carrying capacity *when population has reached the environmental limits* is quite a serious one.

This is probably what happened in Catalonia (and other neighboring areas) during the first half of the 6th century BC. Iron metallurgy had been known in that area for at least one century (Junyent, 1992; Pons, 1982-1983; Pons, 1984), but had not caused any improvement in subsistence technology. Before the 6th century BC, iron was used on a small scale, mainly for prestige items (Ruiz Zapatero, 1985, 852), not to for the making of tools that would have been more efficient and might have improved productivity. In spite of this, the archaeological data strongly suggest that population did grow all through the 7th century BC. It is a reasonable assumption that it reached the environmental limits by the early 6th century BC. It was then, and only then, that new solutions were envisaged within the existing technological knowledge. It is also worthwhile to remember that demographic growth preceding technological innovation has also been reported in such different regions as Denmark and Southern Korea (Kim, 2001).

The reporter's final remark on this issue also deserves a brief commentary. He states that « *l'époque préibérique est encore trop mal connue* », but my aim was not « *trancher entre une multitude de modèles, tous également plausibles* », since I do not think this «multitude» really exists (unless Moret meant «a multitude of possible models», that have still not been built). I have simply tried to use a theoretical model (Johnson and Earle's), which I think is well grounded in the currently available ethnographical and archaeological data and that could, and should be empirically tested. With this I mean, not only that it may be contrasted with the currently available information (which is what I have tried to do), but also that it can drive the collection of new, relevant data. This in turn might prove that the model is flawed or, conversely, that it is still plausible (but, obviously, not that it is necessarily true). I think this is the only way we can advance in this field, instead of waiting for research led in a theoretical vacuum which will illuminate our minds with information that will «speak for itself».

Now, since I said that hypothetical models must be as soundly founded as possible on objective data, I am also bound to emphasize that, in my opinion, it is a mistake to liken endogenous population growth and migration (or invasion) as possible explanations for population growth in Catalonia throughout the 7th century BC. This is so because, while we have positive evidence of the former, there is none for the latter. To say that talking about migration is no longer a taboo (« *la question des migrations n'est plus tabou* ») — a position with which I completely agree — does not add any logical rigor to the reporter's intellectual procedure. Migration might be an important factor of social change, but I think it is

pointless to raise this issue when we do not have the least evidence, nor even any indication, that it actually occurred.

c) The (possible) role of migrations

I do not have much to add to the reporter's remarks on this, as he says, still «delicate» matter. I wholeheartedly agree with most of his theoretical position and I only insist on the fact that migration is a «delicate» issue because a kind of intellectual tyranny has been imposed on archaeological research. That being said, the comments of Pierre Moret reveal some misunderstandings of which I am probably solely responsible.

Firstly, my article was intended to show that a given model of social evolution could efficiently account for the available data on the history of the Catalan area during the first millennium BC. Why then did I also write about the possibility of migrations from the Southeast of the Iberian Peninsula? Because a prominent scholar, J. de Hoz, had suggested —on what seemed to me solid ground— that the Iberian language had expanded northwards from that area (De Hoz, 1993). But then he put forward an explanation for this linguistic expansion (basically, trade) that I did not consider plausible (De Hoz, 1993; De Hoz, 1994). Consequently, I proposed an alternative hypothesis —population movement during the 6th century BC— and tried to find out from the available data some evidence that might support it. I think this is an intellectually correct procedure, since the fact that I firmly believed —and still believe— that endogenous evolution accounts for social change during the 6th century BC *must not* prevent the search for alternative or complementary explanations *if* these are required by the available data. The problem with that issue is that, as P. Moret rightly stresses, the available archaeological information for that period is frustratingly scarce (mostly due to the continual occupation of sites during the Middle and Late Iberian Periods). Even so, there is some evidence of cultural discontinuity:

a) In the funerary record, since most of the Early Iron Age necropolises do not continue into the Early Iberian Period. In other words, most Early Iberian cemeteries in Northern Iberia do not seem to have any link with earlier ones, and could belong to an entirely new population. I think Moret did not follow this reasoning fully – since he seems to have understood that I was basing my argument on a discontinuity between the Early and Middle Iberian Period. But, as I have already said, this is probably due to my inability to express myself clearly.

b) The extremely quick expansion of the typical Iberian pottery. P. Moret argues that this may be simply due to the success of a new technology. In reply to this, I must state that it is not only the way that the pots were made that changed, but also the shapes and decoration, which were extremely similar all over the Mediterranean coast at this time, from the Southeastern Iberian Peninsula to Western Languedoc. In other words, it was a whole ceramic tradition that expanded, and to a large extent substituted the previously existing one, in a very large area and in a very short time.

We cannot preclude the possibility that other disruptions may be observed in different facets of the archaeological record, but as already said, the available data for this period are still inadequate. That being said, again, I would like to stress that I do not consider a possible population movement as an exclusive alternative to social evolution. Both processes may have taken place.

As for the Gaulish cultural traits that are attested to in Catalonia during the Middle Iberian Period, I think they are good indicators of the close contact which existed between the Protohistoric populations of Northern Iberia and Southern Gaul, of which many other historical examples could be easily mentioned, even after the creation of the artificial borders of the so-called «national» states. Nevertheless, I am not sure whether they can be taken as indicators of ethnicity. In my view, there are not clear-cut «templates» that might define what is and what is not specifically Iberian (or, for that matter, Gaulish) in the material record. Rather, as T. H. Champion has put it, «if we examine the distributions of individual types of archaeological material, especially if we use quantitative rather than mere presence-absence information, we find not neatly bounded entities but an enormous variety of cross-cutting patterns». Since there is no cultural disruption in Northern Iberia during the IV-III centuries BC, this is exactly what I think the cultural traits mentioned by P. Moret are indicative of.

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