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Dear Luca,

I have tried to answer the reviewer's comments in detail. I would suspect that he is probably not a prehistorian from the lack of detail in the comments but I may be wrong. It is rather surprising how much he misses the point of what we are trying to do. For one trained in the British "historical" school of prehistory, it is hard not to smile at the emphasis on history by someone who obviously does not know the actual history or in this case prehistory. You should look carefully at number 4 in the enclosed comments. I may not have put this in the best terms. The lines of argument here may well be worth developing in some future work, since little critical work has been applied to this traditional idea and it definitely ties in with the rate question. This is one of the topics I had originally thought of including in paper No. II.

I think we will need some travel funds in the grant but this would probably not be needed at the start, except hopefully in getting to Stanford. My idea would be to try to define the specific problems we want to deal with and the data needed over the first year. I have friends in England who can send xerox copies of the written sources we may need. But at a certain point it is much better to see the material and people directly. When I go to England in May, I will try to see someone (a friend of a friend) who has been working on the neolithic-mesolithic transition in Holland and western Germany and may have finished his thesis. I will also be seeing some friends in Holland on my way back. This is the area that probably offers the best evidence in terms of quality and cooperation. I have also discovered (this probably belongs in quotation marks) that Italy may offer one of the best opportunities for studying the relation between late mesolithics and earliest neolithics. The U-shaped glacial river valleys such as the Adige near Trento offer unusual stratigraphic possibilities. The scree from the high vertical walls builds up at a fast enough rate that cultural layers represent short time intervals. At a good location for occupation, one can get with a little luck a whole series of separate layers (say 3 to 5 layers per 500 years). I know an Italian in the Museum at Trento who is digging such a site with both neolithic and mesolithic layers. Unfortunately, the digging is a little rough. It was only a few weeks ago when I was working on my thesis that I suddenly realized the potential of these sites situated at the base of the walls. It would be amazing if one of our best long term sources ended up being in Italy. I know you have your eyes on North Africa.

It sounds as if you have more than enough things on your hands for one spring. With best wishes.

*yours,
Albert*

In his comments, the reviewer does not get down to the content of the paper. Many of his criticisms (and the straw men he tries to bring in such as "the typological notion of the Neolithic") can be answered by direct reference to the paper itself. It is of interest that no specific questions or objections are raised concerning (1) the assumptions that the model is based on; (2) the data used; and (3) the methods and results of the analysis itself. All these things are side-stepped by claiming that what is called for here is a historical view. It should be fairly obvious that the model and the analysis are not incompatible with "historical" interpretations of the neolithic in Europe.

1. "It obscures the events of history by trying to smooth them, using a pseudo-scientific model." There are a fair number of responsible people who would question the use of "pseudo" in reference to the model. The aim of the analysis was not to "smooth out" history or prehistory, but rather to see whether or not a patterning or regularity existed in the available data on the early neolithic in Europe. The observed regularity -- the high correlation coefficients for estimates of the overall rate of spread -- will, no doubt, strike many prehistorians as "surprising". This is something that has not been shown before. However, the view that the authors are trying to force the neolithic into a formula by means of a statistical conjuring trick is totally unwarranted. In a paper to follow, an attempt has been made to estimate several regional or local rates which can be compared with the overall rate. The point that the authors expect some variation in regional rates is made on page 8 of the present draft.

2. Origins of domestication. The most up-to-date account of the evidence and ideas on various domestication topics for the Old World is to be found in the proceedings of the 1968 London

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conference, which I attended. It is obvious that we have not fallen into the "at-one-time-in-one-place fallacy". Why then is the analysis run for four possible centers? Moreover, these are not taken as centers of domestication in the paper but are used as centers of diffusion from which the spread of relevant domesticates takes place. The four sites selected have the virtue that relevant domesticates are documented at them at dates earlier than their appearance in Europe. The reviewer mentions "various plants, grains, animals". In Europe, we are dealing only with three domesticated animals (Bos, pig and sheep/goat) and two grains (wheat and barley), as mentioned in the paper. These are the relevant domesticates. Except for the dog, there are no serious claims for other domesticated species being found among the remains of early neolithic cultures in Europe. The two grains are perhaps the most instructive. Domesticated forms of wheat and barley can readily be distinguished on morphological grounds from non-domesticated forms. The genetic "history" of wheat is quite well known due largely to the work of Riley. Almost no one holds the position that these grains were domesticated in Europe (either once or repeatedly). There is no secure palaeobotanical evidence for the domestication of these two grains in North Africa. The place where there is good evidence is in the Near East at sites such as Ali Kosh, Beidha and Jericho, which all have C 14 dates that are earlier than the radiocarbon dates at early neolithic sites in Europe. The present paper is not concerned with trying to determine where or when or over how long a period the actual domestication of various species took place. What we are concerned with is how -- both in terms of rate and mode -- wheat and barley get across Europe, given that they are not domesticated in Europe and knowing that they have been documented in the Near East. The movement of these grains across Europe can only be "explained" by some kind of diffusion process.

3a. "The spread was not a single process, but depended on different crops and animals." In passing, it may be worth mentioning that more than one diffusion process is considered

in the paper. What are these "different crops and animals"? Does one get certain domesticates in certain parts of Europe and not in others? Again, we are dealing with the five domesticates listed above. It is these five elements that add the new dimension to neolithic economies in Europe. With the exception of the dog, one does not get other domesticated species. There is no evidence for a differential spread of the five unless one is talking about the circumpolar region or high mountain districts. One does not get a barley-pig early neolithic in one part of Europe and a wheat-Bos early neolithic in another. There is no region where one of the five gets completely blocked out. Where faunal and floral remains have been examined closely, one usually gets several, if not all, of the domesticates occurring together. A point that may be worth making here is that the model (and analysis) does not set a requirement that neolithic economies are all of a uniform composition. As one would expect, the actual composition of an economy varies from site to site but with the five domesticates representing a regularly recurring common denominator.

3b. "The dates in southern France are older than three of the four dates from Italy." The two high dates from southern France are much less reliable than the four Italian dates for the following reason. The Italian sites are all open settlement sites where only neolithic layers are present. Cave sites in Italy such as Grotta della Madonna, Arene Candide and Arma di Nasino all have both neolithic and mesolithic layers. These cave sites were kept out of the analysis because of suspected contamination (definitely the case at Grotta della Madonna), which was one of the criteria for selecting the sample. Direct knowledge of unpublished material in Italy made this discrimination possible. The two French sites are both cave sites. Direct knowledge here might have led to one or both being deleted from the sample, although this would be premature without such knowledge. Both French sites belong in the suspect category. In addition, both have high standard errors of 200 years. More dates are needed in France, especially at open sites, before one wants to start making too many claims here. Most prehistorians see an Italy-to-France trend of development in material culture during the early

neolithic and not the reverse.

4. Environmental adaptations. This is one of the traditional ways of looking at the question of the spread of the neolithic in Europe. To start with, it should be mentioned that, while the environment can influence the rate of spread by offering greater or lesser resistance, it cannot account for how the neolithic actually moves forward. This approach (environmental adaptation) is far from a secure one, until we get to places like Scandinavia in Europe. The following three points may help explain this statement. Suppose we are considering a spread from northern Greece to the interior of the Balkans and Central Europe. (1) We start by looking at specific environments. What is the extent of difference between the soils and climate at say Polykastren on the Axios/Vardar in Greek Macedonia and at Sophia in Bulgaria or at Belgrad in Yugoslavia? On the next leg, what is the degree of difference between conditions at Belgrad and Munich in southern Germany? There are, of course, differences in all these cases but they are far from monumental ones as we are often encouraged to believe. (2) This is particularly true in the context of the climate at the time. What is the climate actually like at c. 7,000 B.P? This is the start of the Atlantic phase of the Post-glacial pollen sequence in Europe, when average temperatures are supposed to be 2° C warmer than they are at present (see for example, R. G. West, Pleistocene Geology and Biology, 1968). The climate of the Balkans drops its guard just at the time that the spread into this area is taking place. One could almost argue here that for a species to stay in the same climate at this time, it has to move north. (3) What is the potential for biological adaptation or modification on the part of the domesticates we are concerned with? This is a question that has usually not been asked. The climatic and environmental tolerances of the three species of animals are more than enough for the journey to Central Europe. The two grains are more sensitive. But on the positive side, they reproduce each year. A span of 100 years gives domesticated strains of wheat and barley 100 generations

to undergo needed minor modifications through genetic processes and selection. Modern genetics may be able to offer the prehistorian some help here. This is after all a genetics problem to a considerable extent. Considering the rather modest adaptations required by environmental differences in relation to the number of generations involved, the potential for biological adaptation on the part of domesticated strains of wheat and barley may well exceed the observed rate of spread. The hold up, if there is one, is probably not on the biological-environmental side.

5. Scandinavia. There is general agreement among prehistorians that environmental factors play a major role in Scandinavia. The analysis of local rates, as well as the lines in Figure 2, indicate that something special is happening here. By c. 5,500 B.P., most of Europe below 54° N Lat. is covered with neolithic cultures. Scandinavia is the northern European terminus of the spread. Scandinavia represents a rather special case. This is clearly revealed by the Ertebølle culture which prevails in the area at c. 5,500 B.P. It is not our aim to gloss over the situation in Scandinavia, which is brought out by the analysis itself. At the same time, the situation here does not undermine the validity of the analysis.

6-8. Starting dates. In paragraph 6, one can only ask where the starting date of 7,000 B.P. in Greece comes from. The five Greek dates listed in Figure 2 are all higher than 7,500 B.P., including the date for Corfu in north-west Greece. The "historic" date at which the neolithic arrived in southern Italy is, no doubt, prior to the Scaramella date of 7,000 B.P.; the standard error alone allows a date of 7,100 B.P. We have, as always with C 14 dates, only approximations. The more determinations and the more sites with dates the better the approximations become. In the next paragraph, the reviewer claims that the "spread north started earlier". Is there good evidence for this? The available evidence indicates that the spread to Italy and the interior of

The Balkans is taking place at essentially the same time. It is probably a mistake to start talking about starting times for various spreads. The C 14 dates for a site can be viewed much more usefully as arrival times (or marking the date of passage of the wave front), since they date occupation and the earliest occupation layer at a site was used whenever dates were available for it. One can only use the time interval differences between sites with due caution, for obvious reasons (the standard errors). This is something we have fortunately been able to avoid in the analysis shown in Figure 1. The reviewer does not seem to have perceived this advance in methodology.