

## Returning to the the Neolithic transition in Europe

*In the early 1970s we started the collaboration between archaeology and human genetics. Much of the initial effort, by L. Cavalli-Sforza and myself, was spent in trying to bridge the differences in our backgrounds. In this way, for the first time, the wave of advance model brought local population growth, site relocation and the rate of spread of early farming all together in a quantitative framework. Now, moving beyond indigenism, the idea of demic diffusion is widely accepted and its wider implications are reflected in the study of the Neolithic transition in Europe.*

**Key word:** Neolithic transition. Genetics. Demic diffusion. Indigenism.

The spread of early farming in Europe has become in recent years a subject of broad interest to a number of disciplines, ranging from prehistory and anthropology to human genetics and linguistics. Today it is well known, for example, that wheat and barley, the cereals which were cultivated by early farmers in Europe, were originally domesticated in the Middle East. This happened some 10000 years ago. From there, early agriculture spread first to Greece and the Balkans and then over the rest of Europe. We have called this shift to food production as a new way of life the Neolithic transition (Ammerman and Cavalli-Sforza, 1984). Previously, the prehistorian V. Gordon Childe had used a different term, the Neolithic revolution, to identify this transformation. Within Europe, it is now well established that it took more than 2500 years -that is, more than 100 human generations- for early farming to spread from Greece to Scandinavia. In short, it comprised a long, slow process and not a revolution in the normal sense of the term. Transition would thus seem to be a more appropriate name, in the case of Europe, for the kinds of changes that were involved.

In returning to the question of the Neolithic transition in Europe, there are three things that I would like to do in

my paper for the Valencia Symposium. The first is to look back on how the collaboration between Luca Cavalli-Sforza and myself began some 30 years ago. The second is to review how this work developed over the years. And the third is to reflect upon some of its wider implications with regard to the approach that we take to the study of the Neolithic period today. It is worth adding that no attempt will be made to deal with the genetic side of the argument here -a field that is, in fact, growing very rapidly as more new work is done each year on mitochondrial DNA and on the Y chromosome (Semino *et alii*, 2000). This is, in any event, something that is covered by the contribution of Jaume Bertranpetit to this symposium. Instead, my focus will be on the archaeological side of the matter. While there has been a rapid growth in the literature on the Neolithic transition in Europe over the last 25 years, it is not always easy to distinguish between short-term swings in fashion and more lasting gains in our understanding of the question of the spread of early farming in Europe.

Before going any further, I need to say a few words about what I have been doing since 1984 and the publication of our book on the Neolithic transition. To make a long story

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short, my research has gone off in new directions: Rome, Athens and Venice. However, as we shall see below, this new work has also encouraged me to look at the Neolithic transition from a new perspective. For the last 16 years, I have done studies in environmental archaeology on early Rome (the Forum, the Capitoline Hill, the Palatine Hill and so forth), on the Agora of ancient Athens, and on early Venice (Torcello and Piazza San Marco) and the origins of the famous city built on water. In short, I have been quite busy in the last few years. The common theme of all of these studies is the move from landscape to cityscape -that is, the purposeful, human transformation of the landscape in order to create an urbanscape.

In addition, in October of 1998, I was able to organize with Paolo Biagi a meeting held in Venice that was called *The Neolithic Transition in Europe: Looking Back, Looking Forward*. The aim of the conference was to examine how our thinking on the question and how the research strategies that we use have evolved over the course of the years (Ammerman and Biagi, 2002). Thus, the purpose of the Venice meeting was to do more than just review the current state of knowledge in the field; a conscious effort was made to pay more attention to the historical context in which we, as archaeologists, attempt to solve the problem of the Neolithic transition in Europe. In the rush to obtain an answer from the archaeological evidence that happens to be immediately at hand, there is a tendency to lose sight of the framework in which we work. The historical context in which the research itself is conducted does not always receive the full attention that it should.

The idea for the Venice meeting first came to me in the spring of 1996, when I was a senior fellow at the National Gallery of Art in Washington, D.C. At the time, I was working on a quite different problem, the many different attempts that have been made to reconstruct the ancient city of Rome since the time of the Renaissance. Over the last five centuries, the reconstructions of Rome have taken many different forms: from written accounts to paintings and maps; from ice sculptures and detailed scale models to the actual restoration of monuments themselves. One of the lessons to learn from tracing such a long and rich series of reconstructions was the value of paying closer attention to the historiography of an archaeological problem. Over time, what is observed in the case of ancient Rome is a restless dialogue between three different elements: the new physical remains that keep coming to light, the new interpretative keys that are brought forward to explain them, and the shifts in the wider cultural historical context that act to reshape the whole endeavor. And each of the three keeps on changing as the story unfolds. In other words, a reconstruction is not just a record of the archaeological evidence that happens to be available at a given time. All attempts at reconstructing the past have to be seen, in ad-

dition, as the expression of the wider cross-currents of intellectual fashion in their own time. What this suggested in the case of the Neolithic transition in Europe -still a comparatively young field of investigation- was the need to take a more historiographical approach to the study of the question. Thus, one of the primary goals of the Venice meeting was to explore this new approach.

The charge to the participants in the preparation of their papers for the Venice meeting was to ask each person to reflect upon how their own personal experience -their education and training, the fieldwork and their collaboration with others on projects, their publications and the debates that they have occasioned- all contributed to shaping the positions that the person held at one point or another in their life. In short, the participants were invited to trace the pathway of their own intellectual development with regard to the problem. By looking back and comparing a range of different experiences, there was the prospect of gaining a better understanding of where we stand in the field today and how we managed to reach this point. In my own contribution to the meeting (Ammerman, 2002), one of the things that I wanted to think more about was the issue of indigenism, which is at the heart of much of the recent debated over the Neolithic transition. Indigenism is the position in anthropology and archaeology that wishes to treat each society as independent and autonomous and to see its development essentially in endogenous terms. In returning again to the Neolithic transition in this paper, reference will be made to some of the views put forward by other speakers at Venice as well.

At this point, it worth recalling the three main lines of explanation that have been put forward for the start of early farming in Europe. One holds that the origins of agriculture took place *in situ* within Europe itself. In short, there was the local, primary domestication of wild forms of wheat and barley in different regions of Europe (Barker, 1985). Foragers, the argument goes, sometimes have a close relationship with wild plants and animals. They also possess the potential for innovation in the management and manipulation of such resources. Accordingly, local domestication becomes a working hypothesis. At the time of the renewed interest in Mesolithic studies in the 1960s and 1970s, this was a fashionable idea. In turn, various claims for local domestication were advanced in Europe. However, since the early 1980s and the advent of the new method of C-14 dating based upon accelerator mass spectrometry (AMS), few of the early claims have stood up to rigorous evaluation. The direct dating of individual cereal grains by the AMS method provides chronological control that was not available before. This is of particular importance in the case of cave sites where taphonomic processes often cause finds of small size to occur out of stratigraphic context (see, for example, Bernabeu *et alii*, 1999). As a result of the

introduction of AMS dating, few new claims for the local domestication of wheat and barley in Europe were made in the last ten years. And there has been a shift away from this hypothesis. Now even claims in the literature for local animal domestication in Europe - a separate but related issue examined by Peter Rowley-Conwy at the Venice meeting - are increasingly being called into question.

This leaves us with the alternative that early farming spread to Europe from the Near East, where the exploitation of wild forms of einkorn and emmer is well attested at early sites and where domesticated forms of these cereals have much earlier dates than they do in Europe. The best evidence for this comes from the work of Moore and Hillman at the site of Abu Huryea in Syria. There are now a fair number of AMS dates done directly on cereals remains from the site, including those that go back to the eleventh millennium before present. Moreover, when it comes to the DNA fingerprinting of einkorn wheat, there is now also evidence, as shown by Heun and co-workers (1997), for a site of domestication located in the Near East and for the dispersal of this crop plant from there. Both lines of evidence argue that one is dealing with the dispersal of domesticated forms of wheat and barley in the case of Europe.

In short, there are two main ways to account for a dispersal of this kind. One is the *cultural* mode of diffusion, the second main line of explanation for the spread of agriculture in Europe. It would see the cereals and farming techniques passing between local groups in different places without their geographic displacement. The third line of explanation is the one that we have called the *demic* mode of diffusion. Here the spread of early farming in Europe would be related to the movement of the farmers themselves. In the formulation of the cultural mode of diffusion proposed by Zvelebil (1986), emphasis is placed on essential continuity between the late Mesolithic and the early Neolithic in a given area. This would include the same settlement patterns and the same basic levels of local population density over the transition. In addition, the change in the subsistence strategy would start with a low reliance upon domesticated plants and animals in a given area. In contrast, the demic mode of diffusion would envision both a package of domesticates and a greater reliance on food production from the start in a given new area where the spread is taking place.

Turning now to the work that I did with Luca Cavalli-Sforza in greater detail, it will be recalled that our first article on the subject, *Measuring the rate of spread of early farming in Europe*, appeared in 1971. In 1973, we went on to develop a new formulation of the problem in A population model for the diffusion of early farming in Europe. In a book published in 1984, *The Neolithic Transition in Europe and the Genetics of Populations in Europe*, we put forward a synthesis of the various studies that we had done over

the years. It may be helpful at this point to review briefly three of the main elements in our formulation of the Neolithic transition. One of these is the concept of demic diffusion. As mentioned above, the spread of early farming in Europe can be seen as a process of diffusion which can be explained in two quite different ways. These need not be mutually exclusive, but they warrant being clearly distinguished at the conceptual level. One hypothesis, as we have just seen, is that of the cultural mode of diffusion: the movement of the cereals and farming methods with no geographic displacement of the local groups or populations. The other hypothesis is demic diffusion: that is, where movement in the diffusionary process takes place when the farmers relocate their settlements. In the specific case of the wave of advance model, as we shall see below, the spread may be due to the frequent relocation of early Neolithic settlements over short distances.

One of the problems that we soon encountered when we first began working on the question was the lack of a suitable terminology in the literature for a spread of early farming based upon frequent, local, small-scale relocations of this kind. Commonly used terms such as colonization and migration were not really appropriate for this. Colonization, in its conventional meaning, refers to the intentional settlement by a coherent group of people usually in a distant land. A familiar example of colonization would be the one practiced by the ancient Greeks in southern Italy. There was, in short, a poverty of language with regard to describing the full range of the ways in which the movement of early farmers might have taken place. Hence, we coined a new term: demic diffusion. In effect, colonization and the wave of advance model represent the opposite ends of a spectrum of different models belonging to the general class of the demic mode of diffusion.

The second element in our formulation is the wave of advance model itself. Here we drew upon the previous work of R.A. Fisher (1937) in genetics and J.G. Skellam (1951) in ecology. This now made it possible to model, in formal terms, the slow and continuous dispersal of a population in space. It can be shown mathematically that, if an increase in population numbers coincides with a modest local migratory activity - the short-distance settlement relocations mentioned above - a wave of population expansion will set in and progress outwards at a steady radial rate. This model, without going into the details of the mathematical treatment here, has the diffusional population wave of advance for its full name - or more simply the wave of advance. It is worth adding here that two researchers in Catalonia, Joaquim Fort and Vicenç Ménédez (1999), have recently refined the mathematical treatment of the migratory component in the formal model (through the use Einstein's approach to Fickian diffusion). This now makes it possible to handle in a better way the occurrence of time

delays in the diffusionary process (that is, the time that passes between site-relocation events). At the time that Cavalli-Sforza and I began working together in 1970, the use of formal models already had a long tradition in the fields of economics and the biological sciences. By contrast, models of the kind put forward by Fisher and Skellam had seldom been used before in archaeology. Thus, the proposal of such a formal model constituted something new in itself for the archaeologist at the time. However, as often happens in the case of an innovation, there is the risk of misunderstanding. Our own experience over the years indicates that the archaeologist does not always have a firm grasp of the distinction between a model, a hypothesis and a statement that is made as a claim to historical knowledge. This has been the source of some confusion in the literature on the Neolithic transition.

A third element of our formulation has to do with the interactions that populations of early farmers and late hunter-gatherers can have with one another in a frontier situation (that is, where both life styles may co-exist in the same area). In 1973 and again in 1984, we outlined some of the main ways in which early Neolithic and late Mesolithic populations may have interacted with one another: for instance, mutualism, acculturation, competition over resources and the transmission of disease (Ammerman and Cavalli-Sforza, 1973: 353; 1984: 116-118). In addition, we realized that different scenarios may have prevailed in different regions of Europe: that is, demic diffusion may have played the leading role in some regions, while cultural diffusion took the lead in others. As we wrote in 1984, the real question may well be to evaluate the relative importance of the two modes of diffusion in different parts of Europe (Ammerman and Cavalli-Sforza, 1984: 6 and 134-135).

How did this formulation come about? I first met Luca Cavalli-Sforza at a conference on quantitative archaeology held at Mamaia on the Black Sea in 1970. In due course, he invited me to give a seminar at the University of Pavia, where he taught before going to Stanford. At the time, he was doing a study of the Babinga pygmies in central Africa -amongst other things. We shared an interest in hunter-gatherers and we both lived in Italy. In 1967, I had moved to London to begin a course of studies in environmental archaeology. Thus, I had the good fortune -by accident and not design- to attend the meeting on the domestication of plants and animals held at the Institute of Archaeology at the end of my first year. I now became interested in the origins of agriculture and went on to write my dissertation on the late hunter-gatherers in Italy. Like other archaeologists at the time, I was interested in tracing local pathways from the Mesolithic to the Neolithic and even possibly finding evidence for local attempts at domestication in Italy. However, I soon came to realize that the latter was not really in the cards for the Italian peninsula. Indeed, since

the early 1980s and the advent of the new AMS method of radiocarbon dating, as mentioned earlier, claims for local domestication in Europe have dried up and all but disappeared.

In my own case, I chose to look closely at the locations and the environmental settings of the Mesolithic and early Neolithic sites in Italy. But this only revealed the cave-bound character of Italian prehistory before the Neolithic. If one considered the evidence from sites other than caves and rock shelters, no real argument for continuity in settlement could be made over the Neolithic transition in most parts of Italy. In addition, a consciousness slowly began to form in my mind with regard to the limitations of studying the transition on the basis of cave deposits alone: that is, the problem of the disturbance and the mixing of cave levels, and the even larger question of what the remains from cave sites with low intensities of occupation may actually represent in terms of the larger picture of human behavior in either the late Mesolithic period or the early Neolithic period.

For the seminar in Pavia, I chose to examine the pattern of the first appearance of farming in Europe as revealed by radiocarbon dates from early Neolithic sites. I can still remember the thick fog at the time that enveloped the city and the surrounding Po Plain in the late autumn. By the early 1970s, enough C-14 dates had become available in different parts of Europe so that one could attempt to trace the pattern of the spread. Soon we were able to make a first estimate of the average rate of spread of early farming from the Near East to northwest Europe: a rate of about 1 kilometer per year. In turn, this brought us to the question of how to explain the spread. Cavalli-Sforza recalled the model that Skellam (1951) had put forward in ecology for the dispersal of a population as a diffusionary process and illustrated with the spread of the muskrat in central Europe. We began to play with the model. And this initial exploration of the model led us, in turn, to think back through the whole problem and formulate the hypothesis of demic diffusion. However, in 1970, diffusion as a form of explanation in anthropology and prehistory had fallen completely out of favour. The use and abuse of diffusionism by a previous generation of scholars, as David Anthony (1997) has recently observed, had discredited this line of explanation -had cast it in a negative light. What was now in fashion was a new approach: one that looked inwards and placed value on the autonomous development of a society. And, if one had to deal with a case of diffusion, it was generally assumed that the cultural mode was the one that was involved. The extent of the acceptance of this position was reflected by an article on Neolithic diffusion rates by Edmonson (1961), which was at the time one of the very few studies on the question in the literature. While those offering comments on his article in *Current Anthropology* were critical of many aspects of Edmonson's study, no one really questioned that what was being measured, if



one tried to make such a measurement, was a rate of cultural diffusion. In other words, there was the need to introduce the concept of demic diffusion and to borrow the wave of advance model from the field of population biology and apply it to a problem in archaeology.

In 1971, I presented the model at the Sheffield conference on the explanation of culture change organized by Colin Renfrew, who offered his encouragement. On one hand, fashion was now on our side. There was an enthusiasm for formal treatments in archaeology at the time that the model was first introduced. On the other hand, almost no one in prehistory at the time could read the mathematics of the model. Indeed, at a conference that focused on models in archaeology, most of us were still budding neophytes at model building. At the time, Cavalli-Sforza was at Stanford University where he had just taken a new position in the Department of Genetics. Upon completing my dissertation at London, I joined him there in 1972. We now had the opportunity to work in close collaboration for the next five years. The work to be done included a closer look at the individual components of the model, the development of simulation studies that focused on settlement patterns, the measurement of the rate of spread using new methods and a much larger data base, and the examination of the genetic implications of the spread (which led eventually to the synthesis by Cavalli-Sforza *et alii*, 1994).

On a separate front, in 1974, I was invited by the new University of Calabria to do an archaeological survey in the region that forms the toe of Italy. I had recently visited the excavations of the Linear Poettery settlements on the Aldenhoven Platte in Germany and was duly impressed. What was completely lacking in the western Mediterranean at the time were settlement pattern studies of this kind for the Neolithic period. When I arrived in Calabria, not much was known about the Neolithic, and three of the four sites where excavations had been conducted so far were cave sites. This would all change rapidly with the survey, where our specific interest was in finding sites dating to the Mesolithic and Neolithic periods. The repeated, intensive coverage of the Acconia area produced from one field season to the next richer and richer patterns of Stentinello or impressed ware Neolithic habitation (Ammerman, 1985). And the excavations at the settlement of Piana di Curinga yielded a different picture for the early Neolithic than what had been found before at the cave sites in Calabria -the first good series of wattle and daub houses in the western Mediterranean (Ammerman *et alii*, 1988). Moreover, at Acconia, some Palaeolithic remains were recovered from the land surface but nothing was found on the Mesolithic side in either the survey or the excavations. In short, at least in the areas of Calabria where we did extensive fieldwork (Acconia, Nicotera and Le Castella), two conclusions could be drawn:

(1) there was little or no evidence for continuity in settlement patterns over the Neolithic transition and (2) there was good evidence for local population growth in the sixth millennium B.C. Of no less importance for the development of my own thinking was a growing realization that the household (something not studied before in Italian prehistory) represented one of the keys to the study of economic and social life in the Neolithic period (Ammerman *et alii*, 1988; Ammerman, 1989a).

In the archaeological literature of the 1970s and 1980s, it is not uncommon to find claims for continuity between the Mesolithic and the Neolithic in Europe. Except in northern Europe, few of these claims turn out to be well substantiated, however. This is especially the case if one excludes cave sites, which probably do not represent the main form of early Neolithic habitation in most regions of Europe. What emerged from the Venice meeting is the pronounced difference between southern and northern Europe in this regard. While continuity is well documented in certain parts of northern Europe such as Denmark (Price), it was striking to learn in Venice how slim the evidence still is for late Mesolithic settlement in many well-investigated areas of Greece (Runnels), Italy (Biagi), southern France (Guilaine) and Portugal (Zilhão; see also the contribution of Carvalho to this volume). And how weak the previous claims for continuity in these countries are often found to be upon closer examination. The evaluation of a specific claim for continuity calls for the detailed comparison of the final Mesolithic in a given area with the early Neolithic in the same area in terms of their diets, settlement patterns, lithic technologies and burial practices. In many parts of the Mediterranean (Greece, southern Italy, Portugal), the evidence on the late Mesolithic side was still quite thin through the 1980s. This situation is now beginning to improve in some places. In the debate over the question of continuity, it is important to remember, in terms of the history of scholarship on the Neolithic transition, that there is a natural inclination on the part of those who engage in the study of the Mesolithic period to want to see their period as the prelude to the Neolithic not just in a chronological sense but in a functional one as well. While such partisanship is fully understandable, it may not always serve the best interests of the balanced evaluation of the question of continuity.

There was a need to bring together all of the various lines of investigation that I had done with Cavalli-Sforza over the years and shape them into a comprehensive statement. Now living on different coasts (I began teaching in New York in 1977) and each with a busy schedule, this took a number of years to complete. The book finally came out in 1984 at a time when indigenism was reaching its peak. No attempt will be made to summarize the various arguments made in the book, which are familiar to those interested in the prob-

lem. A few comments, however, may be in order on the reception of the book. This was mixed as one might expect. In general, it was greeted positively by those in the fields of human biology and genetics. On the side of Neolithic studies, there was the common misunderstanding that our model was somehow taken for a historical claim that there had been constant, uniform and continuous movement of first farmers on the ground (something not claimed even in Skellam's original application of the model to the case of the muskrat). And there were other misrepresentations of what was said in the book as well. But *pazienza*, as they say in Italy: sooner or later it will all come out in the wash. The misrepresentations can be viewed, in retrospect, as part of the natural resistance that the book had to face, since its main argument ran against the tide of indigenism at the time.

Indigenism, as mentioned earlier, is the position in anthropology and archaeology that wishes to read what happens in the life of a given society or population as a self-contained affair. It can be interpreted as part of the necessary reaction to the excesses of the old diffusionism. However, the new indigenism is not without serious limitations of its own. The first time that we introduced the term was in the preface to our book in 1984 (Ammerman and Cavalli-Sforza, 1984: xiv). At the time, we did not try to explore the intellectual roots of indigenism as such. For want of a better term, indigenism served simply to define the counter position to our own in the on-going debate over the Neolithic transition in Europe.

It was only some years later - in looking back from a greater distance and with a quite different historical problem in front of me - that I began to think about the question of indigenism in greater depth. What I also started to realize was the wider potential significance of our work on the Neolithic transition in the sense of taking a pioneering step beyond indigenism. There are a number of factors that contribute directly and indirectly to the attraction of this notion in our time (Ammerman 2002). Let us consider briefly a few of these strands here.

To begin with, indigenism represents the working principle behind the majority of studies in cultural and social anthropology since the second world war. This is the field that, for years, encouraged the lone anthropologist to set out with a knapsack full of notebooks and chart the rules of organization of a remote, indigenist society. Notably, indigenism is at the heart of the school of thought known as structuralism. Structuralism was a term coined by the anthropologist Claude Levi-Strauss to describe a method of applying models of linguistic structure to the study of society as a whole, in particular to customs and myths. Here emphasis is placed on cultural difference and the need to respect it. Societies, in this view, are structures that have relatively stable relations among their elements. Change over time is not a lead-

ing concern and, if it does occur, it is regarded to have its source within a given society. Thus, structuralism, without going into the reasons for why it arose in France after the war, as Mark Lilla (1998) has recently done, spoke of each culture as autonomous.

Another strand of indigenism is connected with nationalism. In the 1960s and 1970s, for example, there was the widespread belief that each nation in Europe was entitled - by a right of chauvinism, as it were - to agricultural origins of its own. Accordingly, one had a series of claims for local domestication in Romania, in Italy, in France and elsewhere: almost none of which has withstood the test of time. In Europe, archaeology is organized essentially along national lines. This carries with it the understandable aspiration of each national archaeology to foster national identity; in effect, each national archaeology has the charge of tracing its own country's distinctive and ideally even independent course of development. Again, emphasis is placed on the autonomous, on the autochthonous, and on cultural difference.

A third strand at work involves the apprehensiveness in the post-Holocaust world that we have all come to associate with the displacement of people. The Holocaust has redefined the way in which we think about the whole question of the movement of people. Indeed, by the late twentieth century, one had already witnessed enough of this. Thus, the recent forced displacement of people in places such as Kosovo and East Timor ran completely counter to the sensibility of the post-modern world. However, the idea of indigenous people today also contains its own paradox (Beteille, 1998); many groups that would like to identify themselves in this way have actually experienced geographical displacement in recent historical times and thus they are no longer autochthonous as such.

Still another way to look at indigenism would be as a post-colonial involution, where the essential aim is to rewrite the past. Indigenism seems to be most pronounced in those nations that had a long colonial experience. In adjusting to a world that has changed, there is a need to realign one's identity. We live in an era of empires giving way to the conscious rediscovery of ethnic identity and to the natural right of self-determination. In this context, there is a strong urge to affirm that one is in control of one's own native past. By way of involution, when it comes time to look outward again and consider other societies, the archaeologist tends to project indigenism on the rest of the world as well.

In combination, these four strands yield a complex of values and attitudes that has a firm hold on our time. Thus, there is far more at stake, when it comes to the Neolithic transition in Europe, than simply how early farming happened to begin in this part of the world. If our formulation of the question is correct, then the Neolithic transition poses a fundamental challenge to the paradigm of indigenism. If this important

chapter in the prehistory of Europe -the shift to a new form of production- can be explained in some other way, then the paradigm itself runs the risk of losing power. The door is open for moving beyond indigenism.

The full realization of what was at stake only dawned on me in 1996, when I was at the National Gallery of Art in Washington D.C. By working on a quite different problem, the cultural history of the many different attempts to reconstruct the ancient city of Rome since the time of the Renaissance, there was the distance to see the debate over the Neolithic transition in a new light. Perhaps the wider significance of our work, in looking back, was in making one of the first exploratory steps beyond indigenism. Of course, little did we realize that this was what we were doing on those first foggy days in Pavia. We did not set out to challenge the intellectual fashion of the day. We were simply following the lead of our own curiosity in an attempt to solve a complex problem. There is also the irony that indigenism, as a way of viewing the world, is clearly out of step when we consider the political situation in Europe today. Looking forward, the transition now in progress -hopefully a more rapid one than the Neolithic transition- points toward more open frontiers and the freer circulation of people and information in Europe. From this viewpoint, indigenism is an anachronism: something from a former age (a way of rendering the past bound to the cold-war years just after the second world war) that is incongruous in terms of where Europe is headed today. It is perhaps time to move beyond indigenism in the study of the past as well as in our preparation for the future.

It is time for a change of pace. Let us consider for a moment, returning to the main theme of the symposium, some of the Neolithic landscapes that I had a chance to see in New Guinea several years ago. Here, by the way, I am only following a suggestion that Cavalli-Sforza made at the Venice meeting: that is, the archaeologist should try to get out and learn more about what it is like to live in a small-scale society with a technology close to that of the Neolithic in Europe. There are still a few places in the world where this can be done (Diamond, 1997). And the prehistorian should take advantage of this opportunity while it still exists. In 1990, I had the chance to travel to Irian Jaya, the western part of New Guinea that belongs to Indonesia, and experience in a more immediate way what life is like among two groups with a stone age technology: the Jali in the Highlands, who live on the basis of gardening, and a small group on the Wildeman River in the southern lowlands, who live for the most part by foraging. I would like to mention very briefly one or two things that my contact with these two small-scale, face-to-faces societies in Irian Jaya forced me to think about for the first time. They have a bearing on questions that concern the relationship between palaeodemography and Neolithic land-

scapes, the subject of the present symposium. For example, I was struck by the fluidity of residence -the open circulation of people between settlements- in the case of the Jali, who follow essentially a sedentary way of life. The whole question of the dynamics of residence (that is, where different individuals in a given community actually choose to reside at various times in their lives) appears to be one that has not received proper attention in the anthropological literature so far. It has some significance for the session devoted to palaeodemography today. There is a tendency in Neolithic studies (and prehistory in general) to equate the life histories of individuals with site histories (Ammerman and Cavalli-Sforza, 1979; Ammerman, 1989b). In fact, in the case of small-scale societies of the kind observed in New Guinea (as well as those recorded historically in parish records for small villages in the Appenines of northern Italy), many individuals do not experience all of the vital events in their life (that is, birth, marriage and death) at one and the same site. Thus, demographic events for a given individual are often distributed over the landscape. This may also be the case for the early Neolithic in many parts of the western Mediterranean. At the same time, while the prehistorian often has some prospect of studying mortality, my own experience, after visiting the Jali in the highlands of New Guinea, suggests that we might be much better served in the study of the Neolithic transition, if we had the chance to know more about fertility and patterns of local migratory activity within a given landscape. In short, mortality may not be the most interesting part of the story. Of course, even learning more about fertility in the Neolithic period represents a very tall order today.

In conclusion, there are five points that I would like to recall in returning to the Neolithic transition in Europe. The first concerns the collaboration between archaeology and human genetics that Cavalli-Sforza and I were able to pioneer. We had the courage to accept this challenge at a time when it was far from clear where the research would lead. In retrospect, the collaboration between Cavalli-Sforza and myself now seems like the obvious and natural thing to do. What could be simpler? In fact, our collaboration was seen as an unconventional and even radical move in the early 1970s. There was at that time no tradition of collaboration between our two fields of study. At the start, much of our initial effort was spent in trying to bridge the differences in our backgrounds. Looking back, we now see that this effort was well spent.

The second involves the idea of demic diffusion, which is now widely accepted and used in the literature. There was a clear need, as explained earlier, to coin this new term. At the time that we proposed the idea, diffusion in any form was out of fashion. In the 1990s, things began to change (Harris, 1996), and the spread of agriculture has once again emerged as a question of major interest. The concept of demic diffusion helped to fill a major gap in terms of how we

think about the movement of people in prehistory (see, for example, Housley *et alii*, 1997).

The third point has to do with the wave of advance model, which made it possible in 1973 to bring local population growth, site relocation and the rate of spread of early farming all together in a quantitative framework for the first time. In retrospect, the wave of advance can be seen today as a first generation model, a conceptual tool at the general level, that helped in reformulating the whole question of the Neolithic transition of Europe. In more recent studies, others have gone on to develop second generation models that treat site relocation in a more complex way. For example, if one takes a closer look at the situation on the ground in a given region, one often finds that the expansion was less continuous in space than the general model predicts. Clusters of early farming sites are separated by open spaces in the map. What this implies is that not all environments were perceived as the same by the first farmers. Some places on the landscape were preferred over others. In short, the wave of advance model was a point of departure. It has led, in turn, to the development of second generation models such as the one proposed by Van Andel and Runnels (1995) for Greece, where alluvial plains with arable soils are seen as the preferred places and they would comprise the main stepping stones in the initial spread there.

The fourth point comprises the fieldwork that I did on Neolithic settlement patterns in Calabria, which offered a chance to translate method and theory into practice. In turn, the results from the field showed how narrow our ideas about Neolithic economics had been -essentially the reduction of the economy to subsistence- and the need to pay more attention to exchange systems and the household. In other words, there was the opportunity for a dialogue between ideas and practice in the study of Neolithic settlements in Calabria.

The fifth point concerns indigenism, which continues to cast its long shadow over how we think about the whole question of the Neolithic transition in Europe. As mentioned above, it is now time to move beyond indigenism. Over the long run, one of the main contributions of our work on the Neolithic transition may well turn out to be that of focusing attention on this very problem when it comes to the study of the remote past in our time. Finally, there is a more immediate implication of all this for the present Symposium, and it would be the following. In the efforts that we make to reconstruct Neolithic landscapes in the Mediterranean world, what we have to watch out for, above all, are immobile, indigenist constructions. The Neolithic landscape, I would argue, was indeed a dynamic place.

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