Discussion

GENES, MIND AND CULTURE

Panel: *John Maddox*, Chair; *Edward O. Wilson, Anthony Quinton, John Turner, and John Bowker*

Abstract. The 1981 book *Genes, Mind and Culture* by Edward O. Wilson and Charles J. Lumsden attempts to offer a comprehensive theory of the linkage between biological and cultural evolution. In the following 21 May 1982 radio broadcast, produced by Julian Brown under the auspices of the British Broadcasting Corporation, Wilson is joined by a philosopher, a geneticist, and a religion scholar in a discussion of "gene culture co-evolution" and of other issues raised by sociobiology. The discussion is introduced and chaired by the editor of *Nature*, John Maddox.

JOHN MADDOX: There are a great many people to whom the word sociobiology is somehow deeply offensive. And it is all because of one man, Edward O. Wilson, professor of science at Harvard University. In 1975 Wilson published a book called *Sociobiology, the New Synthesis*, which has subsequently become not just a successful textbook but a controversial document. Some people think of it as if it were the bible, others as a book in a class with *Mein Kampf* and related tracts. The book created a rumpus because it seemed to many of its critics to present altogether too definite a view of how human behavior is genetically determined. To be fair, the book is for the most part a solid and scholarly comparative account of the behavior of different species of animals, within the framework of evolutionary biology, and that is why it has become a widely-used textbook. But the first and last chapters of

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the original *Sociobiology* seemed to many people to go too far. They say it made too much of the evidence Wilson had assembled for his central proposition that the behavior of animal species, like their other attributes, has been shaped during the course of evolution by the forces of natural selection. The result appeared to be a claim that animal behavior and our behavior is for all practical purposes determined exclusively by the genes we happen to have inherited. In 1978 Wilson rubbed salt in the wounds of those who thought themselves injured by the book by publishing a version of his argument aimed at a more general audience under the title, *On Human Nature*.

But in this discussion, the participants are focusing their attention on his latest book, published towards the end of 1981, called *Genes, Mind and Culture*. It has been written in collaboration with Charles J. Lumsden, a research fellow at Harvard University, and by no stretch of the imagination is it a popular book. Much of it consists of an elaborate mathematical model to account for the ways in which genes of animals and the cultural attributes of the society to which they belong have evolved in parallel.

But before the discussion itself, let me explain a little more about the controversies of the past six or seven years. There is no disagreement among biologists that, to some extent, behavior, including human behavior, is genetically determined. Thus, to give just one example, it is well known that we inherit in our genes the capacity to synthesize in our pituitary glands hormones which have a direct and profound effect on the production of sex hormones which, in turn, are intimately involved with sexual behavior. If we did not have those genes, we would not have sexual behavior as we know it. Similarly, it is also now accepted that some parts of our nervous system are formed in such a way during the course of development before birth that certain nerve cells are almost irreparably linked with other nerve cells so that, for example, we all see and hear in more or less the same way. That is genetic endowment. The dispute between Wilson and the critics of sociobiology is how much else of human behavior is determined in this way. In opening this discussion, Wilson first explains what his new book, *Genes, Mind and Culture*, is about.

EDWARD O. WILSON: Charles Lumsden, a young associate of mine at Harvard, and I have put together what I believe can fairly be called the first attempt at a comprehensive theory of the linkage between biological and cultural evolution. We have envisaged a process which we call "gene culture co-evolution." This concept is based as carefully as we can possibly make it on the known facts of psychology and biology because this effort can be called an extension of sociobiology, a field about which there has been a good deal of misunderstanding. Let me add at once that the new conception—that is, gene culture co-evolution—does not imply that human beings are genetically determined, robots of the genes. It includes a conception of choice (in fact that is one of its principle features) and free will, and it attempts to account for cultural diversity in a relatively precise manner. And, finally, it carries (if it is necessary to make such a statement—it would be in the United States at least) no inherent support for any political ideology, right or left. It is strictly a scientific theory and, for the time being, it would appear to be the only general testable and at least semi-quantitative theory of its kind; and it has created a substantial amount of interest and controversy.

The theory holds that genetic evolution and cultural evolution are tightly linked—hence our use of the expression, co-evolution. On the one hand the genes affect the way that the mind is formed—what stimuli we perceive out of a wide array impinging on the sense organs, how information is processed into long-term storage, which memories are most easily recalled, which emotional responses are most likely to occur, and of these effects (which are well documented in the literature of psychology) we have tended to use the expression "epigenetic rules." Now the epigenetic rules are distinctively human. They have a basis in human biology and they affect the way that culture is formed. Because of the rules, certain cultural choices are far more likely to occur than others. For example, the avoidance of incest is much more likely than the committing of incest—at least brother/sister incest—because of a specific rule. That rule is the inhibition of full sexual activity at sexual maturity between people who have been raised in close domestic proximity during the first six years of life.

To give another example, we see color as four basic colors—blue, green, yellow, red—even when the wave length is being changed in a continuous manner. That is because these cells in the retina and in the intermediate relay stations in the lateral geniculate nucleus of the brain break the wavelength up into discrete areas. And work by anthropologists in which the native speakers of twenty languages from around the world were asked to place subjectively where all of the color terms or many of their color terms fall on standard color spectrum plates, tended to cluster their color terms near the middle of those four discrete areas and not on the boundaries. Other experiments in New Guinea with people who started with a very poor color vocabulary showed that, when given a choice between artificial, contrived vocabularies, preferred vocabularies placed near the middle and, if required to learn them on the edges, took twice as long and remembered them not quite as far. Lumsden and I have shown that it is possible (in theory anyway) to predict patterns of cultural variation from a knowledge of epigenetic rules of this kind, providing they are studied in terms of choice and under appropriate conditions. It is possible to go, in principle, from data on cognitive psychology of the kind that we were just referring to, to data in cultural anthropology and sociology, at least in frequency distributions—patterns of diversity—that should be of central interest to ethnographers and other social scientists. And it is possible, in principle, to go in the reverse direction—that is, from patterns of cultural diversity back down to inferences concerning the degrees and direction of constraints in cognition if the fundamental role of biology is appreciated. Now, this transition from mind to culture which we have just been talking about is *half* of gene-culture co-evolution as we conceive it; the other half is the effect that culture has in the reverse direction, that is, on the underlying genes or gene frequencies. Certain epigenetic rules, that is, certain ways in which the mind can develop, certain biases in innovation, what things we are more likely to invent or try to discover in cultural choice—result in higher survival in this theoretical conception. Over a number of generations, those epigenetic rules (and therefore the genes prescribing them) will tend to become more common in the population in the traditional manner, or under the generally understood manner, of evolution by natural selection. In other words, culture affects genetic evolution just as the genes affect cultural evolution: hence again, co-evolution.

To conclude, among the findings we have made are the following. First, biology and culture are absolutely inseparable. Any change in the one ultimately alters the other. Culture has not been put over biology as an independent stratigraphic layer. Also the blank slate brain, the famous *tabula rasa* in which the mind is created solely by the circumstances of history and learning, is very improbable in evolution, we believe we have shown, and in fact does not exist in human beings. All the weight of evidence, I believe, from cognitive psychology shows that empirically the human mind is strongly biased by its biology, operating through the epigenetic rules of the kind I have just described, and ultimately therefore it is under the influence of the genes. We have also concluded that some amount of genetic evolution of the human brain and mind can easily occur within thirty or forty generations, or about 1,000 years. This so-called "thousand year rule" (as we've designated it, just to make it more easily recalled) implies that the basic mental traits might even continue to evolve, or continue to evolve in historical times, in contrast to the conventional view (or at least widely held view) that such biological evolution ceased tens of thousands of years ago, and that human change has consisted exclusively of cultural evolution since then. There are a number of other results from our pilot models and from the general conception of this form of linkage which we hope will stimulate new modes of research on both evolutionary biology and the social sciences. In general I see no reason why research on human culture cannot be conducted in the manner of natural sciences, building up from a network of causal explanation, model building, and cautious empirical research. I do not see, either, why there should be any boundary between the natural sciences and the social sciences.

MADDOX: Thank you very much. Anthony Quinton, you're a philosopher. How does what Professor Wilson has said strike you?

ANTHONY QUINTON: Well, I don't know that philosophy dictates an answer one way or another. I suppose some philosophers would be strongly committed to the autonomy of the mind as they understand it and therefore hostile to what he said. Other philosophers—and I think I'm one of those (we're called by our opponents "reductionists")—are always anxious to see large bodies of human knowledge or rational beliefs knitted together in organized systems, and there is absolutely no doubt that this is a splendidly ambitious project for the knitting together of the dispersed and methodologically various social sciences under the umbrella of human biology. Needless to say, I feel professionally constrained to be a bit methodologically skeptical here and there. For example, you get out towards the end a very proper trio of requirements that a theory of the highly general kind you lay down has got to satisfy. The first of them, you say, is the unexamined assumption that the social sciences have got to be derivable from the theory. Second, there is the requirement of predictability and testability. And third, the suggestion of new problems and so forth—that is to say a fertile research program.

I am inclined to take you up a little on the second requirement of predictability. Under the general heading of epigenetic rules, you have things which are very, very determinative: they are propensities in human behavior which are more or less universal and from which there is negligible deviation. But other epigenetic rules are just a matter of a slight bias in human beings in the direction of some choices from a range of cultural possibilities rather than other items in that range of cultural possibilities. Let me just give you one quotation, if I may, which takes the thing to the limit. It is towards the end of the book, where you give a certain range of kinds of rule: one of them would be purely genetic (revealing a point of view that some of your critics accused you of holding in the past)—with one gene for one thing, a gene, for instance, that makes men go out and drink in pubs and women stay home and mind the babies. That is the purely genetic culture. When you talk about the opposite extreme, you say that epigenetic rules can be rigid or they can be entirely unselective, resulting in what appears superficially to be a genetically liberated culture. Then you go on: "Whatever the degree of selectivity, the epigenesis is prescribed by gene ensembles—even perfect indifference must be encoded genetically" (p. 344). Well now, that makes the thesis of the ultimately theoretically crucial nature of the genetic irrefutable, doesn't it? Whatever happens, it must be happening because either genetic considerations forced it to happen or the genetic apparatus courteously held back and let it happen in some other way. To put a very crude analogy that occurred to me, somebody might say, from the point of the view of the police, that the writing of nature poetry is determined by the police because the police do not do anything about it. That is to say, the police have encoded themselves to abstain from interfering with the composition of nature poetry. What I am getting at is that you have got these two extreme cases—the purely genetic theory and the humanly autonomous cultural theory. Your theory is somewhere in between, and you have got some striking cases of the uniformity and predictability of human social behavior which it is reasonable to trace back and give a natural selection account of; but the bits of culture that are particularly interesting to people are those where they see there is, or feel there is, a choice: and it's just *there* that the epigenetic rules are most flexible.

MADDOX: John Turner, you're a geneticist. How does it strike you?

JOHN TURNER: Interestingly enough, coming in as a geneticist, I find myself I think taking rather a similar position to Anthony Quinton, who is coming in from a very different angle. The general thesis of the book, that human culture is genetically determined, *has* to be true. We are genetically different from chimpanzees. If our culture was not genetically determined, then the chimpanzees would be sitting around listening to this program. But that is almost a tautological statement.

I find your refutation of the *tabula rasa* idea very effective and, in a way, quite witty, because some of your opponents have been geneticists—indeed, your most vociferous opponents have been geneticists—and you have put up an argument which is of the type that evolutionary geneticists respect. You have produced a theorem that a *tabula rasa* is evolutionarily unstable and will not persist, and I find this a very nice argument. But beyond this point, I find your book difficult to follow—and "follow" in both senses: I find it difficult to understand really what you are saying, and as far as I do understand it, I am having difficulty going along with it.

My general feeling is that the arguments, the mathematical models, are not doing enough work either of prediction or of intellectually interesting explanation to take us into a realm of scientific inquiry. I think in a way we are still in the area of philosophical argument, of assertion and counter-assertion: "people are innately prone to avoid incest," "people are not innately prone to avoid incest"—the sort of argument that goes on and on between sociobiologists and sociologists. But to get into a series of scientific predictions, we need to be able to make some comments about when a cultural difference is going to be based on genetic differences and when it is not. There are two kinds of questions that a geneticist can ask. One is, To what extent is the genetic endowment of the whole human species responsible for particular directions of culture? It is not something a geneticist is terribly interested in doing, and the whole success of current evolutionary biology is based, not on talking about why the genome of a particular species does particular things for it, but on looking for differences within a species and then seeing in some experimental way how evolution actually works on the ground now. Thus the other kind of question is, given cultural differences, such as the fact that in the south of England visitors come to the front door whereas in the north of England visitors come to the back door, under what circumstances would that be the result of a genetic difference? How could we predict whether that is really due to the fact that the ancestors of Yorkshire men are Vikings and the ancestors of Londoners are Anglo-Saxons? Is there a genetic difference for back door and front door behavior between those two populations? This is a more jocular example than incest avoidance, but one needs to be able to make that sort of prediction (which may turn out to be right or not) in order to make it work as a piece of scientific modelling. To go back to incest, why are cross brother/sister marriages between two families not permitted in the Eastern rite whereas they are permitted in the Western rite? Is that a genetic difference? When would it be a genetic difference? When would it not be a genetic difference?

So, specifically you have tried to show that cultural and genetic changes drive each other mutually and are perhaps mutually accelerating. But in fact it is equally easy to argue that when cultural change becomes very rapid, the induced genetic changes should slow down almost to zero. That can be argued quite simply; consequently, I think we are a long way off disproving the idea that a great deal of human cultural diversity has no genetic basis whatever, except in the trivial sense that the human genome determines the boundaries of what we do over a very wide limit. So what we need, I think, is more empirical evidence on the one hand (which I am sure you would go along with) and also models which produce much clearer and more testable actual predictions.

Your book is certainly, I think, a move in the right direction, to try and take human sociobiology into a realm of quite explicit modelling; and whereas some of your vociferous opponents may say that you have gone too far, my feeling is that, in fact, you have not gone far enough.

MADDOX: Are there any points there, Ed Wilson, that you would like to clarify?

WILSON: Let me respond very briefly because I think that Dr. Turner and I could launch into a very fruitful discussion that would take the rest of the day. I should respond by saying that he has quite rightly taken the geneticists' typical stance of wanting to get down to analysis of genetic variations in populations, and that, of course, is of great interest. However, the models that we produce are quite explicit and normal with reference to the prediction of patterns of cultural diversity that can be measured in cross-cultural ethnographic studies, and thus, even though we begin with an assumption of a uniform human genetic structure as long as that produces constraints, the measure of those constraints leads to quite surprising, often surprising results with reference to cultural diversity, that should be of interest to social psychologists. So there are predictions coming out of this model and new ways of analyzing the way cognition works that *would* be outside the main domain of interest of a geneticist, at least in the first run, but I think would nevertheless make even these primitive pilot models useful.

MADDOX: But let me ask you the question that John Turner asked. Should it be possible, or should it not be possible, on the basis of your theory, to be able to predict which people go to the back door and which to the front door when they go to visit John Turner in Leeds?

WILSON: If there can be demonstrated substantial genetic variation in some of the epigenetic rules that produce strong bias, yes. But that is difficult to pin down at this very early, very primitive level of our understanding of human behavioral genetics.

MADDOX: Let's come back to that. John Bowker, how did Ed Wilson's book appear to you?

JOHN BOWKER: My initial reaction is a bit like your fellow countryman contemplating one of Eisenhower's budgets and making the very familiar remark that it is like a man with a headache who gets out a comb and combs his hair: he is very close to the problem but a long way from a solution. And this is what Tony Quinton and John Turner are saying:

everybody here is agreed that this is a necessary, urgent area of exploration, the interaction between genes and culture and their mutual mechanisms; and in my own subject, the study of religious behaviors, I think that what you are proposing gets a great deal of support. One of the examples that you use is that of fava beans, and of alkali cooking to release lysine in maize. It is possible, not only to draw diffusion maps of cuisine but also to draw overlapping maps of distribution of religious customs which absolutely fit on top of this. So in the area of religious behaviors, there is no question that there are fundamental genetic programs which are worked out into religious behaviors which, in turn, protect the gene programs.

But I want to get back to the main issue, because I really am troubled with your verbs. One might take the book, alone, but in the discussion you have been adding a few more verbs. For example, when you were talking to Tony Quinton, the verb *affect the way the mind is formed* was one of the verbs, but there are others in the book. The epigenetic rules *force profound changes*, they *strongly alter social patterns*, they are *underwritten by the genes* at the end of the book but at the very beginning they are *prescribed by the genes*. Now, these are strong transitive active verbs, but they are metaphorical: so what is the real *content* of these strong verbs? Or to put it more bluntly, What *do* the epigenetic rules do, given the different strength of the verbs? This question is being raised by both the other people in this discussion. Another word you use is *constraint*, and I would suggest that that is a more useful word, but it exposes you to the possibility that the set of constraints which control an eventuality into its outcome is much larger than the genetic set of constraints. Now manifestly the genes and the biological processes that you call the epigenetic rules are among the total set of constraints. But I would want to argue that the set of constraints controlling human behaviors into their outcomes is very much *wider* than the genes and the epigenetic rules. Indeed, I would want to argue that you cannot rule out the possibility that constraints are being derived in the religious case from an interactive and responsive Other which has traditionally been referred to as God. So the word constraint seems to me to help your program because it loosens it up at the seams but, of course, it may be threatening to you because it threatens the comprehensiveness of your theory.

MADDOX: Do you have any point of clarification to deny that, Ed Wilson?

WILSON: I think John Bowker stated a viewpoint which is very widespread among both theologians and ethical philosophers that is hard to put into some kind of testable form, but which is certainly logical and valid. Among ethical philosophers, for example, a central question is whether or not ethical precepts exist outside of the human mind and the idiosyncracies of human evolution. In other words, the question is, Do our ethical precepts, what we believe to be right and wrong, merely represent products of our evolutionary history, or are we, through genetics and culture, tracking an external set of ethical precepts which we do not yet have the wit or the logical apparatus to recognize but which somehow blindly we are moving towards? I believe that really is the central question of ethical philosophy.

MADDOX: To which, I infer, you have no answer yet?

WILSON: I have none. As a scientific materialist, and now we are talking metaphysics and epistemology, as a scientific materialist I prefer to go my own materialistic route of assuming, as a working hypothesis, that we will eventually explain all of ethical behavior and ethical precepts as the outcome of genetic evolutionary processes; but I certainly respect and am greatly bemused by the alternative explanation, I think, alluded to by John Bowker.

MADDOX: Each of the speakers so far, Anthony Quinton, John Turner, and John Bowker, has in one form or another brought up the question, How is it possible to be as predictive as you claim your theory can be, given the fuzziness both of the theory—if one might put it directly—and of the data? How do you answer that?

WILSON: I answer it by conceding immediately that the pilot model, that is, the specific, logical, and mathematical relationships that we propose on the basis of what we can understand about how the mind works and the way culture can diversify and so on, that these pilot models are rudimentary in nature and probably applicable at best to only a few of the simplest cases with strong bias, as we believe is the case in color perception or incest. I believe—and this is just an opinion—that we do know enough about the cognitive process to validate those models for the simplest cases of cultural diversity. But clearly when you get into much more complicated and difficult-to-measure phenomena (for example, the case of near equality or near lack of bias combined with wide innovation so that the cultures are inevitably going to be extremely diverse and difficult to predict), when you get into situations where there are cognitive processes going on that the psychologists have an inkling of but have not yet pinned down (such as override phenomenon and complex interaction effects in decision making, where, for example, a religious impulse or group decision-making can override a very strong propensity for a counterveiling individual decision and so on), when we get into those areas, clearly these models that we have proposed are inadequate. But I believe that this is the way to go, that is, to start with the very simple pilot model, firmly based on our empirical information, and then by testing them, by calling for new kinds of data from cognitive psychology, ethnography and so on, to see how far we can proceed step by step.

MADDOX: Anthony Quinton, does that meet your point in part?

QUINTON: Well, it is a perfectly straightforward declaration about what is the proper thing to do next. But the question is, Where is the difficulty, where do we feel the difficulties, in carrying out this project, where are they going to crop up? The point is that there are more or less four participants in your two-way gene-culture street. There is the gene; the epigenetic rule; what I am going to call a mental activity (without prejudice at all to its being activity in the brain) that is the thing that goes on in the individual person, the choosing, which is in accordance with, or an expression of, the epigenetic rules, the individual mental process; and finally, the behavior, the public observable practice. One of these, the fourth of them, is an obviously straightforward, physically realized item.

MADDOX: Are you sure... ?

QUINTON: Well, I am inclined to think so. I agree that that could be disputed, because people will say there are two identical observable actions, one of which is waving goodbye and the other is a sign of disgust. But there will usually be other material correlates or accompaniments, which I think will discriminate them. But let me go on: it is a little speculative, but there is a pretty good basis for saying that individual mental processes are correlated with observable or detectable brain activity. Finally, genetic difference: again we are very much at the threshold of this, but this too is in principle observable. But in contrast to those three, the epigenetic rule as you conceive it is a purely theoretical construct, isn't it? Now that is the item that has come into the development of your theory since it, so to speak, last made a public appearance. The epigenetic rule is a loosening of the link between the genetic base, the individual mental process, and its public expression in behavior. And the epigenetic rule is like a force in physics. It is not like a molecule or some fairly tangible thing, it is something you talk about as a convenient way of talking about certain other things. If you like, it is a bit of *reification*—to use a term which you yourself have given a certain role in your theory; and that makes me a little bit suspicious of it. It is harder to "get at" in principle than the other things, from the point of view of checking whether there really *are* such things. You have got regularities or statistical biases, regular frequencies, predictably steady frequencies of preferential choice. What you are saying is that you have to link those to the genes through the notion of an epigenetic rule, but the epigenetic rule is a purely hypothetical entity, isn't that so?

MADDOX: Which John Bowker wants you to rechristen a constraint.

BOWKER: Well, I do not want to rechristen the epigenetic rules as described in this book as a constraint, so much as to argue for an adequate specification of the *total* range of constraint in accounting for human behavior. But let me take up Quinton's question. Right at the beginning of the book, you say that an epigenetic rule, or rules in the plural, are a set of biological processes. If so, I think that needs clarifying, because those surely can be discerned, at least in principle.

MADDOX: Can they be clarified?

WILSON: Yes, Tony Quinton is quite correct in pointing to the fuzziness of the collective concept. What we have done is to use a collective term, epigenetic rules, to cover a wide array of different biological processes, some of which are reasonably well understood and some of which are only just in evidence, in peculiar properties detected during studies of developmental psychology. But they can be made explicit, and this brings us to the need, I believe, of cognitive research, the requirements placed on cognitive research for proper studies of the relation between genes and culture. The way it would proceed is as follows. We need to know more about the decision-making process with reference to how likely individuals are to change from one choice to another at each decision point that they make. And we have from cognitive psychology crude data. For example, we found about twelve cases for initial decision making on the part of infants and very young children, where the first learning process is being done in laboratory situations, where a variety of competing stimuli are presented and a clear choice can be made by the infant; and there we find, in all of these cases, where the initial choice, the initial decision, lay in which one to prefer and learn to pay attention to, there is a substantial bias. Furthermore, we have established that that bias can be measured. But all that gives us is an indication of the magnitude of the bias that may be taking place later on. Cognitive psychologists have not taken the measure of these transition rates in a context where choices are being made. They simply have not formulated the problem as they should.

BOWKER: Yes, but that means that at the moment, if you talk in that sort of language, you are not laying the ghost of Hume which Tony Quinton has just been raising. As you now describe it, it sounds as though this might be a Humean type problem—that surely it is obvious we are observing cause and effect as one billiard ball strikes another, but what we are actually observing is constancy of conjunction—or of correlation, in your terminology. Now what makes it worse is that you are talking about *in*constancies of conjunction at the moment, while we wait for the data to come in.

WILSON: What we have done is to show how to proceed, if those data are available—and we have only made rough guesses on the magnitudes of these transition rates that I am speaking of, at each decision point—in the case of several instances of village fissioning among Yanomamö Indians, and incest; but these are obtainable data. There is nothing mysterious about them. Psychologists have a technique called "informant analysis" by which they could be obtained. There are other techniques that are available for actually characterizing the nature of the memories that are summoned and upon which choice was made. Cognitive psychologists have begun to move in this direction; and they have the means of making relatively objective, replicable measurements, that there does appear to be structure in the way people make decisions and bring forth semantic complexes in their memory and the like, and that those things can be objectively measured. Once we get the measures on them, then we have step one, as required by the translation models for more complicated behavior. As we suggest here, step two is to determine the effects on this decision making and transition rates from one choice to the other, if the influence of the surrounding culture is evidenced by choices made by other people. Now, once again that is something that has been measured only in a couple of studies by social psychologists, where one can draw a very crude and tentative curve of function of how a change in the surrounding culture, a change in the behavior of people, is affecting those transition rates during the decision making of individuals. But that is a straightforward study which can be conducted, once the social psychologists are persuaded that this is worthwhile doing.

That is the second stage. Now the third empirical study that needs to be done and which we are on the threshold of doing—in this case, it is the ethnographers who are involved—is what we call "the ethnographic curve," that is, the actual pattern of diversity, how many societies as for example, if you were to take all the Amerindian, Orinoco Amazon Basin tribes, you would have quite a sample. Then you could determine how many societies or groups have what percentage of individuals preferring one thing as opposed to another, incest versus outbreeding and the like. So from the ethnographic data, cross-cultural data, you cannot yet derive real ethnographic curves, but they are within our grasp. Once again, these are the kinds of data which specialists can get if they are sufficiently motivated by the theory and think it worthwhile to get; and I would think that would be one of the first things ethnographers should go for, even independent of this theory. Now if we have those three kinds of measurements, then one can proceed, I think, quite far with the type of translation models that Lumsden and I have produced; and in the simplest cases, where these relations seem to be very strong and the curves clear-cut, we should be able to test the validity of making that kind of translation, and make a prediction also of cultural patterns from individual psychological processes.

MADDOX: Ed Wilson, having described yourself as a materialist, a scientific materialist, I am surprised that you don't then go on to say, "*instead of* this research program" or "*as well as* this research program with its three different stages." I am surprised you don't say, "the most important thing, therefore, is to see what is *the mechanism*, the underlying biological or neural mechanism, that may actually translate genetic structure into behavior."

WILSON: The reason for that is, that if one finds a strong cognitive process emerging as a bias or an incapacity at a certain age, which is consistent across populations, one assumes, on the basis of a great deal of accumulated experience involving neurobiology and behavior, that that *does* have a biological basis, and in a sense, in dealing with the theory of the linkage between biology and culture, we can defer that domain of investigation, for the moment, anyway. I wanted to concentrate in my discussion (and Lumsden and I emphasized it in our book) on these other areas that range from cognition up to ethnography, because we felt that that was the more poorly formulated, and that it would be much better to be explicit about what kinds of data are needed *there*, and how they might be linked up. But I completely agree that the biological basis is of surpassing importance.

MADDOX: John Turner, you have heard this discussion. How does it seem to you that specific mechanisms relating genetic structure or genetic constitution to behavior might be devised, and might fit in or not fit in with what Ed Wilson says?

TURNER: What we have so far is a proposed causal connection between genes, programming the development of the organism within certain limits, and the structure of the nervous system, determining in some way the behavior of the organism. I think what your thesis says so far is simply this: if people are strongly constrained, by the way their nervous system develops, to perform one kind of behavior (and that is quite clearly true in some cases—we are strongly constrained to walk bipedally, to take an almost noncultural example), then most people will do it and most cultures will do it; and your ethnographic curve merely states that if most people do not like incest, then "everybody's not doing it," though there will be a few cultures with just enough flexibility to push them to a slightly higher level of inbreeding. But if we have a kind of development which permits a lot of flexibility in our behavior, then cultures will show some kind of diversity, and you may be able to model the kind of diversity they will show. But that is virtually what you have been saying in previous books. The original point of *this* book, I thought, was the co-evolutionary cycle, and the argument (which we do not seem to have dealt with) that there is feedback from cultural development back to the genes. Maybe we could take a specific example, the splitting of Yanomamö Indian villages. As I understand it, you postulate that some individuals, when things get tough in a village, like to go with the guy who is going to found the new village, and some people like to stay. My immediate thought about that is that the feedback reaction is going to look like this: when the village splits, the people who go to the new village are departers. If there really is a genetic basis, if we are going to talk about co-evolution, you should get a village full of departers. That village should now split earlier and at a smaller size than the previous village. In fact, if you trace villages through their history you should find that the villages in the splitting lineage that are splitting off are smaller and smaller, and that they are splitting faster and faster. That is the kind of co-evolutionary prediction which might or might not be borne out in the real world, but that is precisely the sort of conclusion that you don't draw.

WILSON: Yes, well, as a matter of fact, all of our discussion up till now has dealt with just half of the co-evolutionary cycle. We have not yet dealt with the whole inverse coming back from culture to the genes. John Turner has just indicated a very clever model, that might be developed in the case of the village-splitting among the Yanomamö, which we did not introduce into the book. We had other pet cases. But that is quite true. As you have just described it, you might expect a tendency to split at smaller and smaller levels. And in fact, that is what happens, because the Yanomamö generally split at just that level, that minimal size level, in which you can produce two viable villages, so that they have in fact gone about as far as they can. Another case, that could be put down as a research proposal, would be concerned with the facial expressions: here we have an example of a behavior phenomenon that is really quite rigid. The facial expressions denoting the basic emotions—fear, loathing, rage, happiness—are just about universal in human populations, very specific in the form of the face, when you think of it. People from the Highlands of New Guinea can read photographs of Americans and Europeans with about an eighty percent accuracy rate and vice versa. Incidentally, we can read chimpanzee photographs with ten or twenty percent accuracy. They are our closest relative among living species and also, incidentally, they have apparently very similar brother/sister incest epigenetic rules, which have been worked out recently with wild populations and laboratory populations. So one could perhaps look for facial expression mutants within populations and hopefully, as has been done for many other behavioral physiological traits in human beings, begin to work out the genetic basis of them if they exist. I think it is very likely that they do exist, things like excessive smiling, certain forms of ticks, being poker-faced and the like. This is completely conjectural, but I cite it as an example of how one might be able to take behavior or cognitive process which appears to be under these kinds of constraints, in other words, go for it in the manner that Dr. Turner has suggested he would like to see done as a geneticist, in a traditional genetic manner, and approach the problem from that lower level and take it on up. The genetic basis, genetic differentiation, mutants, would give us some insight into what kind of neural of physiological mechanism might be involved, and then we would move on up to see how this affects the choice patterns in individual decision-making, and then we can go on from there towards culture.

BOWKER: Looked at in that way, it is obvious that cultural change and cultural decision can have a very rapid effect on the pattern of gene distribution. An example would be sickle cell anemia, which is under the control of a single gene. The cultural change in Africa of cutting down the forests has had a more rapid effect than even smokeless zones and melanism in moths, in changing the distribution and the frequency of hemoglobin S, precisely because the mosquitos are now breeding all over the place, whereas before they were not; and so the immunity or partial immunity granted is immediately rewarded. So there is no question that the cultural change may work back very very fast indeed.

Now it seems to me that if that is the case, one needs to understand what is going on in the cultural process as clearly as you are trying to understand what is going on in the genetic process. And I have a feeling that what we have got here are two different though coevolving, coadaptive information processing systems. The human organism is one kind of information processing system, but cultural artifacts like the Houses of Parliament, the British Museum, the Kremlin and so on, are a different kind of information processing system, and they are subject to more constraints than those which are derived from the epigenetic rules. Granted that there must be some prime ministers who have to be genetically constructed, there must be some professors (fortunately) nevertheless the point I was trying to make earlier comes back again. You do not have to be an idealist, antireductionist in order to try to specify a wider range of constraints over cultural reality. It sounded as though you and Tony Quinton were agreeing that either you were a reductionist or you were not. If you were not, you could have some sort of fuzzy thing like a wider set of constraints. Now, that seems to me very misleading and I think you are living in a very either/or world: *either* people marry out *or* sibling incest; *either* people have beards *or* they are clean-shaven. Bjorn Borg "sort of wears a beard" for Wimbledon. Is that predictable from the epigenetic rules or from the cultural process of superstition? And therefore you are left, to me, with a totally unworkable model at this level, this way round, because in your book you reduce it again to either/or. Right in the middle of the book you say, "In such a case, surely we can speak of culture's acquiring a life of its own, utterly independent of individual concerns. But no, not at all, we merely return to the leash principle. It is possible to demonstrate that no cultural juggernaut will persist indefinitely under such ill-fitting conditions." For sure. Who could possibly disagree with you?

WILSON: Many, many anthropologists.

BOWKER: I mean that I would not disagree with you on that. What I disagree about is the overstatement of pulling apart *either* the cultural juggernaut *or* the epigenetic rules. Now maybe you put that strong dichotomy in because you have your own particular battles with anthropologists. What it seems to me is that we need to know much more clearly what are the mechanisms by which culture works back on gene patterns—as in the example I gave from Africa—and also what are the discernible constraints in addition to the epigenetic constraints which allow cultural change, process, and exploration to take place?

MADDOX: Could I make one point, and I would be glad if John Turner would correct me if I am wrong about this African example. I can quite see how changes in agricultural factors have changed the actual gene composition of the population living in West Africa, the proportion where there is sickle cell anemia genes or not; but it seems to me that there is, as yet, no evidence that changes in agricultural practice have changed the structure of the human genome in West Africa. The distinction is between the actual genes possessed by the people living there and their genetic capacity, the, as it were, structure of the genome. Now, if that is right, I still find it hard to see what mechanisms there could be for culture actually working back, not merely on the composition of the genetic make-up of the population but actually on the structure of the genome. Can you see any, John Turner?

TURNER: I do not think that large changes in the genome are at all relevant to this discussion! What is relevant is the simplest kind of genetic change you can get, of the kind that Professor Bowker was talking about, where there is a change in a single gene; those are the ones that we know a lot about, and where we can make some fairly firm predictions. Now, you see, the sickle cell anemia case is interesting, because you have got there a cultural practice influencing disease transmission and then immediately influencing the genetic resistance to that disease. That is a very obvious way in which culture is going to influence human evolution, and enough changes of that kind will ultimately, in the very long term, result in quite substantial changes in the genetic structure of the species. But the point about this is that people do not have a choice as to whether they have got sickle cell anemia or not, and it is something which makes a lot of difference to your survival. But if you come down to something which *is* cultural, where people *do* have the choice and their behavior is not totally determined by their genes, then the effects of selection will be much *weaker* on the genes; the environment does not perceive the gene, it perceives only what somebody does. If they do one thing they have more children, if they do the other thing they have fewer children. But the speed of evolution in those circumstances is much slower, because you have got this kind of indeterminacy between the behavior and the gene, and natural selection cannot get in there and work on the gene. It is working at a second order level. So changes will be much slower. If the cultural practice is to cause rapid evolution, it has to be something that really affects your survival in quite a dramatic way. A cultural practice which only makes a small difference to the number of off-spring you leave is going to produce much weaker selection on the genes, and one would not expect cultural change to produce very rapid evolutionary changes if there is a degree of flexibility in the behavior.

Therefore, the "thousand year rule" is I think the fastest you can get out of the model. It is not the norm you would expect for genetic change—it is about the quickest you can get if you put all you have got into making things go quickly.

To come back to the point I made at the beginning, as cultural change speeds up with this kind of flexible behavior, you would expect genetic change to slow down. Let me just give a simple, topical example. Wearing a seat belt when you are driving a car makes a difference to your survival and surely is subject to natural selection: people who do and do not wear seatbelts will leave different numbers of children eventually. Wearing a seatbelt is no doubt influenced to some extent by your temperament, and we know that some aspects of temperament are likely to be inherited. Fine. So if a society has for 2,000 years the choice of wearing or not wearing seatbelts you would expect to see substantial genetic changes just as a result of that behavioral choice. But, in fact, we do not have that choice for a couple of thousand years; we have had it for twenty years and society is now about to go over to 100 percent seatbelt wearing. The genetic changes produced by seatbelts will be absolutely negligible. And that must be becoming a very general rule in human cultural evolution. The cultural change has become too rapid to produce any real genetic alteration.

WILSON: That is a very accurate summary, incidentally, of our feedback models; and let me add to this the following: yes, indeed, the intervention of choice does slow the genetic evolution, it slows it, as we have showed. I think this is one of the values of our models, that we actually could measure how much it slowed. It slowed it by about three or four times. However, the conditions under which cultural innovation is occurring, along the lines you were just indicating, can slow down genetic evolution still more, well below that maximum rate which you accurately noted is encapsulated in the thousand year rule. The conditions are much more complicated and interesting than you just indicated. It is really a matter of how many cultural innovations and how rapidly they turn over. So that whether we can track these innovations or not, that is why we emphasize whether or not a certain choice pattern would be available to a population for say twenty years or 200 years. It makes an enormous difference. If it is available for 200 years, the population has probably begun to track it, because you get a significant amount of genetic change. But one can conceive also of a pattern of innovation with new techniques, new choices appearing through invention or importation, at such a pace that now the evolution can be brought to that maximum. So I think of the interaction that, although generally the intervention of culture slows down genetic evolution, it slows it down far less than most people intuitively believe, and that there are conditions in which innovation, cultural innovation and change, can move genetic evolution along at a rapid clip. But, of course, the interesting thing is that we do not yet understand all of those conditions or whether or not they have been met in different periods of history.

MADDOX: So there we are. You must judge for yourselves whether Wilson stood up to the criticisms levelled at him. When he was in Britain a few weeks ago he spent a good deal of time talking to groups of academics about his latest theory and about sociobiology, the occasion of the earlier storms. I gather that most of those who heard him were impressed by the moderation of his case and the intelligence with which he made it. But you will also have gathered from our conversation that several blank spots remain. We are not sure that Wilson's concepts, epigenetic rules for example, are clearly defined or that they are simple enough to be dealt with by mathematical models of the kind that he has constructed. We are not sure whether his predictions about the evolution of society are as useful or as certain as he claims. We are agreed that Wilson has taken hold of an important and interesting problem, but we are all convinced that a great deal remains to be done before it will be comprehensively established.