opposed to c_1 is denoted u_{12}." This definition of u_{12} is inconsistent with the definition of u_{12} given in the body of the paper, where u_{12} is defined as the probability of making a switch from c_1 to c_2. For example, although 80% of the individuals may prefer c_2 and so according to Table 1, $u_{12}-u_{21} = 0.08-0.2 = 0.6$, it may be the case that each individual is satisfied with his choice and so the probabilities of a switch from c_1 to c_2 or from c_2 to c_1 are both zero. This ambiguity in the meaning of the epigenetic rules appears throughout the paper and also appears in *Genes* and in the BBS Précis (1982b).

To make contact with the empirical data, the definition of epigenetic rule as it appears in Table 1 is needed, because, as L&W point out, cultural "responses have not been investigated with reference to their dependence on the behavior of the rest of society" (1980b, p. 4385) as would be required in evaluating u_{12} (n_1,n_2) defined as a transition probability function. The L&W theory cannot be related to the empirical data, at least at present, because the transition probability function which is needed as an input to the mathematical model is a totally unknown quantity (assuming such a quantity exists at all) and cannot be estimated from the empirical data of Table 1.

Making use of all these assumptions (including, in addition, an unstated one that the frequency of decision points is governed by a first-order rate law), L&W are then able to write down and solve a differential equation relating the time rate of change of the probability that at any given time n_1 and n_2 individuals will possess culturgens 1 and 2 respectively to the assimilation functions u_{ij}.

The major conclusion L&W draw from their model is that "even small differences in the epigenetic rules, reflected in the assimilation functions are magnified during social interaction into the dependent ethnographic patterns" (1980b, p. 4384). In other words, imperceptible genetic differences can lead to widely varying social behavior. For the particular choice of the u_{ij}'s made by L&W, as well as for many other choices of the u_{ij}'s, the solution of the differential equation contains a term in which one of the genetically determined parameters characterizing a u_{ij} is multipled by N, the population size. Because this product appears as an exponent and because N is large, the sensitivity of the results to small changes in the parameter is guaranteed. L&W maintain that this sensitivity is a strength of the theory because it shows how small changes in the genetic rules, the u_{ij}'s, can result in large changes in the cultural patterns. We regard this sensitivity as arising from the extreme sensitivity of the model to its fundamental assumptions. The magnification property arises because each individual is affected to an equal extent by every other member of the population and because the population is quite large.

L&W constructed their theory in an attempt to overcome the current limitations of sociobiology. However, because the assumptions of their theory are so severe and because the environmental parameters included are limited to a single one (the number of individuals in a particular cultural state), the L&W theory does not appear to offer any new understanding of the coevolution of genes and culture.

Gene–culture theory and inherited individual differences in personality

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We believe Lumsden and Wilson's (L & W's) *Genes, Mind, and Culture* (1981; henceforth *Genes*) to be a landmark book. L & W's basic thesis, that there is a positive feedback loop such that genes → neural and chemical substrates → mind → behavior → culturgen assimilation → genes, with the environment exerting influence at each link, seems substantially true (i.e. congruent with most of what we know). More important, gene–culture theory suggests novel programs of research that may lead to a synthesis of the biological and social sciences. We suggest that progress in this endeavor will be facilitated by the explicit addition of the nomothetic study of individual differences (i.e. the psychology of personality).

The nomothetic study of personality consists of a search for general laws having wide applicability to people in which consistent patterns of individual differences in behavior, sometimes called traits, play a central role. Basic assumptions of this approach include substantial consistencies of people's behavior when reliably assessed, and considerable predictive power of measures of traits in accounting for behavior (Rushton, Jackson & Paunonen 1981). Numerous dimensions of personality have been investigated over the last few decades, and assessment techniques have been created for their measurement (Anastasi 1982). Moreover, there is a growing literature demonstrating that individual differences on many of these traits are inherited, including: activity level (Willerman 1973), aggression (Owen & Sines 1970), altruism (Rushton, Fulker, Neale, Blizard & Eysenck, 1984), anxiety (Floderus-Myrhed, Pedersen & Rasmuson 1980), criminality (Ellis 1982), dominance (Carey, Goldsmith, Tellegan & Gottesman 1978), intelligence (Bouchard & McGue 1981), locus of control (Miller & Rose 1982), political attitudes (Eaves & Eysenck 1974), sexuality (Eysenck 1976), sociability (Floderus-Myrhed et al. 1980), tough-mindedness (Eysenck & Eysenck 1976), and values and vocational interest (Loehlin & Nichols 1976). The cited studies found that approximately 50% of the phenotypic variance was associated with additive genetic influences. We suggest, therefore, a redrawing of the schematic presentation of L & W's reciprocal process between genes and culture to make the individual-difference component explicit. Thus: Individual differences in genes → individual differences in neural and chemical substrates → individual differences in minds → individual differences in behavior → individual differences in culturgen assimilation → individual differences in genes.

It seems strange to us that an explicit focus on inherited individual differences is such a rare occurrence in writings on human sociobiology for, clearly, theories in evolutionary biology *require* that individuals differ genetically one from the other. Yet most sociobiological writings focus on either interspecies differences (rather than intraspecies) or on presumed universals in human behavior. *Genes* is only partly an exception to this. Although at the outset L & W posit that, for their theory to be correct, "genetic variance in epigenetic rules must exist within human populations" (p. 10), they subsequently place little emphasis on such genetic variance in their discussion of either the epigenetic rules themselves (Chapters 2 and 3) or how the genes do translate into culture (Chapter 4). This is unfortunate, for a focus on individual differences might have highlighted interesting facts. Consider, for example L & W's discussion of the hypothesized epigenetic rules underlying fear of strangers among infants. Their discussion proceeds as though such fears were (a) universal, and (b) limited to a particular point in ontogeny. A focus on individual differences, however, might have led to the prediction that those infants who were the most fearful of strangers would grow into the most socially anxious adults, an expectation borne out by data (Block 1981; Kagan & Moss 1962). Thus, from an individual-difference perspective, anxiety is a deep-rooted personality disposition, partly inherited, demonstrating longitudinal stability, and manifesting itself at a very early age. From a gene–culture coevolutionary perspective, it might also be expected that high and low anxiety people will have different life-styles and social environments (culturgens) and subsequently demonstrate differential genetic fitness.

A synthesis of the psychology of personality, behavior genetics, and the theory of coevolution allows for a range of intermediate tests of gene–culture theory and leads to interesting lines of inquiry. Thus it follows that variance in (partly inherited) measurable personality traits will be correlated with (a) variance in the physiological systems underlying those traits, (b) variance in the culturgens produced and assimilated, and (c) variance in genetic fitness. Preliminary evidence can be gathered in support of each of these predictions. In regard to (a), that is the physiological systems underlying traits, Gray (1982) has described the cytoarchitecture of the "brain inhibition system" and linked activity in these fiber tracts to personality differences in anxiety level. [See also *BBS* multiple book review of Gray's *The Neuropsychology of Anxiety*, *BBS* 5(3) 1982).] The work on the evoked potential and other physiological correlates of IQ (Hendrickson & Hendrickson 1981) constitutes another prime example of matching individual differences in behavior with those in neurophysiological systems. In regard to (b), that is different personality types producing or assimilating different culturgens, consider the studies examining the role that personality plays in scientific creativity. Many studies have found successful scientists to be more socially introverted than average (e.g. Cattell 1962; Terman 1955); other studies have also found them to be more intellectually curious, needing of cognitive structure, dominant, and independent (Rushton, Murray & Paunonen 1983). Thus individual differences in scientific creativity are in part inherited (see also Karlsson 1978). In regard to (c), that is differential genetic fitness, epidemiological and demographic studies of abnormal personality suggest that those who suffer from extreme anxiety, depression, and low IQ have fewer children than those with more moderate scores (Rosenthal 1970).

The synthesis of gene–culture coevolution with behavior genetics and personality psychology has only just begun. The implications, however, may be far-reaching. One might conjecture, for example, that some personality types will thrive more in some cultures than others. To take some speculative examples, (a) genetically similar personality types may seek each other out in order to provide mutually supportive cultures (there is, for example, assortative mating for personality traits; Vandenberg 1972); (b) genetically similar individuals may form natural antipathies toward those who have genetically dissimilar personalities; (c) cross-cultural and group differences in behavior may be partly genetic in origin (Osborne, Noble & Weyl 1978); and (d) religious, political, and other ideological battles may become as heated as they do partly because they have implications for genetic fitness; in other words, genotypes will thrive more in some ideological cultures than others (recall that political attitudes are partly inherited; Eaves & Eysenck 1974).

Irrespective of the above, we are proposing that genetically based individual differences become a crucible for theory construction in gene–culture theory so that the formulation of hypotheses should lead to an immediate individual-difference test. If this were done, not only would predictions become more honed, but some hypotheses would be considered less useful even at the outset. For example, in Chapter 8, L & W suggest that knowledge of the deep structure of epigenetic rules might help humans to find and agree on universal goals. From the perspective of individual differences, however, one might ask: How could there ever be universal agreement on goals if there will always be individual differences in goal preferences?

Natural selection and unnatural selection of data

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I have now had the benefit of reading reviewers' comments on Lumsden & Wilson (1982b) (L & W) and the authors' reply (Lumsden & Wilson 1982a). In view of the confusion which exists even among geneticists about the basic concepts of genetics, I am not much surprised at the favourable and, at times, admiring comments L & W receive from some of the non-geneticists. The second sentence of L & W's response is that "the reviewers do not deny that biological and cultural evolution are somehow coupled." To me this epitomises the basic misunderstanding among those who are loosely called "social biologists." The relationship between biological and cultural evolution is evident in phrases like natural selection by adaptation and the survival of the fittest. Needless to say, what a species found better for its survival, it incorporated into its "culture." It is true that some selectionists created controversy by attempting to explain the development of every feature of an organism by natural selection, but the relationship between the biological imperatives of a species and its "culture" has to my knowledge not been in dispute for some time. The problem arises when this simple and obvious hypothesis is applied to an organism that can control and alter its environment and culture.

I may be forgiven if I feel uninterested in the exotic debate concerning the definition of culturgens. I simply think that the word was an unfortunate choice. Neither have I any interest in the models of cultural transmission that involve no selection. L & W state epigenetic rules which, they say, are genetically determined procedures. They are determined to show that social behaviour is shaped by natural selection, and they give specific examples of gene–culture translation. Along the way they throw caution and scientific objectivity to the winds. They also show a remarkable unawareness of the major problem in establishing the evolution of any trait by natural selection.

L & W discuss three examples of gene–culture translation where genetically determined epigenetic rules apply. The first and most important is brother–sister incest avoidance. They say that "the epigenetic rule appears well established: a deep sexual inhibition develops between people who live in close domestic contact during the first six years of life" (1981, pp. 147–48). In support of this assertion they cite Wolf's (1966; 1968; 1970) data and also Kaffman (1977). Their description of Wolf's data, if accurate, would require an objective reader to grant the possibility and, indeed, the likelihood of the existence of their epigenetic rule. Unfortunately, omissions make it unreliable. Moreover, they fail to evaluate Wolf's research objectively.

L & W write that in "the nineteen families analyzed by Wolf's 1966 report, for example, the young couples refused to go ahead with the match in fifteen cases. In two cases one member of the pair died in childhood, while the two remaining couples married" (1981, p. 149). Actually, in his 1966 report Wolf discussed two different samples. Members of the sample L & W discuss were born between 1910 and 1930. Members of the other sample were born before 1910. In that sample the position was almost exactly the opposite. This may be the reason L & W ignore it. Of the 22 young couples in that sample, one member of the pair died in four cases, in one case the parents decided not to go ahead with the arrangement, and in one case they were dissuaded from going ahead. The remaining 16 couples married. This hardly provides evidence for L & W's "well-established" epigenetic rule. Anyone reading Wolf's papers objectively cannot disagree with his assertion (Wolf 1966) that "while these data [the 1910–30 sample] indicate that young people were not always happy with the alternate form of marriage, this is not necessarily a result of their having grown up in intimate association."

L & W are entitled to say that they dispute Wolf's interpretation of his data, but they are not entitled to do so if they indulge in selection from his data. Moreover, by selecting data they free themselves from a consideration of the reason for the cultural change indicated by the two sets of data. Did this change occur because of some epigenetic rule? The answer is no.

The families Wolf studied brought up the daughters of other families as "little brides" in their own homes. These brides were