Biological Determinism and Symbolic Interaction: Hereditary Streams and Cultural Roads

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This paper contributes one more chapter to the long history of the intersection between physiological and social psychology. The impetus for this paper is a report by Caspi et al. published in *Science* (2002), one of the most prestigious general science journals[i], which attracted considerable media attention in the summer of 2002 for its claim that criminal behavior had a biological basis, arising from childhood experiences of maltreatment. Although much of the subtlety of this paper was lost in the media portrayal of it, the widespread attention it received is a reminder of the fact that many people are receptive to a kind of 'biological imperialism'. In response, we argue that symbolic interaction must rediscover and re-appropriate the engagement with biology that was a consistent theme in the work of Mead and his contemporaries. By so doing, symbolic interactionists will be well-placed to participate fully in interdisciplinary studies.

A biological basis for crime?

The popular media tend to present behavioral genetics in what Plomin (1994) has called OGOD (One Gene, One Disease) terms. This may make biological arguments easier to follow but it leads to serious distortions. In fact, most current biological accounts are much more subtle than OGOD. Genes are now routinely understood by researchers to have both multiple targets and effects and to be regulated by their 'environments'. The research report discussed in this paper by Caspi et al. (2002) is a case in point. This research was reported in the media as the discovery of an OGOD gene for crime. In fact, the research was rather more involved. What the paper actually claims is that childhood maltreatment interacts with a functional polymorphism[ii] in the gene encoding for an enzyme called monoamine oxidase A (MAOA), leading to a systematic variation in the level of antisocial problems caused by these individuals.

The study is based on a New Zealand birth cohort of 1,037 children (52 per cent male) that had been tracked to age 26. Maltreatment before the age of 11 was associated with antisocial problems. However, the combination of maltreatment with a 'low MAOA genotype' increased the risk of 'antisocial outcomes' while maltreatment with a 'high MAOA genotype' did not. The authors argue that this association may be consistent with a causal relationship between stressful experiences and antisocial behavior mediated by neurotransmitter development. So, the research reported by Caspi et al. is a combination of molecular biology and longitudinal data that considers the interaction MAOA types, maltreatment and anti-social behavior.

Caspi et al.'s paper is elegantly constructed and is reasonably judicious in its conclusions. However, despite its recognition of environmental factors, many social scientists would find it entirely unconvincing. This is because its molecular focus has reduced complicated and consequential social processes to an undifferentiated blur. When dealing with molecular biology, the subject matter of the natural sciences, the paper is persuasive, but this is lost as soon as the paper considers elements of the social world, notably 'maltreatment' and 'anti-social behavior'. It is useful to contrast the precision of the paper's handling of genes with the murkiness of its handling of social factors. Caspi et al. begin with a very precise definition of a relationship between a specific gene, which may occur in different alleles, its production of a particular enzyme and the action of this enzyme in the metabolization of identified neurotransmitters.[iii] However this precision deserts the team as soon as they turn to considering the alleged social correlates of these biological processes. Caspi et al. see the natural science world very clearly and at 'close range'. However, the messier world studied by social scientists is a blur to them. As we will argue in this paper, Caspi et al. conflate different objects when they try to integrate problematic elements of the social world such as 'maltreatment' and 'anti-social behavior' with less problematic elements of the natural world, such as enzymes.

The paper begins by arguing the case for an association between maltreatment and neurotransmitter systems, hypothesizing that maltreatment heightens neurotransmitter activity, with the result that any deficiency in an enzyme that metabolizes neurotransmitters will have an exaggerated effect in terms of subsequent outcomes. However, the weakness of Caspi et al.'s argument is that maltreatment is defined only in terms of a loose set of proxy indicators. These are: lack of parental affection and neglect, as judged by observers of parent-child interaction at age 3, severe physical punishment, based on self-reports by parents at ages 7 and 9 and self-reports by cohort members at age 26, multiple changes in primary caregiver and unwanted sexual contact, also based on self-reports at age 26. These different experiences are merged into a single cumulative exposure index. 64 per cent of the sample had no maltreatment by this index, 28 per cent had one experience, labeled as 'probable maltreatment' and 8 per cent had two or more, labeled as 'severe maltreatment'.[iv]

'Maltreatment' is, however, ultimately a label applied by an observer to a set of acts. As a result, its definition has proved highly malleable, according to the interests of investigators (Gelles 1975; Graham et al. 1985). The prevalence and incidence of maltreatment can be constructed more or less at will, according to the definition that is chosen (Dingwall 1989). Although Caspi et al.'s study is supposed to be based on a cumulative index, there are actually only three points on the scale – nil, probable and severe. This inevitably maximizes the number of children classified as 'severely maltreated'. We can illustrate this by reference to the definition of sexual abuse. This includes any self-report of genital touching before the age of 11, as well as grosser acts of attempted or actual intercourse. There is no consideration of the context of the touching or whether it was actively undesired – all touching is assumed to be unwanted. A single act in the common context of play between small children co-bathing with each other or with parents is sufficient to warrant labeling a cohort member as potentially abused. This is certainly a view current in child protection circles but for many researchers it is a marker of the prissiness of moral entrepreneurs rather than something comparable to the chemical signature of MAOA.

Problems also arise with the equally diffuse outcome measures of 'anti-social behavior'. This set conflates judgments of adolescent conduct disorder ('adolescents displaying a persistent pattern of behavior that violates the rights of others'); convictions for violent crimes (common assault, aggravated assault, domestic violence, manslaughter, rape); self-reports on an aggression scale; and ratings by associates. Anti-social behavior, however, depends on what counts as pro-social behavior in particular cultural environments. As we noted above, the Caspi *et al.* study appears to invoke a standard of gentility, which may reflect the world as the authors would like it to be but which may not have much to do with the world as it is outside the groves of academe. The person who 'does not show guilt after doing something bad', for instance would have been a hero to the existentialist philosophers of the 1940s and 1950s. The person who is 'impulsive, rushes into things without thinking' may have an important role on the sports field or in armed combat. A soccer team, like an army, may need both thinkers and doers, people who will take risks without evaluating them.

The current biological stance, though, is exemplified by Rowe (2002:3) who scoffs, in a recent introductory text on biology and crime, at 'some social deconstructionists [who] say that crime is an entirely arbitrary cultural invention' and goes on to assert that murder and adultery are universally prohibited. Unfortunately, he is simply wrong. As the essays in Bohannan (1960) show, the dividing line between homicide and suicide is a variable one in many traditional African societies and, even in our own, there is a complex jurisprudence on the difference between murder, manslaughter and accidental death. Murder may be universally prohibited but what constitutes 'murder' is entirely contingent on the way particular societies define the significance of deaths. Essentially the same points can be made about adultery and Rowe's other examples - stealing food and telling untruths. These categories are the contextual product of social processes by which behaviors are defined rather than Aristotelian forms, where meanings are inherent in acts. We do not know what murder is when we see it, in the way that biologists know what MAOA is. There are no inscription devices (Latour and Woolgar 1979), merely a lengthy process of social organization that leads to a warrantable for all practical purposes decision that this death is a murder and that death is manslaughter and that other death is suicide. The problems were acknowledged almost twenty years ago by a leading UK birth cohort researcher, discussing the problem of predicting delinquency from longitudinal data:

> What is called a crime depends in part on who the caller is. The totality of acts that break the law may have no other shared description. One cannot know in advance whether criminal acts (even criminal acts of a certain type e.g. breaking and entering) have a single set of causes. Heterogeneity of crimes could mask important causal relationships. (McCord and Wadsworth 1985: 61)

Conversely, homogenizing crimes may create spurious causal relationships.

Caspi et al. have nothing to say about the possible consequences for their argument of the complex processes of decision-making that lead to the identification of some members of their cohort as anti-social. If we stick only to those who have been incarcerated, it is the uncontested wisdom of even the most traditional criminologist that this population is not a representative sample of those who commit acts capable of being defined as criminal. Incarceration is the result of a long sequence of screening decisions, beginning with the victim of a crime or a first responder and their decision whether or not to seek the intervention of the criminal justice system. At each stage, from investigation through arrest, charge, trial and conviction, the population is winnowed in non-random ways that lead to the particular pattern of variables that characterize the incarcerated. It is, then, essential to distinguish between those factors associated with the original act and those associated with decisions to screen in or out at each stage of the criminal justice process. This point was originally made in response to Lombroso's (1911) criminal anthropology, which argued that a set of physical characteristics that led men and women to be regarded as particularly brutish and threatening to respectable society were related to their acts rather than to law enforcers' screening decisions. More recently it has been raised in relation to sex chromosome abnormalities, which often led to distinctive physiques:

> Even if their behaviour was no more aggressive than XXY males, it might be that because of their great height and build they would present such a frightening picture that the courts and psychiatrists would be biased to direct them to special hospitals for community safety. The bias might be further aggravated by the associated intellectual abnormality. This factor might find expression in the raised incidence of XYY (and XXYY) males in special hospital groups. (Hunter 1966: 984).

We might reasonably ask, then, whether the factors that lead both professional and lay reviewers of the behavior of these cohort members to define them as in some sense disturbed also lead criminal justice system personnel to process them in ways that increase their risk of conviction and incarceration. It is not necessarily the case that they are any more or less anti-social but that they are more likely to be selected for high-tariff processing. The same point can be made in respect of the psychiatric diagnoses, which again rest on non-random samples of the population of potential candidates, especially for a condition like adolescent personality disorder.

If these acts had the same thing-like quality as MAOA, then it might be easier to sustain the claim for a causal association. Unless a comparable behavioral object, independent of social definitions can be specified, however, the claim makes very little sense. Here, the best that seems possible is that some people with low levels of MAOA seem to cause other people some unspecified trouble, while others with high levels of MAOA seem not to. Even then, we may still need to know more about what 'trouble' means.

The core of the problem lies in the philosophical realism of some biologists, a position that they share with most practicing scientists. Realism does not produce obstinate difficulties in the everyday conduct of science. 'Any competent geneticist' can see the existence of the polymorphism, or at least regard the inference of its existence from indirect measures as unproblematic. The association of the polymorphism with varying levels of MAOA rests upon a chain of intervening processes whose activities are clearly recognized, if not always fully understood. That understanding is, however, merely a matter of time as the process of research leads the collective knowledge of the community of scientists into closer correspondence with the reality that it is observing. The sociology of science's skepticism about this epistemology is almost incomprehensible to most working scientists. When you hold an Eppendorf tube up to the light and see a tangle of DNA at the bottom, you are seeing something that seems very real and non-arbitrary. Social scientists work with very different materials that make straightforward realist positions simply unsustainable – which is not, of course, to say that there have not been periodic attempts to sustain them. The problem with the biological explanation of crime is that it attempts to cross from one kind of object to another without recognizing the need to confront the epistemological challenges that arise in the process.

The difficulties involved have been well recognized by social scientists since the differentiation of biology, psychology and sociology between 1880 and 1920. At the beginning of this period, Herbert Spencer could write authoritatively about all three. By the end, they are institutionally distinct disciplines, with their own research agendas, journals and networks of support and patronage. In the process of moving from the undifferentiated homogeneity of the sciences that had characterized scholarship from the revolution of the seventeenth century to the distinct heterogeneity that we recognize today, however, there was a willingness to engage in direct arguments that has since largely disappeared, for good institutional reasons (Abbott 2001). Nevertheless, as this discussion has shown, the result of such barriers is that distinguished biologists can invest considerable time, money and creative energy in research whose methodological problems could be exposed by an average undergraduate. The reasons for such investment are, of course, not purely intellectual ones. The possibility of such a misallocation of resources arises in part, though, from each discipline's neglect of the theoretical debates that contributed to their division during the early part of the twentieth century.

Physiological Psychology and Social Psychology

As Dingwall (2001) has pointed out, there is a certain anachronism in referring to work as 'symbolic interactionist' before 1937, when Blumer first coined the term. Nevertheless, it is not entirely unjustifiable since most of the key figures had some association with G.H. Mead or shared with him a common experience of graduate study in Germany, where an emerging discipline of psychology was being cross-fertilized by the proposals about the nature of mind and behavior emerging from pragmatism. WI Thomas (1896), for example, discusses the limited progress made by European 'psycho-physics'. He rejected the attempt to ground psychology in biological structures, like brain weight or cranial measurement, while retaining a notion of drive or instinct, particularly in relation to food and sex. The expression of these drives was, though, environmentally determined.

> It is a popular view that moral and cultural views and interests have superseded our animal instincts; but the cultural period is only a span in comparison with prehistoric times and the prehuman period of life, and it seems probable that types of psychic reaction were once for all developed and fixed; and while objects of attention and interest in different historical periods are different, we shall never get far away from the original types of stimulus and response. It is indeed a condition of normal life that we should not get too far away from them (Thomas 1901: 751).

This is an argument that would be quite familiar to to-day's evolutionary psychologists. For a period, there was a flurry of interest in trying to define human instincts as the drivers for behavior. McDougall (1909), for example, lists flight, repulsion, curiosity, pugnacity, subjection, self-assertion, the parental instinct, reproduction, gregariousness, acquisition and construction. Thomas, himself, elaborated his 'food and sex' instincts into his legendary 'four wishes' – recognition, response, new experience and security.

By 1921, Faris and Bernard were pointing to the dire confusion that had resulted[v]:

> How does it happen that gifted men are so unable to agree on what they consider the basic facts of human nature? Some slight differences might be understood, but surely the range is distressingly wide. One [instinct] or two, or four, or eleven, or sixteen, or thirty, or forty – this looks suspicious. (Faris 1921: 188)

Some part of this resulted from loose usage: Bernard (1921: 100-101) comments on the tendency of social scientists to use 'instinct' as a vague way of talking about habitual action, without distinguishing this from the genuine automatism of an inherited action pattern triggered by a specific stimulus. Faris notes that instinct has most usually been explained by 'the so-called genetic method' (p.198). By this he means a Lamarckian process, where previously advantageous behaviors are impressed on the human organism in an enduring fashion (see also Bernard 1921: 108). Two examples that he takes from a contemporary psychologist are the suggestion that the love of baseball reflects prehistoric man's need to run, throw and strike, while the former dependence of humans on horses is shown by the instinct of children to ride rocking horses. If one is talking about societal evolution, Lamarckianism is a more viable theory than in the case of biology: clearly social groups can study their competitors and seek to incorporate their behavior. However, the idea that this is then somehow fixed into physical human structures has all the problems that Darwin and Wallace identified. Several millennia of circumcision have not led to Jewish boys being born without foreskins. One could respecify the theory in more Darwinian terms. Boys who are successful at running, throwing and striking are advantaged in mate selection and reproduction, passing on skills that are then transferable to baseball. But, as Faris notes, such arguments are quickly falsified by ethnology. If we know that the human species has only a minor and relatively trivial degree of genetic variation, then either this gives rise to a high degree of uniformity of behavior or it has very little influence at all. Why would the same selection process result in baseball in the US, cricket in England and pelota in the Basque regions of Spain and France?

Moreover, Faris points out, our account of the selection process is entirely mythical. McDougall (1909: 282), for example, describes the 'primitive family' in terms that he borrows from the folklorist Andrew Lang:

> The primitive society was a polygamous family consisting of a patriarch, his wives and children. The young males, as they became full-grown, were driven out of the community by the patriarch who was jealous of all possible rivals to his marital privileges. They formed semi-independent bands hanging, perhaps, on the skirts of the family circle, from which they were jealously excluded. From time to time the young males would be brought by their sex impulse into deadly strife with the patriarch, and, when one of them succeeded in overcoming him, this one would take his place and rule in his stead.

No-one has ever observed such a society. This is simply a 'just-so' story. Faris goes on to present a very entertaining account of his six month old baby's 'instinct for toe-sucking' in terms of its advantage in recycling food dropped on cave floors! Frequently, he adds, these 'just-so' stories are also supported by highly selective examples from lower animals. 'Such naïve inventions based on a theory of evolution', he concludes, 'form no part of a valid scientific method' (Faris 1921: 193).

Faris acknowledges the role of instinct in animals and possibly in respect of simple acts by very young children. However, he sees no conclusive evidence that humans have any specific instinctive patterns. The 'genetic psychologist' assumes that which he or she should make a hypothesis. An instinct must be capable of universal expression. Ethnology, or as we would now say social anthropology, consistently falsifies any such claims. However, he does open an interesting possibility, namely the study of temperament. Where instinct deals with humans in the aggregate, temperament would deal with them on the basis of individual differences. He insists that temperament is as much a hypothesis as instinct but that it may be more profitable to pursue, even if only because it has received less investigation, at least as of 1921. We shall return to this suggestion.

Although an interest in biology continued among sociologists for a while after the attack by Faris and Bernard, there is no doubt that this topic went into a decline from which it has never fully recovered. A good index is the historiography of writing about George Herbert Mead, who has probably had the most enduring influence of the scholars working at the boundary between philosophy, psychology and sociology before the First World War. Mead himself had a considerable interest in the embodiment of humans. His first book (Mead 2001), apparently intended for publication in 1910 but never returned to the printer, devotes roughly a third of its length to a discussion of the field of social psychology and its relationship to physiological psychology. This remains a recurrent theme of the lecture course on Mind, Self and Society published by his students in 1934 after his death (Mead 1962). However, Mead's leading interpreters, Herbert Blumer (1969) and Anselm Strauss (1977) both discarded this dimension of his work. The recent rediscovery of the body as a topic in sociology has rarely led back to this agenda but has, rather, been caught up with the postmodern turn in microsociology which treats the body as a cultural artifact rather than as a topic in its own right. The core of Mead's social psychology is his explication of the basis on which acts acquire meaning.

Mead (1962) begins from a critique of JB Watson's (1925) behaviorism and Darwin's (1872) writing on emotions. Both, he argues, have misconceived the relationship between physical states and behavior in humans by overgeneralizing from studies of lower animals. Lower animals communicate in an automatic fashion by means of gestures and responses. Two dogs seeking to establish which is dominant will run through a fixed sequence of behaviors culminating in the withdrawal of one or the other, through an equally predictable display. Human communication is, however, selective and symbolic. We do not have an undifferentiated response to environmental stimuli. Mead refers to the emerging literature on the psychology of attention to support this claim.

(Although the nature of the text does not lend itself to citation, he would probably be thinking of work such as that of Bartlett, whose *Remembering* (1932) summarizes fifteen years of previous research and publication on the relationship between perception and recall, or Wundt's notion of vocal gesture.) Our responses to our environments are selected and organized through our ability to use symbols. Unlike animal communication, symbols provide for intervening processes between gesture and response and for the entry of the social into these processes. The most important symbols are those of language, which is a shared and collective experience: as Wittgenstein (1972) later emphasized, the notion of a private language is simply nonsensical. Language is intersubjective or it is nothing. The particular mental processes that Mead proposed may no longer justify much discussion. In many ways they are as much a 'just-so' story as the better-known Freudian trinity of ego, superego and id. However, his analysis of the centrality of language remains central. Because we cannot know what is in another person's mind, we can only infer this from their behavior and from the observation of their response to our inferences. The meaning of our actions is not to be found in our intentions – which are inaccessible – but in others' responses.

The development of conversation analysis since the 1960s, with the help of modern recording technologies, provides an empirical demonstration of what Mead could only contend, namely that, at its simplest, all face-to-face interaction rests on a three-turn structure. I say something; you respond to it; and I can then use the third turn to decide whether your response is adequate and adequately-connected to what I said first. In that turn, I can either decide to move on or rework (repair) my first turn and hope you will respond more satisfactorily or ask you to explain (account for) your failure to link your turn in second position to my first utterance. This is a dynamic structure: the second turn for me is the first turn for you so that you can examine what I do in the third turn, from my position, as a second turn for you – and then use your next turn as a third position to comment on what I have done.

<u>My sequence</u>	=	<u>Your sequence</u>
1. My Statement		
2. Your response	=	1 Your Statement
3. My Review	=	2. My Response
		3. Your Review

In reality, the process is often somewhat more complicated. Other parts can be inserted in the sequence and the third turn may be left empty if the speaker does not choose to use it. Nevertheless, this will make the basic point – that the meaning of my actions is not determined by me as their author but by the response of others and our subsequent negotiation.

This analysis is at the heart of the social scientists' difficulties with the idea of biological accounts of human behavior. The idea of a 'gene for violence' presupposes that we know what violence is. Violence is actually a label that observers apply to behavior as the outcome of their application of a set of ideas current in a culture and which they then respond to on the basis of that culture's notions about what to do about violent acts and how those notions might assemble into some idea of a 'violent person'. The argument has been pursued more fully in the context of addiction and drunkenness by Lindesmith and by MacAndrew and Edgerton. Lindesmith (1947) pointed out that opiate addiction required that a person recognize the connection between the withdrawal or unavailability of the substance and his or her negative physical sensations. Where opiates were administered for straightforward pain relief, under the conditions of his time, that connection was not made and one could not say that addiction had resulted. Clearly, a physiologist might identify modifications to that person's biological processes, which could lead to an investigation of the disruptive impact of the substance on their 'normal functioning'. However, there was no simple equivalence between those disruptions and the behavioral consequences of being recognized, by self or others, as 'addicted'. MacAndrew and Edgerton (1970) looked at the experience of introducing alcohol to the indigenous peoples of North America. They show that initially their response was one of puzzlement and a degree of disorientation. Alcohol consumption did not result in the acts that might have been expected if there were a simple relationship between physiology and behavior. These acts appeared after a period of time when Native Americans had been able to observe European behavior under intoxication and to formulate their experiences in a comparable way with comparable behavioral consequences. Becker (1953, 1967) used a similar approach to discuss responses to cannabis and to LSD, and the difference between the social experience of drugs knowingly consumed in a group environment and those that could be consumed unknowingly or in isolation. The ingestion of pharmacologically comparable substances does not lead to consistent and uniform behavioral effects in the way that biological determinism requires. The naïve user of drugs comes to learn what the experiences mean and how to act on the basis of them as a result of interaction with the sources of information and symbolic encoding available to them. These arguments can be extended further to consider 'normal body experiences' more generally. We learn how to be well and how to be sick (Dingwall 1976)

It is important, however, not to overstate this case. One of Mead's important contributions is his insistence on the materiality of embodiment.

> All social interrelations and interactions are rooted in a certain common socio-physiological endowment of every individual involved in them. (Mead 1962: 139n)

As Dingwall (1976) stresses, the ability to operate 'normally' as a member of a particular socio-cultural group depends upon the consistency of one's physical endowments and functioning with the requirements of membership. This is an important distinction from more recent constructionist arguments. The material world is not explained away or treated as indefinitely pliable. Even a post-modernist cannot play soccer with a broken leg.

Human Biology and the Social Sciences

What would it take, then, to reconcile biologists interested in the contributing to the explanation of human behavior with the social scientists who regard themselves as the experts in this field? Three elements are involved.

First, biologists must recognize that objects in the natural and social world are fundamentally different. The realism that they take as a self-evident part of molecular biology cannot be sustained in sociological research. A particular enzyme is tangible in a way that that, for example, a maltreatment index is not. Biologists, especially those working at the molecular level, therefore need a better understanding of environments. The sorts of generalizations that are being made on the basis of biological work are species-wide but are rarely subjected to adequate testing in relation to the diversity of environments under which our species is capable of flourishing. Those environments are not simply material, in the sense that an ecologist might recognize but are also cultural and symbolic. A generalization about the relationship between genetic polymorphisms and behavior needs to be sustainable across the varying cultural frameworks that contribute to the understanding of that behavior. Murder is not the same as ritually prescribed killing or an unknowing act of witchcraft, although all may involve a violent death. As Bernard (1921: 116) observed, the distinctive characteristic of our species is its adaptability.

> Man is able to dispense with instinct because he has a highly complex and well organized social environment, and in so far as this environment is improved and becomes more adequately organized to meet his present and future needs it dispenses with his instincts in the evolutionary process of selection or it represses and transforms them in the progressive character development of the individual.

Substitute genes for instincts and the argument retains its force.

The second (related) point is that biologists need a greater degree of specificity in the linkage between biology and behavior. The contrast between the precision with which genotypes and their physiological consequences are described and the looseness with which the social consequences are matched to them is striking. We have noted one example in the elision between 'unwanted touching of genitals' and 'touching of genitals' in Caspi *et al.* but this is not unique. More fundamentally, that paper makes a leap of inference from a set of indicators that suggest some people are more troublesome than others to be around in an Australasian context to claiming a potentially universal connection to violence. The latter, however, is a definition founded in local culture and applied in particular contexts by particular observers. It is a property ascribed to the behavior rather than inherent in it.

Third and finally, it is equally important for social scientists to take biology more seriously. The dismissive fashion in which it has been treated since World War II does not do justice to the scale and subtlety of the body of work involved. Both Mead and Cooley, who essentially sought to close the issue by asserting the parallelism of social and physiological psychology should be our guides:

> "Life, it appears, is all one great whole, a kinship, unified by a common descent and by common principles of existence; and our part in it will not be understood unless we can see, in a general way at least, how it is related to other parts. The stream of this life-history, whose sources are so remote and whose branchings so various, appear to flow in two rather distinct channels. Or perhaps we might better say there is a stream and a road running along the bank – two lines of transmission. The stream is heredity or animal transmission; the road is communication or social transmission. One flows though the germ-plasm; the other comes by way of language, intercourse, and education. The road is more recent than the stream: it is an improvement that did not exist at all in the earliest flow of animal life, but appears later as a vague trail alongside the stream, becomes more and more distinct and traveled and finally develops into an elaborate highway, supporting many kinds of vehicles and a traffic fully equal to that of the stream itself. (Cooley, 1922: 3. See also Mead 1909)

However, we believe that this argument has to be assessed carefully. We think, in particular, that Faris's comments on temperament are worthy of further reflection. It should not be a great point of contest for social scientists to accept that people's biological constitutions differ in ways that may have relevance to the material conditions for social interactions. If the Caspi *et al.* study were read as a study of genetic polymorphism and temperament, it might be rather more persuasive.

People may well have their neurological processes constructed and organized in ways that have a marginal influence on the speed with which they operate, their internal regulation and the retention and recall of information, to name but three possibilities. Put into social situations, any of these may have an impact on process and outcome. However, that is unlikely to be a simple linear effect. It will, at the very least, be affected by the biological material that they encounter in the form of other people and their temperaments, and by the symbolic resources shared by the participants that provide the raw material out of which each interprets and responds to the others' behavior. The modest study of temperament may, however, be much less exciting and fundable than the alluring prospect of a pharmacological fix for social deviance.

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[i] *Science* was the second-ranked 'multidisciplinary sciences' journal in the SSCI rankings for 2001, with an impact factor of 23.329. By comparison, the *American Sociological Review*, which was the highest-ranked sociology journal that year, has an impact factor of 2.767. While much of this difference reflects differences in the citation practices of social and natural sciences, it is a reasonable index of the importance of *Science* to its community.

[ii] This means that the gene can be present in varying forms leading in this case, either to the production of normal levels of MAOA or to a failure to produce it at all. MAOA is important in the breakdown of two neurotransmitters, serotonin and norepinephrine. Other research (Brunner et al. 1993) has suggested that the absence of MAOA is associated with a tendency towards violent behavior.

[iii] The different forms in which genes can occur are known as 'alleles'. In this context, metabolization refers to the breakdown and inactivation of the chemicals involved in transmitting messages within the brain once these have performed their function so that they do not accumulate.

[iv] Most of this material is not in the published paper but less accessibly presented on a linked website.

[v] Ellsworth Faris replaced WI Thomas on the Chicago faculty in 1918, teaching social psychology. Luther Bernard was awarded his PhD at Chicago in 1910 and continued a close association with scholars there throughout a rather itinerant career.