PERSONALITY AND HEREDITY

An introduction to psychogenetics

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PREFACE

One's early acquaintance with a subject is often much more intellectually hazardous than is realized – and this is particularly so in the case of students who must absorb information at a rate which makes reflection and the development of a critical attitude towards what one is learning extremely difficult. However, it is not only students who must take care: when first making the acquaintance of any new subject we are all equally vulnerable to the effects of 'potted accounts' and 'definitions' as they generally come at a stage when our critical resistance is at its lowest. Hopefully, we develop antibodies in due course but we are fortunate if we escape completely from the long-term effects of our early encounter. At least this has been my own experience as both an undergraduate and a lecturer in universities.

In my own case, the process of being introduced to psychology seemed completely harmless: the defining characteristics of content and acceptable methods of investigation were reasonable and temperate, if not particularly inspiring. But for years after graduation I still carried with me the narrow positivistic model which had been built upon the original definition. It took some time but, little by little, I began to realize how impoverished my approach towards psychology was, excluding as it did a whole gamut of what I should now regard as central issues – from unconscious processes to real-life social and political phenomena.

Early conditioning and the urge to be accepted and orthodox can hardly be overestimated, for, when one's own views begin to diverge from what has come to be seen as conventionally correct, there is a powerful tendency in most of us to feel that the fault must lie within – perhaps due to our having become too contaminated with worldly applications to remain scientifically pure. Absolute nonsense of course, yet the experience is commonplace enough, and many of us may never escape the effects of our training and orientation. Only with

the aid of a good measure of hindsight do most of us ever realize that the 'definitions' of evolving concepts in new subject areas can only ever be interim statements; conventions which must never be allowed to become crystallizations or limiting frameworks. From a teaching point of view, it may be helpful to set some provisional limits but it is at least as necessary to stress the evanescent and arbitrary nature of these conceptualizations as it is to emphasize the characteristics which currently seem appropriate.

Psychology is what psychologists do: not what particular psychologists define it as. Similarly, *psychogenetics* – or, as it is alternatively titled, *behavioural genetics* – is best provisionally thought of as what is being done by those attempting to relate our knowledge of psychology to our knowledge of genetics. Trite and unsatisfactory as this may seem in some respects, it is at least safer than accepting premature codifications as these, rather inevitably, will tend to mirror the personal interests, attitudes and prejudices of the particular rule-maker. Accordingly, in what follows, we shall attempt to set no such limitations beyond stating the obvious – that psychogenetics is concerned with the degree and manner in which behavioural and other psychological characteristics are genetically determined. For the rest, the variety of approaches which books such as this draw together should suggest the subject's present scope, as well as indicating something about its potential for growth and change. If my own introduction to psychology taught me anything, it was not to accept any 'definition' of subject matters and their methodologies – something which I hope will be apparent in what follows – though it is nigh impossible to avoid suggesting one's own perceived boundaries when making a selection such as this. Inevitably, some of my choices will seem eccentric to other workers in this field, though much that is more usual is here also.

At present the *cause célèbre* is the heritability of intelligence, and particularly whether there are any significant differences between genetic stocks as represented by race. The inheritance of mental illness is also a hotly debated issue, though it proceeds with less public fuss and more scientific decorum; certainly a great deal more than attends the question of whether psycho-sexual attributes are also under any degree of genetic control. But, polemic aside, this latter topic is still a most fruitful and compelling area – one which highlights very nicely the essence of many scientific arguments concerning possible mechanisms of psycho-biological differentiation and adds a great deal to the substance of all formulations concerning the inheritance of personality characteristics.

I have always found that a good antidote to over-much preoccupation with human behaviour is to spend some time watching, and reflecting upon, animals going about their business. The activity is not only relaxing and agreeable in itself but also helps in framing hypotheses and sharpening one's thoughts about human nature. Anyone who has spent much time looking at the behaviour of other creatures, particularly if they have been involved in the breeding of them, cannot help but notice how different their individual temperaments and intelligence may be from the very outset. Dog breeders are perhaps especially conscious of how temperament and, later, character may be affected by their selection of breeding stock. Rearing and training have their effects, too, but I have found that practical experience in animal breeding brings its own strong suggestion that psychological characteristics can, at least at the infra-human level, be substantially genetically determined.

For many people a biological investigation of mind and, worse still, the comparison of human beings with animals is a source of some distress and much distrust. Of course man is different from all other animals in innumerable important ways but, having said this, he is also a part of biological creation, whatever one's beliefs about the nature of mind, the presence of a human spirit, or the consequences of a unique social evolution. Unless we are to suppose that man did not evolve from a long infra-human ancestry but was the subject of a special creation, it is difficult to avoid the conclusion that his psychological characteristics, no less than his bodily ones, will conform to many of the same general laws as apply to other creatures. And, if this is so, reference to animal studies in the course of our investigation will often be appropriate – though the aptness of particular comparisons must always be subject to scrutiny. In the event, despite our focus upon human behavioural genetics, some animal studies will also be referred to as sources of comparison, of otherwise impossible experimentation, and of hypothesis.

To explore our biological properties is not to degrade our human status; only prejudice would do that. Some of our findings may not be palatable, they may not conform to our wishes or our philosophies, but mankind's greatest dignity lies not in inventing myths to cover ignorance and prejudgements but rather in discovering what we may of our own true nature and that of the creation we inhabit. Psychogenetics, though still very young and fragmentary, is one of the ways in which this aim is presently being pursued – sometimes with great success, sometimes with appalling results, but never without considerable zest, exhilaration, and relevance to us all.

Now to say something about the form and content of the book. It was conceived and written as a brief first introduction to the subject and, with this in mind, I have gone as far as I feel is prudent in reducing the conventional scholarly impedimenta in order to produce a relatively clear and uncomplicated account. The alternative would be to pepper the text with the names of authors, summaries of their data, and references to the particular experiments. Instead, I have chosen to stick to a policy of outlining what seem, to this observer at least, the most generally accepted conclusions current amongst those who are active in each of the problem areas with which we deal.

Only a few of the innumerable studies relating to each topic have been referred to directly but, where they have, and the source is a journal article, this is indicated by a footnote. In cases where particular pieces have been singled out for special attention, it is either because they exemplify a much larger body of work or opinion, or because they are of particular methodological interest. However, dwelling on individual studies has for the most part been kept to a minimum though, for those wishing to take their interest further, I have compiled what I hope will prove to be a useful guide to the literature in the section 'Bibliography and Suggested Reading'. In this, I have included volumes which contain the otherwise unreferenced research of workers named in the text and have selected other books which seemed to me most helpful in further elucidating and expanding upon each of the following chapters. Between them, they contain very extensive bibliographies of the more recondite or scholarly scientific papers which readers might wish to consult.

One final word on the use of this book. The order of chapters, though linked in some degree, is not such as to necessitate the reader following the same sequence. As far as is possible, given the nature of the material, each chapter attempts to deal with one broad aspect of normal or abnormal psychological functioning and is therefore fairly complete and independent of the others. So, should the reader be interested in only some of these topics, or be following a course of instruction in which they appear in a different order, no harm will be done in rearranging the order of reading this book. However, chapter two – which briefly introduces the mechanisms by which heredity occurs, and the ways in which it is studied within psychogenetics – is better read before proceeding to the specialized chapters.

When writing a book such as this, one must necessarily *select* the evidence from among the existing scientific literature, and no selection that could practically be made would satisfy all of the many points of view current in psychogenetics. With the best will in the world, it is

difficult (some may say impossible) not to choose material in order to satisfy one's own beliefs. Undeniably this is a real danger and the will to be objective is, alas, no guarantee of one's success. Anyone who has sufficient involvement with a subject to write on it at length can hardly be expected to be free of personal interests and opinions, so it would require a quite unusual degree of detachment not to shape and interpret material along certain favourite lines. And, with a subject-matter as controversial and as important as the fundamental springs of human behaviour, any reader would be well advised to look at as many sources as possible, always to question the interpretation of the data, and to see how convincing alternative interpretations might be.

Fact and opinion are always inextricably interwoven in human affairs – science not excepted – and it seems to me that the claim of complete objectivity is usually the mark of unreflective prejudice. I have attempted to minimize my own prejudices where I have been aware of them, whilst still indicating my own opinion, but I should judge the success of this book not so much on the degree to which I have convinced the reader but on the extent to which I have stimulated an interest for dipping more deeply into this fascinating new area of behavioural research.

ACKNOWLEDGEMENTS

I should like to express gratitude to some of the people who have made my own explorations more interesting and informed. At the very outset, I was fortunate enough to find mentors in my own city of Glasgow and am particularly indebted to my colleague, Dr Peter Hull of the Department of Biology in the University of Strathclyde, who accepted me as a 'student' on his genetics course; to Professor M. A. Ferguson Smith, head of the Medical Genetics Department at the Royal Hospital for Sick Children, whose staff were kind enough (and patient enough) to teach me the rudiments of cytogenetic technique; and to Mr Bryan Bett, of my own department, with whom I have shared many fruitful exchanges of knowledge.

Thanks too are due to Mrs Jean Cuthill for her secretarial support, and to the widening circle of people with whom one inevitably interacts during the course of research and writing. Their number has now grown to a point where it would be very difficult to pick out individuals but, nevertheless, I should like to reaffirm my gratitude for the many generous gifts of information, encouragement and comment received during the time that this book has been in preparation.

We are grateful to Routledge and Kegan Paul for permission to reproduce a diagram from H. J. Eysenck, *Crime and Personality*.

Chapter one

PERSPECTIVES AND HORIZONS

On 1 July 1858 an intellectual atom bomb exploded and annihilated civilized man's view of himself. Not that anyone would have been aware of this fact at the time: indeed, the very opposite was the case – the event passed off with hardly more than a conventional response to a thorough piece of work. The occasion was, of course, the presentation of Darwin and Wallace's joint paper *On the Tendencies of Species to Form Varieties; and on the Perpetuation of Varieties and Species by Natural Selection.*

Now the strange thing about this incident, at least from our present historical perspective, is that the response was not immediate. The fact is, though, that the ground had been so well prepared in scientific circles that the cogent statement of evolutionary theory was, at first, seen more as a technical achievement than as the epicentre of a philosophical, moral, and intellectual iconoclasm. The very fact that Darwin was presenting a joint paper was evidence of this. Alfred Russel Wallace, though hardly known of nowadays, had independently come to virtually the same conclusions as a result of his own biological researches in the Malayan archipelago. Ironically, as it turned out, Wallace communicated his ideas directly to Darwin – asking for a critique from this already distinguished scientist. If any bombs were heard to explode at this time, they must surely have seemed to come from the Darwin household where the great man's long-cherished theory had been pre-empted.

The historical record is quite clear that Charles Darwin had come to his evolutionary ideas many years previously – perhaps as many as twenty years earlier – but as the issue was so very much in the air it was obviously only a matter of time before it found convincing expression. The circumstances leading up to the presentation of the joint scientific paper and the publication of Darwin's *Origin of Species* in the following year are too intricate to summarize easily. There are,

however, a number of strands and milestones in this process of discovery which do stand out. For example, geological research had already shown that the biblical account of creation had problems: particularly in relation to the venerated statement that it took place as comparatively recently as 4004 BC. Moreover, the fossil remains embedded in the rocks revealed an amazing abundance of different species and many gradations of apparently *intermediate* forms.

Biologists too had observed how some species, which were separated by natural barriers and were exceedingly similar in most respects, differed especially in qualities which would have a distinct survival value in their own particular environmental conditions. Many such examples had earlier been recorded but Darwin had his own remarkable opportunity to make such observations at first hand in his capacity as a naturalist on HMS *Beagle*. This celebrated expedition, involving Darwin from 1831–36, had the remit of surveying the coasts and islands of many little known parts of the world, particularly in the southern hemisphere. He was especially interested in South American fossils and the living forms on the nearby, but isolated, Galápagos Archipelago. The variation in species which he observed in separated islands and slightly different habitats helped to lead Darwin, as comparable observations made on another archipelago led Wallace, to have doubts about the prevailing views on the fixed nature of species. For most people there was no problem: it was obvious that there was such a variety of similar species because God had arranged the creation so that each one should be perfectly adapted to its own environment. But the days of such a circular and doctrinaire viewpoint were now numbered – even if the notion was not due for a particularly rapid demise.

Of course, the science of genetics had yet to be born and although many people had useful practical skills in agriculture and stock-breeding, the principles underlying these manipulations were but dimly perceived as a 'blending' of the characteristics of the two parents. Selective breeding was known to be a way of varying the quality of the progeny but here the key to differentiation lay in the implementation of purposive matings engineered by the human will and intellect. How though, short of a divine plan, could one account for the progressive shifts taking place in the descendents of creatures living, and reproducing, within natural conditions?

The solution to this problem was suggested to both Darwin and Wallace on reading Malthus' *Essay on the Principle of Population*. Both realized that selection *does* operate in nature after all: the struggle for survival ensures that those with the better qualities for environmental

adaptation will indeed survive whilst those who are less well equipped will succumb and be erased from the breeding stock.

In effect then, evolutionary theory was implicitly proposing a system in which the universe was not governed by a beneficent deity unfolding his divine moral purpose in a world where everything had its ordained place. Instead, it was being suggested that all species, including mankind, were simply more or less complex life-form adaptations in the biological necessity to adapt or perish. Man himself must therefore be viewed as related to the animals, the very descendant and heir of primitive creatures. The religious-philosophical implications of Darwinism are too obvious to need elaboration and were soon grasped. Thus, a year after the sober scientific reception of the original paper, the first edition of *The Origin of Species* was sold out on the day of its publication and the hue and cry was on.

Darwin must have realized that a row was inevitable. Indeed, the thought of it probably accounted in large part for his delay in publishing. He was a nervous and retiring man, as well as a gentle one, and the prospect of arousing people and hurting their feelings through undermining cherished beliefs must have been very repugnant to him. But when he finally did take the plunge, as well as provoking the unavoidable animosity and pain, it was to add immeasurably to our understanding of ourselves and the world in which we live. Not least, he illuminated the importance of biological transmission in behavioural as well as physical adaptation.

But it was at this very point, the biological transmission of new adaptive characteristics, that Darwin's own great powers failed him. Most of the innumerable objections to evolutionary theory proved to be tractable, but it was at the level of the *genetic mechanisms* involved that the theory ran into real difficulty, the most damaging critic on this line of attack being a Scottish engineer, Fleeming Jenkin.

Jenkin's objection was two-fold. In the first case, he argued, new characteristics thrown up in individuals had virtually no chance of significantly affecting the nature of the breeding stock for, even allowing that viable variants occur, most species produce young in an abundance far beyond the possibilities of survival. The ecological balance is maintained where each breeding pair contrive to produce an average of about two replacement adults to perpetuate the species. But, as we know, individuals of many species produce thousands, or even millions, of eggs which are the food for other creatures in the biological skein. Inevitably then, Jenkin argued, the chances of sufficient and similar favourable variants surviving this wholesale predation so as to breed in sufficient quantity to affect the species

would be statistically exceedingly improbable unless such variants occurred much more frequently than the evidence suggested.

However, it was Jenkin's second objection which most truly found its mark and which was to have a surprising effect on both evolutionary theory and the still unborn science of genetics. The basis of the objection was that new variants would, through interbreeding, simply disappear in the vast reservoir of the species' normal characteristics. Jenkin offered the example of the white castaway who lands on a native island and marries several wives. The characteristics of this one white settler would, it was argued, simply be diluted and absorbed in the course of a few generations: the effect of one individual, however sexually active, would be far too slight to change permanently the skin colour of the islanders to yellow, let alone white. In the light of notions of heredity current at that time, the argument was very nearly a mortal blow to Darwin's ideas.

The reason why Jenkin's second objection was so powerful is, as we have mentioned, that it was then generally agreed that heredity was a matter of *blending* characteristics – rather like mixing different coloured paints. The solution to the evolutionary dilemma therefore had to be in terms of either destroying the blending theory itself or of finding some hereditary theory which could be reconciled with this process.

One approach to evolution which avoided this impasse was the older, and widely scientifically disreputable, theory of the French biologist Lamarck. He too had promulgated an evolutionary theory based on the inheritance of acquired characteristics. But the difference was that Lamarck's theory was based on parental physical adaptation to environmental circumstances being transmitted directly to the next generation. The theory held, for example, that giraffes had such long necks because reaching upwards for food led to a stretching, and gradual lengthening, of the neck which was passed on from generation to generation. Further, it was argued, the blacksmith's acquired muscles or the acrobat's acquired facility in balancing his body would be inherited by the next generation.

Ironically, Darwin, who had earlier dubbed the theory absurd, was himself ultimately forced by the weight of Jenkin's argument to produce a form of the same idea in order to preserve his own evolutionary theory.

Darwin termed his genetic process 'Pangenesis' – alluding to the hypothesized particles or 'pangenes' which were supposed to be produced in each part of the body and brought together, via the body fluids, as the genetic core of the egg or sperm cells. The pangenes

themselves were conceived of as the medium by which such acquired characteristics as powerfully developed muscles could be transmitted from parent to progeny. The theory met with the sort of opposition which it deserved – even from many of the most sturdy supporters of evolutionary theory. Scientifically speaking, none of the Lamarckian-type notions of genetics ever got off the ground, despite the backing of so eminent a figure as Darwin. Nevertheless, the underlying central concept was to prove much more durable than its purely scientific merits would suggest, for, in the twentieth century, it was also found to contain the seed of a socially and politically attractive ideology.

But, however unpalatable Darwin's supporters found his genetic theorizing, Darwin himself was obliged to defend his evolutionary theory as best he could until a more satisfying account of genetic transmission was available. And, curiously enough, the groundwork for this new science was already being laid – though in circumstances so far removed from the centre of urgent debate that it was overlooked completely until the beginning of this century. Indeed the circumstances could hardly have been more different for whereas Darwin was wealthy, academically distinguished, and concerning himself with matters unmistakably right at the core of man's view of the natural creation and of his humanity, the new iconoclast was quietly dabbling with 'plant hybridization'. This new Titan of science, born in obscurity and dying in it, was of course Gregor Mendel.

Mendel's origins were as humble as his calling – he was born into a Moravian peasant family and devoted his life to the church as an Augustinian monk. His occupation, until he became abbot of the monastery, was that of a teacher – though he failed to qualify for his teacher's certificate and performed very poorly in his biology examination. But, duties and bad examination results notwithstanding, he harnessed his very special combination of talents, interests and knowledge to become a creative experimental scientist, despite the many theological, intellectual and practical constraints that he had to accept in order to do any research at all. The critical experimental programme began in 1856 and its results were published in 1865 – still three years before Darwin had committed himself to a genetic theory which even he would probably have chosen never to be associated with.

Mendel's research took place in the gardens of the monastery where he cultivated, and artificially crossed, varieties of the common garden pea. Not a very dramatic set of circumstances considering the outcomes, but it later proved that he had been wonderfully fortunate in his choice of a subject for experiment. As it happens, the garden pea might have

been tailored for Mendel's purposes as the characteristics he took for study – such things as seed colour and shape, pod form, flower colour, and tall versus dwarf size – are each determined by a single gene. Had he originally selected an organism in which the genes controlling the selected variables were not simply and independently controlled, his results would almost certainly have been beyond his capacity to unravel.

The first stage in Mendel's experimental series was, of course, to establish stock plants which differed from one another in obvious ways yet which regularly reproduced themselves according to type. The garden pea, because it is normally self-pollinating, presents no special difficulties in this respect. Mendel then proceeded to interrupt the normal process of self-pollination by transferring pollen from plants which regularly presented a given characteristic to the pistil of plants which produced a differing characteristic in all their progeny. He then waited to count, and carefully record, how each member of the ensuing generation fared. These hybrids might, in turn, be crossed with one another to reveal more intricate interactions or, as Mendel did to establish his most fundamental principles of genetic transmission, members of the first hybrid strain were allowed to self-pollinate in order to show how far the progeny bred true to the parental type.

Mendel himself performed a large number of experiments on a range of characteristics but if for the moment we take just one of these as a typical example, say the crossing of tallness with dwarfness, the classic and most basic principles of genetics may easily be demonstrated.

When a tall pea plant is crossed with a dwarf, the result is always a tall plant of the same average stature as the tall strain itself. But if this first generation hybrid is left to pollinate itself, the result is not another crop of consistently tall plants: instead, it contains a mixture of approximately three-quarters typically tall and one-quarter typically dwarf varieties.

From results like these, which he repeated many times over and on seven different traits and many combinations, Mendel drew most, if not quite all, of the major conclusions which are now the axioms of modern genetics.

Of course, it was immediately obvious that the mechanism of inheritance does not work by a process of averaging or *blending* parental characteristics: the classic 3:1 ratio in the sort of experiment described indicated that much more interesting things were taking place in the reproductive cells and the fertilized egg. Mendel accounted for the observed ratio by reasoning backwards and postulating that

each characteristic must be controlled by one or other type of 'factor' and that, in the course of reproductive cells being formed, the genetic complement is halved and re-combination through fertilization results in a restoration of the original number. So Mendel had hit upon the idea of 'genes' – though he did not term them such – and he had also established the principle that these genes may carry different instructions which, whether they are acted upon in a given generation or not, are passed on unchanged. Thus also came the realization that genes must have the property of being either dominant or recessive and consequently have different chances of being expressed in an organism.

The discovery that many genetic possibilities exist within any given individual has since turned out to have considerable implications for evolutionary as well as genetic theory. It follows, for example, that a species' or breeding population's adaptations are not solely dependent upon mutations: the already existing actual and potential variability is of considerable significance because it may enable them to respond rapidly to changing environmental conditions, whether or not fortuitous mutations take place in the genetic structure of individuals. So, a potential variability, cherished in the unexpressed as well as the manifest genetic forms of individuals, is now recognized as one of the important ways in which a population ensures its survival.

In the next chapter we shall be taking a more detailed look at the mechanisms of heredity, but for the moment the important thing to note is that Mendel had successfully accounted for the fundamental nature of genetic transmission – and at a time when evolutionary theory was under very heavy pressure to show how such a phenomenon was possible. It is extremely unlikely that Darwin ever heard of Mendel's experiments – they were promulgated in obscure circumstances and the statistical treatment of 'natural history' was generally to prove both intellectually and temperamentally repellant to biologists for many years to come. On the other hand, Mendel did know of Darwin's evolutionary theory but whether he was able to connect the two, or whether his calling forbade even the attempt, we are never likely to know.

What we *do* know is that Mendel himself never seems to have extended or generalized his genetic ideas very far beyond his own chosen field of scientific horticulture. Despite the universal applicability that his principles were later found to have, and the scorching heat which they have since often generated when applied to human beings, the gentle monk appears never to have strayed far beyond the domestic scale of things.

Perhaps, though, it was fortunate for Mendel the pious priest that his other experiments in heredity were less successful when he followed advice to broaden the base of his experimentation by working with other organisms. One of these, hawkweed, proved to have a more complex mode of transmission than his peas – though it was no exception to the basic rules – but Mendel was not able to unravel the processes involved and demonstrate the generality of his discovery.

However, he did briefly apply his genetic ideas to the animal kingdom, to another of his interests, apiary. But the bees which he bred, though excellent honey-producers, were so ferocious that they presented a quite unacceptable hazard to the neighbourhood and led to a swift end to this line of research!

Mendel still had useful years of life ahead of him in which to serve his calling, but he died without ever knowing that he was spearheading a powerful new branch of science – one which would, like Darwin's, change man's entire vision of himself. Not until 1900, sixteen years after Mendel's death, was his work retrieved from obscurity – and this because his principles were re-discovered by three different workers, in three different countries, none of whom had previously known of the 1865 publication. The time was apparently now ripe for biology to grasp the importance of Mendel's principles.

But, once re-discovered, theorizing and research on all manner of living creatures grew apace: soon genetics revolutionized biology itself and established the basis for evolutionary theory. Fleeming Jenkin had been answered: inheritance does not take place through a process of 'blending', like mixing paint. Had this been the case, siblings would all be the same in height and all other physical characteristics and would fall at a point intermediate between their two parents. Instead, inheritance among people turned out to be a game of chance in which unsuspected genes could skip unchanged through generations, being made manifest only in particular combinations and permutations of the material derived from the maternal and paternal parent.

But, if it seemed that the scientific battle to understand the processes of heredity had finally been won, it soon transpired that more surprises were in store, not just in the sense of new discoveries within the broad Mendelian framework but involving complete changes in direction. The one which is most famous, or notorious, is 'Michurnism' – the scientific and political vehicle of the Russian, Trofim Lysenko – a conceptualization which turns out to have a long history.

In their own day, the lives and thoughts of Darwin and Marx brushed past one another with the revolutionist Marx unacceptably

offering to dedicate *Das Kapital* to the evolutionist Darwin. But, paradoxically enough, long after the death of both they were to be drawn into the same political ideology by the apparent Soviet need to see direction, growth and progress in evolution. Many strands account for the success of Lysenko's formulation of genetic theory, but one of his sources of strength lay in Darwin's expression of the Lamarkian-type doctrine that environmentally-induced changes in parental constitutions are transmitted to their progeny.

'Michurnism' was conceived during the agricultural crises of the 1930s and based on some ill-controlled experiments by the horticulturalist Michurin. Lysenko took up this work and promised Stalin not only more abundant food supplies but also an ideologically more attractive form of science. In return, Lysenko achieved a dominating position in Soviet biology and a swift, and often final, rejoinder to his critics. The war caused a sufficient degree of chaos and destruction to preserve Lysenko from the sharpest tests of his crop-breeding theories but, inevitably, he proved unable to produce the goods and, after the death of Stalin in 1953, Lysenko lost his grip and more orthodox theory was reinstated.

While it lasted, Lysenko's central dogma asserted 'the unity of the organism with its environment'. His use of this unexceptionable concept was elusive but, in plant terms, it was justified by such claims as that wheat could beget rye if the former was raised under appropriate environmental conditions. As critics of this view said at the time, we might also deduce that men could beget monkeys if subjected to the appropriate environmental circumstances! Lysenko argued that not only is the environmental shaping of the parent passed on to the progeny but that chromosomes are of no special significance in heredity, and that genes do not exist.

If these ideas now seem astonishingly naive and wrongheaded, it is as well to bear in mind that Lysenko died only in 1976 and that his propositions were taken seriously by many people until a very short while ago. However, the incident has some cautionary value, being a very nearly perfect illustration of the absurdities which may follow when the attempt is made to squeeze science into an ideological mould. Unfortunately though, its ramifications did not stop short of tragedy and one of the most poignant examples of those people who were trapped by the conflict of their political beliefs and their scientific integrity is provided by the case of J. B. S. Haldane. Being a distinguished geneticist himself – as well as a Marxist, popularizer of science, and editor of the communist *Daily Worker* – Haldane could not ignore the Lysenko affair and the things which were going on in

Russia. During the war, he wrote a number of articles, later collected together in book form, among which is an essay on Soviet genetics.* Considering Haldane's tremendous gifts, and his great integrity in so many admirable ways, this essay must surely rank with some of history's more pathetic pieces of chauvinism. In it, Haldane contrives to avoid the whole scientific point of the Lysenko controversy and simply weaves his words around the subject of the excellence of Russian science in other respects and in other fields whilst seeking reciprocal lines of criticism for Western genetics.

Even the arrests, dismissals and probable executions of prominent scientists brought no more from Haldane than an account of cases where people in the West had been dismissed for petty reasons and an assertion that dialectical materialism is only a method, *not* a dogma to which Soviet scientists must conform. This in the face of his colleagues' struggles for survival. True enough, Haldane ultimately became sickened by his attempts to reconcile scientific truth with politics, but the damage to himself and others had already been done.

Lysenko's great rival of the time was Nikolai Ivanovich Vavilov, a prominent theoretical geneticist and specialist on the origin of plant species. The attack of 'formal genetics' which Lysenko so powerfully led against him and others was welcomed by Haldane who wished that all such scientific controversies were pursued in the same public manner, and averred that he preferred the 'democratic Soviet method' of settling policy issues, rather than the English method of settling things over private dinner parties. Finally, Haldane wished that his European and American colleagues would transfer the energies that they were expending in defending Vavilov, who Haldane said was quite capable of looking after himself, and apply them to the genetical theories of racial inequality which were becoming current not only in Germany but also in Britain and the USA. Vavilov lost his arguments on genetics and died in a Siberian prison in 1943.

Of course Darwin, Mendel, Vavilov, and others since have not been regarded as such dangerous influences for what they proclaimed about speciation or horticulture: their threat lay in what their ideas seemed to imply about the nature of mankind. As Haldane points out, racial comparisons certainly can be invidious but then so too, whether made in the East or the West, can be the proposition that men are not created fundamentally alike. Or rather, that they may vary considerably in their genetically determined potential and that education and conditions of life are not sufficient to overcome inborn 'inequalities'.

* In *Science Advances*, pp. 220–6. Allen & Unwin, 1949.

The notion that some people are inherently 'superior' to others, though of wide historical currency, has been called more and more into question as society liberates itself from feudal repressions and moves towards the modern democratic state. For example, eighty-two years before *The Origin of Species* was published, on 4 July 1776, the (then) thirteen United States of America had issued their 'Declaration of Independence' – asserting certain basic principles related to human dignity and freedom. This document, in its second sentence, reads:

> We hold these truths to be self-evident, that all men are created equal, that they are endowed by their Creator with certain inalienable Rights, that amongst these are Life, Liberty and the pursuit of Happiness.

And here's the rub: in what sense are we to agree that 'all men are created equal'? If we are to take the quotation as a whole, we might suppose that it means that all men have equal rights to life, liberty and the pursuit of happiness. But this interpretation is by no means self-evident to everyone and many take it to mean that all of us, at birth, start life with the same potential: almost that 'anyone can do anything' if he is given the right opportunities. This philosophy, which has so much political and humanitarian appeal, is particularly attractive because it is so optimistic in its implications, promising at least the possibility of ultimate utopian perfection.

The fact is though that people are created *different*, and each one uniquely so. Whether they are created equal is another matter again, and one which can only be established by defining the sense in which we are using the word 'equal'. If we are speaking of equal rights under the law, or of equality before God, then there would be relatively little dissent. If we are to speak of equality of opportunity, then we already invoke the question of the significance of undeniable individual differences, for it is arguable whether the person with no musical or artistic talent, and perhaps little interest or dedication, may demand the same opportunities and facilities as the rising musician or painter. The same might be argued in every intellectual or occupational sphere. But then we are faced with the moral problem of whether the 'gifted' should have special facilities and indeed, by the same token, whether the impaired or retarded have any such special claims either.

Some people, however, would argue that this is only a pseudo-problem anyway as all *are* created equal and only differ in respect of their opportunities and fortune. But, having asserted that some people do seem to accept this position, it could always be argued that this point of view only realistically reflects a generalized opinion which has certain exceptions. Indeed, the thoroughgoing proposition that all

men are created equal is so patently wrong that it may only be a cock-shy: an absurd argument set up by people who disagree with the spirit of the formulation and who do so only in order to knock it down and thus give the impression that they have totally discredited the whole thing.

And surely no one but the most prejudiced could suppose for a moment that all men are created equal in capacities and potential. We have all come across mongol children and those born blind, deaf, crippled, or otherwise impaired. It would be the greatest act of self-deceit to suppose that each child was equally capable of becoming a theoretical physicist, a musician, or an athlete – however comparable, or even special, the opportunities provided.

The fact that all people are not created equal, in the sense of being equally able to achieve the same ends, is surely not in doubt: particularly where these ends are affected by observable handicaps due to physique or the sense organs. The dwarf will never become a champion long-jumper any more than a blind man is likely to excel in slalom events. But when we get to performances which are manifestations of less directly observable capacities, for example intelligence, and where the people concerned are not obviously incapacitated, we tend to become less open to the idea of genetic and congenital differences.

However, it is one of the charms of the subject that the question of how far psychological characteristics are inherited is not only fascinating at a technical level but it is also a compulsively interesting wider issue because of the vast range of social and political matters which any such enquiry also uncovers. And when technical knowledge about the mechanisms of heredity is of a rudimentary sort, as it is for most of us, we find that the subject is often (at best) hotly debated at the scientific level of the 'flat earth' hypothesis. In this particular case though, the arguments all too frequently seem to revolve around another mystically endowed substance – *blood.*

For example, we still hear a good deal about the significance of having this or that country or race's 'blood' in our veins. Families often feel themselves united by sharing the same blood – 'blood relatives' – whilst others may be held to have 'bad blood', 'tainted blood', or whatever: a notion which can then be used to describe, predict, or explain personalities. Of course, most of those people who bandy such ideas may well realize that the fluid itself is not really responsible for defining our biological characteristics but, nevertheless, blood is still often a powerful feature in conceiving the semi-mystical unity of families, tribes and nations. Bunk, but troublesome and dangerous

bunk, as it so frequently also carries additional implications of exclusiveness and purity.

Of course, there *are* physically-transmitted characteristics which serve to unite groups through their presence but, as we shall see, these are *human* characteristics which are common to our species and know no such sharp conceptual bounds as the family or the nation. Even qualities like skin pigmentation, whose surface effects are so obvious that they have often been used to divide humanity, are only miniscular differences in the context of our similarities. If evolutionary theory is right, we are all members of the same family anyway, sharing the same (albeit remote) ancestors whether we come from Liverpool or Lagos, New York or New Guinea, Paris or Peking. The drifting apart of continents and migrations prompted by successive ice ages, famines or overpopulation have each helped promote these geographical, and thus physical, divisions of mankind.

We may fight, kill, abuse or despise one another, but we may as well get used to the idea that we are biologically one true family and that what we have in common with one another is much greater than the things which we use to divide us.

Race is, of course, one of the great bedevilling problems of our time: or rather, it is something of which we still contrive to make a problem. That this should have occurred historically is not surprising, as our light-skinned predecessors, mainly finding technologically more rudimentary cultures among the dark-skinned inhabitants of our world, proceded to judge them on this – and on their 'heathen' beliefs. The subsequent domination or enslavement of these peoples, not surprisingly, led to a feeling that the one was superior to the other. Indeed, the subordinate role which most dark-skinned races were obliged to play in order to survive must have added to the impression created. Further, in a world in which the Church taught the fixed order of things (and people), this valuation crystallized and, as the subject-races had few opportunities for self improvement or social evolution, the prognostications must have seemed to have come true as the gap between the light- and dark-skinned races continued to widen.

For the most part the architects of these injustices and their victims have long since departed this earth, but the problems have been left behind and the working out of new relationships has led society to re-state the questions relating to how fundamental are the differences between peoples. Unfortunately, mankind's record – black, white, and yellow – seems to be universally bad whenever it has been possible to conceive differences between one group and members of another. People, and peoples, differ from one another. The challenge of

civilized man is not to deny the fact but to understand the reasons why they differ and then to be prepared to offer, not prescribe, whatever remedies may be needed in those cases where a difference implies a disability.

Objectively, certain individuals, cultures, and races have trouble surviving in our evolving forms of society. Many of their problems are simply economic, cultural or educational but it has been proposed that genetic factors are also at work. When this involves those intellectual capacities which are measured by IQ tests and which correlate so well with educational and occupational success, then clearly issues of a very practical consequence are being raised. This is a matter to which we shall return again in chapter three, when we shall focus our complete attention upon the evidence regarding the heritability of intelligence. As we shall see, intelligence test measures are only one small (though spectacular) facet of the problem. People differ in so many different ways, even when they are of the same sex, age, culture, family background, and so on. But, given that important psychological differences do occur between individuals and groups, the question then becomes one of establishing how far genetic theories can account for them.

Genetics, can, of course, supply many clues but we should never forget that heredity, or that which the new generation inherits from its predecessors, is more than just the genetic package. Additionally, there is the prenatal environment, the postnatal physical environment and, most significantly, the cultural and social milieu which interacts with these and our biological nature to produce a more or less unique product in every case. We shall trace these interactions in what follows but it is quite obvious that an important part of our inheritance comes in the form of libraries and other such verbal and behavioural codes as religious beliefs, legends, myths and customs. Indeed, so obvious is this form of inheritance that many psychologists have been tempted to suppose that it was the only one of any significance.

Perhaps because early proto-psychology emerged primarily from philosophy rather than biology, the whole discipline has tended to give an almost total primacy to the social aspects of our beings. In this, the philosophy of such empiricists as John Locke in the seventeenth century seem to have been particularly significant. Locke's notion of the newborn child's mind as a *tabula rasa*, or a blank slate ready to be written on by experience, has proved to be as misleading as it was elegant, popular and apparently self-evident. Also in the same mould was the work of J. B. Watson, the American psychologist and founding father of behaviourism, who stated that, given a child and the

techniques of conditioning at his disposal, he could produce just whatever sort of person was desired. He never did, of course.

But, in the process of redressing the balance and drawing attention to the genetic components of our mental development, ideological issues have again been raised which have made dispassionate restructuring of our beliefs much more difficult. It is, for example, extremely unpopular in some quarters to suggest that any mental differences between people are genetically determined – even though the objector cannot avoid accepting the principle in relation to other structural bodily characteristics, or extreme psychological differences such as those involved in mongolism.

However, if people are sensitive to the assertion that there are marked genetic differences between both individuals and races, it is perhaps because experience has taught many of us that such propositions can be, and often still are, the justification for privilege, discrimination and persecution. Countries like Britain have suffered very obviously, and in much the same way as many others, from the discriminations which we make on the basis of our (sometimes astonishingly remote) origins. The maintenance of valued cultural differences may, of course, be a very constructive and high-minded activity but it is also the case that the various regional groups which inhabit our islands are also easy prey to the notion of special 'blood'.

Differences between national groups there certainly are, though these may usually be traced back to the cultural divergencies which arise from different geographical, occupational and political circumstances. 'Nations' are, of course, pure abstractions – and most of them fairly recent ones at that. The British have long suffered from these regional 'racial' problems but it is surely the case of the Jews that gives us the best example of how easily intellectual and moral prejudices may be justified and rationalized with bogus genetics.

Few of us, if any, will need reminding of the Nazis' 'research' into racial differences. 'Master race' delusions aside, it was in its discriminatory implications that we saw the most terrible face of misplaced ideological zeal. The 'taint' of Jewish 'blood' became a criterion for murder, the possession of non-Aryan a source of inferiority, and the passion for 'purity' became a rationale for homicide, infanticide and genocide. Quite a record for any science to live down.

Naturally, the science itself was not the culprit: it would be equally absurd to blame a stone and not the murderer for battering someone to death. Corrupt men, some of them scientists, were the malefactors in this case but, just as we have controls to regulate the use of dangerous

objects and materials, some people are now arguing that similar restraints should be applied to genetic research into racial differences. And, were I a Jew or negro, I am sure that I should have a good deal more sympathy for this point of view – just in case new weapons or fetters were being forged for me and my family.

The present controversy about the relative intelligence of 'caucasians' and 'negroes' is a case in point. This particular issue is one which we shall be looking at in more detail later but, as is very apparent, it is an area where emotion and prejudice are again creating intellectual and physical hazards. However, at the present time it seems that it is the researchers themselves rather than the minority group involved who are likely to suffer direct attack. The fact that investigators argue for the social value of their work as a way of establishing the special social and intellectual needs of the disadvantaged minority seldom cuts any ice. American negroes apparently feel firstly that there *are no* genetic differences anyway, and secondly, that their need is much greater for equal acceptance and the establishment of a solid basis for self-regard than the special facilities which carry the implication of basic inferiority.

The physical attacks made upon scientists working in this area are, it goes without saying, completely inexcusable, but even a moment's reflection should convince most of us who are white that any research which apparently purported to show *our* inferiority to any other race would be likely to be received by us not only with apprehension, disbelief and ridicule, but also with aggression. That these reactions would be magnified by many times if the research had been undertaken by members of a different and more dominating group, also seems equally likely to be the case. Genetic studies of racial characteristics are so obviously likely to provoke social tensions that it is often argued along ethical, humanitarian and plain practical grounds that racial differences with a presumed genetic basis should simply not be studied – unless they involve matters which could not possibly be interpreted in terms of the superiority or inferiority of either of the groups being compared.

A similar argument might also be advanced in relation to individuals drawn from the *same* national and racial group – for example, IQ comparisons of the children of working class and upper class parents. In fact, it is also often argued that there are areas of the human sciences which simply should not be explored at all because of the effect that any subsequent revelations (and their misrepresentations) might have upon society.

In the past, this principle has been mainly, though not exclusively,

the prerogative of the Church. Astronomy and human anatomy are among the most celebrated of innumerable possible examples of the Church 'protecting' its members (and its own interests and ideology) from disturbing truths. But, had we not rejected the criticisms and prohibitions which were levelled at every stage in our emancipation from a more primitive world view, we should no doubt still be living in it. Even so, if we are to reject the notion that there are forbidden areas of investigation, scientists must somehow learn to become a good deal more socially sensitive and aware of the ways in which their work may be used or distorted to affect other people's lives. Sadly enough though, public misrepresentation is an art (and sometimes a profession) in itself and neither the scientist nor the private citizen can do a great deal to curb it. However, caution and realism should obviously be at the core of any research which has to do with human relations and people's perceptions of themselves and others.

The issue of racial differences, though one of the most hotly debated areas of human behavioural genetics, is not a central issue of the discipline – nor is it of this book. Racial comparisons, though potentially extremely interesting from a genetic point of view, are still proving virtually impossible to pursue in a rigorous scientific way because of the almost insuperable problems involved in partialling out the effects of quite disparate cultural traditions and social and economic circumstances. Additionally, there are daunting technical problems in undertaking comparative psychological investigations when the groups for comparison are divided by profound psycho-linguistic differences. As a result, such research as has been completed tends to be somewhat sparse and, because it seldom achieves a satisfactory degree of scientific rigour, should be given most careful critical attention.

However, before attempting to assess the scientific credentials of any particular pieces of work, it is perhaps first desirable to look more closely at both the underlying genetic principles and at the experimental techniques which are typically employed to investigate human behavioural genetics.

Chapter two
MECHANISMS AND METHODOLOGIES

In the last chapter we began to discuss the significance of Mendel's 'hybridization' observations, and the interpretations which he put on them. And, though it may seem a very long way from horticultural experimentation to developing an understanding of man's fundamental nature, subsequent work has shown that they *are* linked through the sharing of common mechanisms of cell reproduction. Indeed, recent work has made it seem probable that all living things, however primitive, have their appearance and activity organized by the same chemical material – DNA, or deoxyribonucleic acid.

So celebrated has been the work of Crick and Watson leading up to the molecular analysis of DNA that few of us nowadays can help but know something of its intricate helical structure. However, this sort of knowledge is by no means necessary for our present purposes: it is quite sufficient simply to know that DNA is the basic material of the genes and chromosomes and that it is, so to speak, the 'data tape' which carries genetic instructions.

In actuality, the 'data tape' of human beings and all sexually reproducing life forms is really two sets of tapes: one derived from the male parent, and the other from the female parent. In mankind, our tapes are both complex and numerous – encoded upon forty-six chromosomes, of which twenty-three come from each parent. Our individual life history may be said to begin only when the half-sets contained in sperm and egg cells are brought together in the fertilized egg. There, the chromosome complement is restored to the normal number for all human somatic cells and the resultant new cell has the full set of plans necessary to commence its development. And it is from this single new cell that must come the orders from which a human being may be assembled and programmed: maternal nutrition and other biological support systems are, of course, necessary but the instructions for producing and regulating all the incredibly complex

biochemical machinery of a human being must be present in the cell from the beginning.

In going through its intra-uterine developmental sequence, the newly fertilized egg will, as every schoolboy knows, grow and divide into two, four, eight, sixteen and so on until the number of cells can only be estimated in uncounted millions. Well, this is a commonplace idea and no special problems in comprehending it are encountered if, say, we are considering the replication of single identical cells, as in the reproduction of amoebae. But when we think of the building of complicated creatures like ourselves it does evoke some startling thoughts.

First of all, there is the currently enigmatic problem of how these dividing cells 'know' when and where to differentiate themselves into specialized structures such as nerve, liver, bone, muscle or blood cells. This particular branch of developmental biology would, alas, carry us far from our present interest but it does raise issues concerning the nature and behaviour of the genetic materials. The surprising thing is that the nucleus of every single somatic cell in an individual's body contains exactly identical genetic 'tapes', however different the appearance or function of the cell. The genetic instructions stored within the chromosomes of brain cells are just the same as those in the skin which surrounds the big toe. The difference lies in which of the genes are currently 'switched on' in order to serve a specialized purpose. In many cases animal experiments have shown that cells which serve a particular purpose in a given part of an animal may, through transplantation, change and adapt to their new circumstances so that cells which once constituted skin tissue may become the fabric of an eye. Normally speaking though, cells specialize early and remain as they are, even though the most humble among them contain encyclopaedic information relating to our anatomy and physiology, and perhaps also to our psychology. Unless mutations occur, the chromosomes are, through a process known as *mitosis*, replicated exactly at every cell division. The only exception to this rule is in relation to the forming of egg and sperm cells where, through a complex reduction of genetic material in a process known as *meiosis*, half-sets of chromosomal materials are prepared.

The actual process of meiosis is somewhat different in the male and the female but essentially similar in that the *gamete cells* which are formed, sperm or egg, contain a new and unique set of developmental instructions. In the course of their formation chromosomes, which the person now producing gametes has inherited from his or her parents of each sex, pair together before being randomly separated during the

process of meiotic division. The twenty-three single chromosomes thus produced may, for example, contain ten of the original paternal chromosomes and thirteen of the original maternal – or any combination ranging from all paternally to all maternally derived.

So, the number of possible combinations of the original paternal chromosomes in any given gamete allows for 2^{23} possibilities – or, nearly eight and a half million alternative combinations. When a male and a female producing gamete cells mate, and chance has again played its part in determining which combinations shall come together to form the *zygote*, the fertilized egg, this finally represents just one out of the seventy trillion combinations of genetic possibilities which might have occurred as a result of that single mating. Thus each individual so produced is unique in a very real sense.

Actually, this degree of uniqueness is even greater for, while the processes of meiotic division take place in the formation of the egg or sperm cells, there also frequently occurs an exchange, or *crossing-over*, of parts of the genetic materials on the maternally and paternally derived chromosomes. And, as some genes are associated with one another in *linkage groups*, unless some degree of crossing-over did occur to break up these patterns, certain gene-controlled characteristics would always appear in association with one another. As it is, this exchange of genetic material occurring in meiosis ensures a much greater variability in organisms than would otherwise occur.

To this enormous recipe for potential variation at least one other ingredient might be added at this stage – the possibility of *mutations* occurring. Mutations, or spontaneous changes taking place within chromosomes or genes, are perfectly natural phenomena. Relatively rare though they are, they are nevertheless very significant events as they add to a species' possibilities for evolutionary progress or continuity should prevailing environmental circumstances make survival of the typical form difficult or impossible. However, organisms are generally so finely balanced in their functioning and adaptations that most mutations are, if not actually lethal, then detrimental and affect survival span and reproductive potential. Thus they tend to be selected out. Even so, many are tolerated by the organism, particularly as they are most commonly *recessive* and therefore not detrimental to the organism carrying them. Unfortunately though, they may later produce serious difficulties for their own progeny.

The zygote, having been formed from the half-sets of data tapes derived from each parent, must now implement some sort of selective procedure to determine which instructions are to be followed. Were it

the case that both parental sets contained only half of the information necessary, and that when these were combined they would equally contribute to the final formula, then the whole thing would be clear and simple. But both sets of the chromosome pairs contain very comprehensive specifications and, as we have seen, many of the gene forms are likely to be very different.

We have also seen that genes do not 'blend' into averages of their two qualities – rather, one gene will normally be dominant over the other when they are programmed for alternative characteristics. When two complementary genes in a chromosome pair carry the same instructions they are said to be *homozygous*; when they contain different instructions they are termed *heterozygous* and it is then that the question is raised as to which set of instructions should be obeyed. These alternative forms of corresponding genes are termed *alleles* and, of course, any particular gene may have many alternative allelic forms, Mendel's classic experiments in the hybridization of tall and dwarf peas having, from the very beginning of genetic studies, clearly demonstrated that some allelic forms are dominant and some are recessive.

In other words, the instructions for some characteristics, being dominant, are acted upon whenever they appear, whereas recessive allelic forms will only become manifest when both genes carry the same message. But even when the recessive form fails to be manifested in a given individual, the genes which carry them are themselves unaffected and they may be transmitted intact to the next generation – where their prospect of making a physical appearance will again depend upon whether or not they combine with another gamete bearing similar instructions. Not surprisingly, it is often a cause of great suspicion and disbelief that a marked physical characteristic in a child may suddenly appear in a family where neither parent knows of its earlier manifestation among their predecessors!

There are rare exceptions to the simple dominant-recessive dichotomy: for example, some plants will produce intermediate pink colour forms when red and white varieties are crossed, so we know that dominance may be a relative feature rather than an all-or-nothing phenomenon. In human beings this may yet prove to be of some importance but in *apparently* analogous situations – such as interbreeding between black- and white-skinned races, basically different processes are generally at work. Interbreeding of this sort certainly suggests the blending of two genes to produce the intermediate colour forms which we observe, and certainly we never see the progeny of such unions turning out as calculable ratios of one

colour to the other as we would in, say, the interbreeding of pure-bred Aberdeen Angus and Redpoll cattle.

In this latter case we observe the dominance of the black colour allele in the production of all black calves in their progeny. Subsequently, though, interbreeding of these calves will allow for the reappearance of red cattle in those cases where the recessive alleles have been paired together. The ratio of this reappearance is itself quite predictable as, on a probability basis, we may expect that there will be roughly equal pairings of the two alleles. So, if we let *X* stand for the dominant form and *x* for the recessive, and if we record the possible pairings sequentially according to the male and female contribution, we shall derive the combinations *XX*, *xX*, *Xx*, *xx*. Thus, in cases of single-gene or single-factor inheritance, a recessive characteristic may yet appear in the progeny – in the present example, with the heterogenous recessive combination of *xx*. As may be seen from the possible combinations, their number should not significantly depart from an incidence of 25 per cent, or the classic ratio of 1:3, unless selective breeding policies or preferences are involved. However, in human beings, qualities such as skin colour or height, though often conceptualized as discrete characteristics, are commonly complex compounds mediated by a number of genes. In fact, the one-gene: one-characteristic relationship is an extreme rarity in human physical attributes and is probably rarer still in processes which affect behavioural determination. Fortunately, experimental evidence with simpler organisms, and those complex ones which may be subjected to controlled breeding experiments, has yielded a very clear picture of the genetic processes involved in the transmission of many multiple-gene, or *polygenically* controlled characteristics.

Presumably, and hopefully, it will never be practicable to arrange the systematic interbreeding of parents with their children, brothers with their sisters, cousins with each other, grandparents with grandchildren, and so on which is so necessary where intricate hereditary patterns must be delicately traced through constant comparison of results. So, though we may have no reason to doubt that the same mechanical rules of genetics apply with equal force to human beings, we must rely for the most part upon inferences from non-human research, and upon a range of less than optimal alternative research strategies, in order to account for our own observable differences.

Alas, though, even in simple organisms, genetic principles alone are seldom sufficient to account for particular outcomes. Though, in one sense, *all* behaviour is genetically controlled – in that tissues could not be formed, nor could they function, without genetic direction – it is

another thing again to conclude that the form and direction of the behavioural acts and developmental sequences is predetermined in any meaningful sense. After all, genes may be a *necessary* cause of a given effect but they are never a *sufficient* cause. This is because of the generalized nature of the genetic message no less than the total interdependence of genes, cellular tissue and materials, and an environment which may be more or less favourable to the expression of any given genetic message. Thus, despite the fact that it is the DNA alone that carries the instructions which will, under normal circumstances, be interpreted in structural and functional terms, nevertheless environmental conditions – both pre-natal and post-natal – are always highly variable. The same genetic material may, under different environmental circumstances, express itself in different ways.

For example, the genes might contain instructions for the individual to develop a large and strong body: these instructions would be his *genotype*. But environmental circumstances, perhaps poor nutrition, might make it impossible for the individual to develop in this fashion and he might actually turn out to be undersized and weak: this would be his *phenotype*. When this sort of thing happens, we may find that we have an indistinguishable phenocopy of another, normally developed, genetic constitution, i.e., physical similarity to another person whose genotype disposed him towards fragile development and whose normal circumstances of nutrition and growth ensured that the results of phenotype would closely correspond to the original genotype. This phenomenon, which is easily demonstrated in animal research, naturally adds considerably to our difficulties in arguing back from phenotypical appearance to genetic endowment.

But, from what has already been said, it will be clear that there must always be serious problems in establishing the nature of the genotype in those cases where controlled breeding and strict environmental control are not possible. Added to this, we also have the difficulty of analysing the relative contribution of individual components in cases of *polygenic* transmission – where a number of genes, some allelic forms dominant and some recessive, together control the appearance of a given phenomenon such as intelligence. In the next chapter we shall be looking at examples of ways in which it is possible to establish something of the nature of such complex modes of inheritance but, for the moment, it will be sufficient to note that they rely upon statistical analyses of the distribution of different phenotypes in large populations. Thus individual predictions are very much more difficult to make.

Microscopic and other laboratory methods will, in a small number

of cases, yield more precise predictions about psychological functioning but these tend to refer only to rather gross forms of disturbance. At present, there is no way of examining normal sets of human chromosomes and deriving information or predictions concerning normal adjustment. However, in the chapters which follow, we shall be looking at a number of examples in which chromosomal aberration is used to explain pronounced intellectual or emotional disorder and maladjustment, and to suggest a basic genetic control of these functions.

However, even in those extreme cases where there is obvious evidence of chromosomal damage, malformation, or missing or additional chromosomes, the developmental pattern must still unfold within a given context. And the quality of this context will inevitably help shape individual outcomes in just the same way as it does where the genetic material is apparently quite normal. We are often tempted to dichotomize the genetic and the environmental elements but, in reality, they are quite inseparable. We may, perhaps, think of the phenotype as a function of genetics plus environment but it would be much nearer the truth to think of the phenotype as being a function of our genetic dispositions interacting with the environment.

At birth, all children also inherit a psychological framework: a culture within which they must learn how to shape their mental and physical behaviour. When we come to look at the growing child or the adult, we are always observing a product of both biological processes and social forces – a product in which the ingredients seem to be mixed and fused beyond all hope of separating out the elements again. Yet, though imperfect, ways have been derived to handle this problem – principally by looking at the amount of similarity, on any given criterion, between blood-related members of the same family.

Naturally, close members of the same family generally tend to share very broadly similar environmental and cultural frames of reference, though there are always useful exceptions. And one can always invoke the known laws of genetic transmission to check whether our recorded occurrences of the criterion quality, in more or less distantly related members of families, accord with the theoretical expectations of their appearance. In this pursuit, it follows that *index* cases, or *probands*, must be selected and, from these starting points, we must trace family relationships of both a direct or *lineal* sort – like grandparents, parents, and children – and a *collateral* sort – such as brothers, aunts, cousins, and so on.

As will become apparent later, the choice of index case may be crucial in determining the results obtained; and it will, in any case, normally be necessary to collect data from both the starting point of

known cases possessing the characteristic being investigated and unselected control sources. This sort of approach, which itself generates innumerable types of research design, is generically of the *consanguinity* variety – that is, concerned with correlations between possession of given characteristics and closeness of 'blood relationship'. The degree to which the manifestation of any such given characteristics and blood relationships coincides is more technically and succinctly referred to as a measure of *concordance* – a key term in all family studies.

Of course, these family, pedigree, or biographical methods of studying family resemblances are of a much greater antiquity than is the science of genetics. Indeed, the concept of individual survival through our children is one of the most fundamental tenets of mankind, and one certainly will not have to look far in the history and literature of a people to see how repetitive is the theme of family resemblance and the presumed importance of one's ancestry in determining not only appearance but also character and mental gifts. A science was hardly necessary to draw attention to what has always been obvious to plain people. But scientific procedures *were* needed to systematize such observations, and to elucidate the underlying mechanisms. Mendel and those who came after him were to lead the way in demonstrating the physical processes, but it was Francis Galton in England who was to set the stage for more precise analyses of human genetics through the study of within-family resemblances.

Galton, like Mendel, was also born in 1822 – though the circumstances of the two men's families were very different, and perhaps decisive in moulding their unique approaches to heredity. As we have already seen, Mendel was of undistinguished farming stock and his research related to relatively uncontroversial horticultural matters. In other words, there was a coincidence of his family origins, experience, and the expectations associated with his monkish calling. By contrast, Galton was of an intellectual élite, whose family were related, among others, to the Darwins, and whose comfortable circumstances made it possible for him to travel to remote parts of the world and to under-write his own interests in human variation. But, more significantly, his family connections set him very near the centre of the evolutionary controversy and the great debate on the nature and descent of man. Given his own great intellect, it is almost unthinkable that he should not have been drawn into these matters in some way or another and it looks very much as though human heredity was as natural a choice of subject matter for Galton as was horticulture for Mendel.

In the person of Francis Galton, positive action, a formidable and ranging intellect, and the self-confidence to engage in vigorous polemic coincided with membership of a very remarkable family. It is therefore not too surprising that his starting point should be the familial inheritance of intelligence. Indeed, four years after Mendel's timid, and little noticed, publication Galton produced his *Hereditary Genius* – the first of several books dealing with eugenics – in which evidence drawn from detailed family studies was used in support of the view that mental characteristics are of substantially genetic origin.

By modern standards, Galton's pedigree studies may seem to lack balance in that they give too little emphasis to those aspects which would serve to disconfirm, rather than confirm, his hypothesis. For example, one would nowadays expect scientists to go to considerable trouble in postulating the influence of every conceivable environmental variable, and then sifting all possible evidence for their operation, before embracing the originally proposed hypothesis. But this is not a perfect world and we are still inclined to fall far short of the ideal. And, as Galton was one of the founders of human behavioural science, perhaps we should be more surprised by the originality and subtlety of his thought than by its limitations.

Naturally, the great difficulty with any family-tree type of approach is that, even if we are able to achieve some measure of criterion quality in all the members of two or more generations, the effect of environment is exceedingly difficult to exclude. And, in practice, two complete generations still yields meagre information – especially as this usually depends upon the fallible memory of members of the family itself, or upon fallible measures of the psychological criterion, or else upon a daunting combination of both. It is also unfortunate, from a technical point of view, that generations have such very wide age compositions and so, at any given time when research is being undertaken, some members may not yet have achieved the age at which given criterion characteristics would be manifested. Furthermore, wars and increasing occupational mobility have of late decimated and scattered families in a way which may leave many gaps in the available record. Where this happens, there are mathematical formulae which can be applied to predict missing results on the basis of those which have been successfully derived, but these are never as convincing as seeing the actual cases.

Even when the conditions for pedigree studies are ideal and all family members are available, and can be assessed over a period of time, much more is required in order to judge the genetic case. For example, scientific arguments must depend upon a broadly based

sample – which will mean accumulating a great many pedigrees showing the same patterns of appearance within given blood relationships. Exceptions to these patterns must be sought, and convincingly accounted for in terms of other causes, if the genetic theories are to achieve any reputable status. This will imply either clear-cut results or very refined and thoroughgoing analyses of the environmental influences. Generally, there will be no question of any such dichotomy but, in principle at least, the genetic case *could* be made simply by reference to the appearance of a given characteristic within related persons. If, for instance, a given phenomenon regularly followed a clear Mendelian pattern in its occurrence among relatives, then it would certainly be accepted as evidence of genetic transmission because the likelihood of such patterns happening by chance would defy all reasonable criteria of probability. In the event, though, findings such as these have not yet occurred in relation to any known psychological characteristic and so we are always concerned with ways of unravelling the causal relationship between the inherited and the environmental component of any given effect, whether in the field of normality or of abnormality.

But, if clear Mendelian patterns are not to be found, there are abundant examples in which it can be demonstrated that particular psychological attributes and conditions are more densely concentrated within certain families and, further, that these are significantly more often shared by close blood relatives than those more distantly connected. Such data are widely held to offer presumptive evidence that many abnormal mental conditions, like schizophrenia, are of either wholly or partial genetic origin. However, in the absence of a theoretically predictable pattern working within the known principles of genetic transmission, such associations do not guarantee causality. In fact, critics of the genetic position argue that psychological influences such as parental behaviour and child-rearing practices are also quite capable of accounting for concentrations in particular families, and especially for the phenomenon of closely related individuals being more alike than are more distant relatives.

In order to establish genetic causality, the ideal circumstances would be achieved if it was possible to hold genetic constitution constant whilst observing the effect of a different environment, or where environment was held constant whilst genetic constitution was different. If, as an additional bonus, one could also observe a number of discrete cases where both genetics and environment were similar, then one would have the perfect conditions for unravelling the nature-nurture mystery. These requirements are most closely met in the case

of twin studies, perhaps the most powerful single tool available in human behavioural genetics.

Identical twins are those who have a common genetic endowment – due to the fact that they are *monozygotic* (MZ), or have developed from the division of a single fertilized egg. They are often also referred to as 'one-egg twins'. Such twins are identified by many techniques beyond their obvious similarity, but most definitively by an extensive range of blood tests: fortunately, blood is composed of a number of constituents which have a well understood genetic basis. Non-identical twins do not share the same genetic endowment as they each derive from separate eggs fertilized by different sperm cells and so, as they develop from different zygotes, they are often referred to as *dizygotic* (DZ) twins or, reducing the jargon somewhat, as 'two-egg twins'.

In contrast with one-egg twins, two-egg twins are just as likely to be of the opposite sex as they are to be of the same. In fact, they are no more alike genetically than would be ordinary brothers and sisters – though their environmental circumstances, particularly if they are of the same sex and superficially resemble one another, are clearly likely to be much more similar than would those of siblings reared at different times and stages in the family's own development. In consequence, it is a common strategy to compare one-egg twins, where environment and genetics are similar, with same-sex pairs of two-egg twins where only the environment is held relatively constant.

A refinement of this type of study is where comparisons are made between the development of both one- and two-egg twins who have, through unfortunate circumstances, been separated and raised independently from an early age. And it is in those cases in which the rearing conditions of one-egg twins are most different from one another that the relative effects of heredity and environment should be most obvious. Once again Galton, who pioneered so much in genetics, psychology, and science generally, was to be responsible for introducing this technique of *co-twin control* studies.

Alas, twins are not especially common, and pairs of one-egg twins are less so. Even among those that there are, the incidence of remarkable and easily distinguishable psychological characteristics is a good deal less. And of those which additionally have been reared apart, and are still traceable, the numbers are small indeed. But surprisingly enough, a sufficient number of such cases has been traced and they have resulted in some fascinating research – mainly into the inheritance of intelligence or mental disorder.

However, all twin studies imply a number of assumptions which are not universally acceptable – though which particular ones will prove to

be unacceptable tends to depend upon whether the objector takes a predominantly genetic or a predominantly psychological view of what are likely to be the most important causal determinants in a given case. For example, a basically genetic orientation tends to suggest that when the degree of concordance, or similarity, in the possession of a given psychological characteristic is substantially greater in one-egg than in two-egg twins, a *prima facie* case has been made for significant genetic control. On the other hand, environmentally or psychologically orientated individuals tend to argue that such results are quite congruent with *their* interpretation as one-egg twins experience much more similar environments than any other siblings because the very fact of their striking physical resemblance stimulates people to treat them specially, and identically, from the very beginning. No doubt this is so in many cases, but it is a line of argument which is difficult to sustain when twins have been reared apart from a very early age. In such cases, if marked differences regularly occur in the degree of similarity between one- and two-egg twins, unless convincing environmental reasons can be advanced the parsimonious solution would be to accept that the process under consideration is influenced by genetic differences.

Of course, *all* behaviour is ultimately controlled by the influence which our genes exert upon the development and function of our biological systems: the problem is therefore really whether more or less effective mechanisms may regularly be transmitted within families, classes, races or whatever. And what is proposed by the co-twin control method is that, where there is no reason to assume that the environment is working selectively on one or the other of the twin pairs, any differences in psychological and behavioural capacities or characteristics which are found to exist between one- and two-egg pairs suggest the magnitude of the heritable component. Inevitably though, however high or low this degree of heritability may be expressed, it is in practice always relative to the prevailing developmental conditions.

Even if we can be sure that one-egg twins are genetically *absolutely* alike, and this is impossible to prove empirically, there are always other hazards in presuming such twins to be physiologically identical. 'Environmental' influences are not limited to events occurring after birth but refer to any circumstances, from the earliest stages of cell development which may influence the structure, position, or functioning of genes or chromosomes – or which might work directly upon the tissues of the evolving foetus, or the mind or body of the child or adult. The womb itself is often a dangerous environment, involving

as it does crowding, obstructions, physiological dependence, and the hazards of constriction in the birth canal.

Such dangers are the commonplace conditions of life but, for the twin, they are increased because of the need to share the restricted facilities. Indeed, single-egg identical twins, especially if they share the same chorion, are particularly susceptible to the adverse effects of crowding, and to be being differentially affected by the inequalities which arise in having to share the same maternal blood supply. The effects of this sharing are often that one of a pair is born underweight and less robust, and that the twins are further subjected to differential stresses depending upon their birth order and other obstetrical circumstances. All of which may be taken to argue that if unusual similarities still pertain between one-egg twins, then the genetic influences are probably even stronger than they might seem from the manifest similarities. Further, such considerations may also be used to suggest that when one-egg twins are less than 100 per cent similar (concordant) in some apparently strongly genetically-determined process, these physical environmental circumstances might reasonably be invoked to account for the empirical irregularities which trouble the thoroughgoing genetic theorist.

Achieving exactly similar environments is, even in principle, impossible. Just defining the range of potentially effective influences upon an individual presents insuperable problems – due partly to the difficulty of deciding which environmental influences are operating and effective, but mainly to the problem of knowing the effect of given stimuli. Our perception of meaning is, for example, at least partly a function of our previous associated experience, combining with a whole host of immediately operating contextual and motivational features. That an event may be experienced in as many different ways as there are people to share it is, though trite, also true. Thus a description of stimulus conditions will not, except under very simple circumstances, guarantee anything about the nature of the ensuing perception. So, when we refer to people sharing similar environments, we are speaking very loosely indeed. Even Siamese twins, one-egg twins who have remained physically joined, contrive to have different life experiences and to develop distinguishable personalities.

Yet twins usually come much closer than any other two people to sharing the same rearing environment, and because of this they still represent the best, if not the perfect, opportunity to examine the developmental outcome of pairs of children who start life with either identical or dissimilar genetic endowments but who grow up in an environment which is as similar for each child as can practically be

achieved. Conversely, when twins of either the one-egg or two-egg variety have the misfortune to be separated at an early age and reared apart, we have the best practicable situation to study the effects of the environment. Moreover, when one or other of the foster homes has unusual features – like a foster parent becoming schizophrenic or alcoholic, there come into play really interesting possibilities for the analysis of heredity-environmental influences through studying each of the twin pair's own susceptibility to similar adjustment failures. The methodological problems and weaknesses inherent in these techniques are formidable, but we shall be taking a closer critical look at the methods and results of some of these studies in later chapters.

It is, however, worth making the point at this stage that twins are not a representative sample of people in general and that, given results based on twin studies, we must still question how far they may be generalized to non-twin populations. This line of objection turns on a number of considerations, both environmental and biological. The environmental conditions of twins are such that parents, and especially mothers, must divide their attentions between their babies in a way which would not normally occur with children who were at different ages. Thus, mother-child interactions may have significantly different characteristics. Then there are all the unusual forces which may act to influence twins to take on the role of one half of a pair – or to react, and perhaps to over-react, against this happening.

But there are also many physical and developmental differences which make twins a non-representative sample. For example, more than half are born prematurely – both in the sense of arriving early, and by the weight criterion of being five-and-a-half pounds or less at birth. This developmental disadvantage seems to be more than a passing phase as there are now a number of studies which indicate that twins, as a group, continue to show a degree of retardation in later life – and not only in size, but also in intellectual development. The differences between the twins and the rest of the population are not gross but they do suggest that any extrapolations of results from twin studies should be very cautiously viewed when they are applied to the general population.

However, such a caveat is seldom taken as a reason for throwing away the baby with the bath water: it simply suggests that whatever results come out of twin studies should be compared with data derived from other sources, and that interpretations should be set in a wider context of evidence. We have seen that all consanguinity (blood-relationship) studies raise methodological difficulties but, if some degree of consistency marks results which have been derived from a

wide range of environmental circumstances, and if the pattern of results coincides with what may reasonably be inferred from known genetic principles and controlled experiments with animals, then we might perhaps be said to be getting close to achieving conclusive research. In some areas this ideal is beginning to be realized but, here too, there are methodological problems – particularly those involved in relevant behavioural genetic studies with animals, at least of a kind in which the conclusions are capable of being generalized to human behaviour. Even so, 'adoption', or 'cross-fostering', experiments with behaviourally distinctive inbred strains of animals are extremely suggestive when taken in conjunction with studies of the inheritance of human emotionality and cognitive processes.

The field is a difficult one, but not impossibly so, and, despite the fact that present work is, in most cases, quite inadequate to serve as more than a source of hypothesis and a spur to further work, it is extremely valuable in this respect. There are always plenty of pessimists around with reasons why something is impossible but in the end, with enough patience, ingenuity and flexibility to mount attacks from a sufficiently large number of angles, most worthwhile problems ultimately seem to yield up their secrets. No doubt this will also prove to be true in relation to the inheritance of behavioural and psychological characteristics. For the present though, it is a matter of proceeding with caution and avoiding too hasty conclusions – however attractive the results of particular pieces of research.

Chapter three

THE INHERITANCE OF INTELLIGENCE

If there is one issue in behavioural genetics which really raises the temperature whenever it is mentioned, that subject is the inheritance of intelligence. Above all others, this is the issue which makes people feel most threatened, or worried that the egalitarian ideal is coming under attack. Accordingly, the proposition that intelligence is largely genetically controlled is the very one which is most likely to provoke people to dismiss the whole of behavioural genetics out of hand. The grounds for this rejection are innumerable but some do stand out as being most salient and typical.

In the first place, it is countered that it is *not true* that intelligence has any sort of genetic basis: it is, the argument runs, simply a matter of environment and the opportunity to learn. The second objection is that it is *not possible to measure intelligence* in any realistic way; that intelligence tests measure only ability to perform such tests, and that test scores are demonstrably influenced by environmental opportunities and practice at the tasks. Then there are the *ethical and social objectors* who argue that, in any case, it is wrong to undertake research which seeks to establish basic, and perhaps divisive, differences between individuals, groups and races. This latter point of view is mainly concerned with the ways in which research findings may be used by irresponsible members of one section of society in order to justify exploiting, or otherwise badly treating, other sections of the community.

Finally, it is often argued, studies of the genetic component of intelligence should not be undertaken at all as they have little or *no utility*. In other words, public money simply should not be used in this area of research for, if it should prove that intelligence is largely genetically determined, no useful practical outcome would follow. Alternatively, if the genetic component should be shown to be inconsequential, then money would have been squandered on an equally useless piece of knowledge.

As can be seen, the reasons which have been marshalled against the pursuit or the publication of research in this field are very definite – and each of them can command a great deal of support. The emotional and political aspects of research into intelligence certainly cannot be set completely aside: they are often an integral part of the perceptions, pursuits, and preconceptions of the people concerned. And it would be naive to suppose that issues like these, which have such important practical consequences for education, race relations and social policies generally could, or should, be held aloof from those who are touched by them. However, before becoming too involved in an analysis of opinion and rhetoric, it would be as well if we were first to review the discoveries and the events which have taken place in this line of enquiry. Then we can attempt to set these in the context of the wider issues that surround them.

Paradoxically, despite the fact that so many people strongly object to behavioural genetics because they wish to believe that intelligence is not genetically mediated, it is in this very sphere that it is most easy to demonstrate the action of genes on mental functioning. That intelligence, especially where it is very high or very low, has a marked familial basis is by no means a recent observation. As we have seen, Galton long ago argued a case for the inheritance of superior intelligence. But methodological analysts had been quick to point out that 'familial' connections do not imply a necessary genetic link: the effect of special opportunities pertaining in the homes of the particularly able and successful is always a possible confounding factor. Had Galton, or his successors, been able to point to either a pattern of inheritance which unequivocally matched the predicted manifestations of classical genetics, or had they been able to point to identifiable chromosomal differences between the very intelligent and the rest of us, then their case might well have prevailed. As it is, these requirements have still not been met.

On the other hand, cases of remarkably low intelligence have, in many instances, been clearly shown to have an unequivocal genetic basis. At first, though, the available evidence was no different in kind from that used in Galton's study of genius: that is, the collation of selected family records for a number of generations. Fortunately, many of those families which were regularly producing mentally subnormal offspring proved to be of rural origin and thus tended to be residentially settled and inbred to an unusually high degree. This, of course, meant that there were a good many cousin marriages – a circumstance which confers numerous technical advantages in genetic research, particularly when recessive genes are involved. In

fact, it transpired that the sort of family studies which were commonly conducted in institutions for the mentally subnormal, though in themselves inferential and potentially confounded by the environmental variables, nevertheless produced such promising results that there was much additional encouragement to continue along all possible genetic avenues.

Furthermore, as the number of serious mental retardates in this country alone amounts to somewhere between one-third and one-half million, the practical reasons for establishing precise causalities in this sphere are obviously much greater than those concerning the inheritance of superior intelligence. Not surprisingly, therefore, we now have considerably more information about the genetics of mental defect than we have about the inheritance of either normal or above average capacities.

Of course, not all cases of mental deficiency or retardation are due to genetic causes. There are many types, and causes, of intellectual malfunction, some of which have fairly obvious pathologies, such as damage to the developing brain due to mechanical injury or other adverse conditions arising before, during or after birth. However, a very substantial proportion of hospitalized cases do have a genetic basis – a conclusion which was finally made inescapable as a result of laboratory evidence.

Right up to the period between the two world wars, our solid knowledge of actual genetic mechanisms and their operational processes was of the most rudimentary sort as each of the most direct approaches presented great technical difficulties. No one was in any doubt that the most obvious approach would have been to start with a visual examination of genetic and other cellular material. Comparisons between apparently normal individuals and those who exhibited a mental defect which was unusually prevalent within generations of their family could reasonably be expected to reveal some of the links. However, it has taken a long time to develop such a methodology and it was not until after the second world war that the direct observational methods of *cytogenetics* had advanced far enough to engage in even fairly crude microscopical analysis of human chromosomes.

The other methodological possibility was to compare aspects of the body chemistry of normal individuals with those of people suffering from forms of mental retardation which family-tree studies had already suggested might have an hereditary basis. Naturally, this method is likely to be somewhat more inferential but it has nevertheless produced many of the earlier demonstrations of both the genetic basis and the functional stages involved in the transmission of

disorders. The classic example of this approach derives from as long ago as 1934 when Asbjörn Fölling noted that there was something unusual about the composition of urine obtained from a mentally retarded brother and sister. Subsequent research revealed that this biochemical abnormality was linked with a specific syndrome or sub-class of retardation which exhibited a marked familial basis and which was also partly defined by a typical physical appearance due to deficiencies in pigmentation.

Further biochemical studies went on to show that the basis of the disorder was an inborn error in the body's metabolism such that a common constituent of protein foods, phenylalanine, is not normally processed. As a result, an affected child suffers from a deficiency of the substances which would normally be created, and an excess of phenylpyruvic acid – the material which, when excreted in the urine, gives the clue as to diagnosis and the defective system involved. The syndrome was termed *phenylketonuria* (or PKU) and, where nature takes its course, most youngsters suffering from it become severely mentally retarded.

Because the incidence of PKU forms a clear-cut pattern, and because it proved possible to identify which specific enzyme was deficient in the process of metabolism, it also proved possible to show that the condition was due to a single recessive gene. In other words, when both parents contribute this same recessive form in the fertilized egg, the child is inevitably born with a programme which will, in a completely normal environment, result in his growing up to be mentally defective. PKU is, of course, one only of many conditions which have demonstrated the ways in which mental development depends upon our inheriting correct physiological directives – but it is a good example of its type. No one now doubts the genetic basis of this condition but its proof must rest upon indirect observations and inferences, no matter how convincing these may be. What most of us seem to prefer, though, is a more direct glimpse of the aberrant mechanisms, even if this is not always as illuminating.

Of recent years, it has become possible to make preparations of human chromosomes with relative ease, and to examine these microscopically for any unusual features. The process remains rather crude as it is still only possible to look at the gross form of the chromosomes without knowing much about the genes which compose these bodies. Even so, very interesting and revealing surprises were in store. For example, by 1959 J. Lejeune and his co-workers were able to show that 'mongolism' is due to the patient having too many chromosomes – forty-seven instead of the usual forty-six. This

additional chromosome appears to be a duplicate of one of the smaller 'group 21' pairs – the grouping reference itself merely deriving from the fact that, in conventional analysis, chromosome pairs are identifiable through having been generally classified in descending order of size. Alas though, taxonomic identification, despite giving a semblence of precision, yields no very precise information as to the mechanisms involved in such pathological cases – but it is a beginning. In addition to mongolism, or *Down's Syndrome*, as it is more correctly termed, it would be possible to refer to other examples of *trisomy* – that is, where having an additional chromosome in one or other of the groups leads to mental retardation. But the point that very low intelligence may literally be shown to result from chromosomal, and thus genetic, aberration surely needs no further labouring.

The source of pathological conditions due to abnormal chromosome numbers is usually thought to lie in *meiosis*, when the female produces her egg cells by the process of reduction division. In some cases, egg cells containing both of a given chromosome pair will be formed because they failed to separate during the division. By the same token, the twins of these dividing cells will be deficient by one chromosome. Either way, such germ cells are not in possession of the requisite 'half set' of genetic material. The vast majority of these abnormalities would not be fertilized or, if they were, would not be viable and no infant would therefore result. However, as with the case of mongolism, some abnormalities are viable and result in live births – particularly if it is the sex chromosomes which are involved. Usually there are penalties to be paid, not only in terms of sexual development but also in terms of intellectual growth and functioning. Most, though not all, individuals affected in this way tend also to suffer from some degree of mental dysfunction, ranging from slight to gross retardation and, as we shall see in chapter six, the degree of impairment tends to be related to the amount of chromosomal aberration.

With some exceptions, gross chromosomal abnormalities tend to produce fairly marked pathological conditions, though it is equally true that gross intellectual impairment of a hereditary nature may occur without any obvious signs of abnormality at the chromosomal level and that major chromosomal aberration may occur without necessarily causing any obvious mental impairment. The genetic information which has significance for intellectual performance evidently does not have any simple and unitary characteristic. Nor would we expect it to have: intelligence is a complex and compound behavioural concept and its manifestations depend upon a very large number of capacities which, in turn, depend upon many sensory and

cognitive systems which are of demonstrably variable quality in even the most normal individuals. The dangers of reifying a concept and supposing that, because we have a single word to sum up a lot of more or less related ideas and experiences, there is a singular object in nature which accounts for it, cannot be too strongly stressed. For our own part, we should keep it constantly in mind that we are dealing with the end product of many biological and environmental processes interacting with one another: not just a psychic phenomenon.

It is, perhaps, abundantly obvious to most people who have ever given the matter more than a passing thought that the construction of every brain, and its biochemical environment, is ultimately dependent upon genes in order to prescribe the growth of tissue and to regulate its metabolism through the production of enzymes. But what appears to be much less obvious to most of us is that the pathological examples suggest more than a diagnostically useful correlation of mental dysfunction with chromosomal abnormality: they also suggest that we might incorporate the same paradigm into our investigation of the manner in which underlying components of intellectual and behavioural processes are normally controlled and mediated.

Having determined some of the ways in which subnormal intelligence may be genetically investigated, the next question is whether the range of non-pathological intelligence – from brightness to dullness – can be shown to be a matter of heredity. In this quest, a biological and biochemical rationale will ultimately be necessary to account for the behavioural observations, and this means building up a corpus of experimental evidence. Naturally, the type of experimental studies which may be undertaken with human beings is relatively restricted but, fortunately, animal studies are often very illuminating.

Now there are always those people who argue, usually somewhat emotively, that studies of animals will only tell one about animals, not about human beings. But the plain fact of the matter is that human beings *are* animals, not some special form of creation. And despite all the distinctive characteristics of our own species, our basic biological processes have a good deal in common with the rest of the animal kingdom – quite obviously so with close relatives like the higher vertebrates and other primates, but also even with some of its much more lowly representatives. Nevertheless, the relevance of comparative studies must be judged on their individual merits, and extrapolation from animal behaviour to the presumed basis of human psychological activity always suggests a very conservative approach. Even so, where patterns emerge, or where experimental findings are not incompatible with what we know of human processes, such results

may appropriately be used as *part* of the scientific case – subordinate to evidence based on actual human studies and suggestive of hypotheses and research directions.

Extrapolation from animal studies is made particularly troublesome because of the difficulty of defining 'intelligence' in animal terms. Researchers differ in their approach to this problem and many, wisely, leave it to others to decide whether the processes they have studied in animals can appropriately be identified with those which underlie human intelligence. In practice, however, what tends to happen is that an animal's capacity to learn new adaptive responses is roughly equated with intelligent behaviour and then still more roughly generalized to what we mean by intelligence in man. And, given the great biological similarities which mark all higher animals, few would wish to deny that such comparative studies seem to have a *prima facie* relevance. However, let us be quite clear that we are only speaking of relevance, not equivalence.

Perhaps one of the most celebrated pieces of animal research to have emerged from the literature of selective breeding for intelligence was a study reported by Rober Tryon in 1940.* Essentially, what he did was to take a group of unselected white rats and grade them according to their ability in learning how to get through a maze. Having graded them, he then interbred the 'bright' rats – those who scored highly on the task – and did the same with the low scoring 'dull' animals. The interbreeding process of only mating high maze scorers among the 'bright' progeny, and only interbreeding the low scorers among the progeny of the 'dull' group was continued for twenty-one generations. But, after only eight generations, there emerged pronounced differences among the progeny – such that even the poorest performers among the 'bright' strain were better at the task than the brightest of the 'dull' strain. In parenthesis, it is interesting to note that this process is reversible: with the cessation of selective breeding and the reintroduction of uncontrolled mating, the original distribution of abilities may rapidly be reinstated.

Despite many objections to the experiment, and to its interpretations regarding human intelligence, the work still stands as a prime example of much subsequent work done with other animals in other circumstances. For many workers, such results are taken as clear-cut evidence that intelligence has a large hereditary component but, for others, it seems that the memory process tapped by

* *Yearbook Nat. Soc. Study Educ.* (1940) **39**, 111–19.

experiments of this type suggests a measure of something less profound than 'intelligence'.

Semantic and conceptual confusion is hard to avoid in these issues and, in consequence, we are sometimes tempted to make quite unjustifiable distinctions between memory and intelligence. The truth of the matter is, of course, that there is a quite intricate relationship between the two – their conceptualizations being such as to defy any exclusive definition. For, though intelligence is a much more general concept in some ways, it is extremely difficult to conceive of intelligent behaviour which does not involve memory. All situations which evoke a response based on learning, and this is just about everything other than primitive reflexes, necessarily depend upon memory, whether in the form of conscious memories from previous experiences or behaviour derived from the non-conscious store of learned skills. Now, if we can agree that memory is a fundamental characteristic of both learning and intelligence, we can perhaps begin to consider the evidence for genetic involvement in these processes.

At the level of body and brain functions it is still virtually impossible to specify the exact properties of genes, but it is possible to say something of their associative properties and the way in which they function. We know, for example, the sequence of activity whereby the genes, which are composed of deoxyribonucleic acid (DNA), stimulate the production of simpler executive substances like ribonucleic acids (RNA) which, in turn, bring about the production of a wide range of proteins. So, although we cannot observe the activity of the genes directly, we may presume their operation from measured changes in local levels of RNA and protein and it is possible to put this knowledge to work in unravelling more of the story.

At one time it was believed that the reason why chicks stay close to their own mothers was simply that they were 'instinctively' drawn to her. Such a way of defining the situation carried within it the apparently adequate causal explanation and thus tended to rule out the notion of intelligence being involved – however intelligent the actual strategy might be. Recent research has, though, shown that the way in which the chicks of many species come to relate to their parent is by means of rapid early learning. To be sure, this process may not seem a very high water mark in our scale of intelligence, involving as it often does in the laboratory many grotesque choices of parent, but nevertheless the chick does satisfy a criterion of intelligence in its ability to learn adaptive solutions to life's problems. Continuing the extrapolation, if we accept that such adjustive behaviour is not fundamentally incompatible with many situations which are held to

invoke human intelligence, then we may proceed to consider some experimental work which it would not be possible to undertake with humans.

For example, it was of considerable genetical importance to learn that localized biochemical changes occur in the brains of organisms learning new responses. This phenomenon, revealing elevated levels of RNA and protein production in those animals involved in a learning programme, as contrasted with those excluded and used for comparative purposes as controls, is now quite well documented. But, in a series of even more sophisticated experiments involving the surgical disconnection of the two halves of chicks' brains, Steven Rose and his colleagues were able to achieve an even more convincing demonstration of the effect. The purpose of this 'split-brain preparation' is to connect visual learning mediated by one eye with only one side of the brain whilst the other side, excluded from the learning situation by blanking off its connected eye, could act as the control in making subsequent comparisons of biochemical activity. As might be surmised, results have shown that the hemisphere involved in learning shows a higher RNA and protein concentration than the non-involved hemisphere.

The rationale behind such studies is straightforward enough, turning as it does upon our knowledge of neurochemistry and the processes whereby genetic material controls and mediates the manufacture of proteins. In nerve cells, certain of these substances are known to flow outwards towards the peripheral parts of the cell where they are then involved in modifying the connections between adjacent nerves. It seems likely that it is these same chemical 'bridges' linking cells, and groups of cells, which are largely responsible for our capacity to store and co-ordinate information – and thus to behave intelligently.

Now, though there are many obscure and controversial aspects to this genetical linking of DNA-RNA-protein production-memory-learning-intelligence, nevertheless it is quite compatible with our present state of knowledge. Furthermore, it is supported by evidence from many experimental interventions into the chain, it being found, for example, that injecting mice with the protein-inhibiting drug puromycin will impair their long-term memory and learning capacities. Like any other system, our neurochemical processes may be more or less efficient in comparison with those of other people. And any hereditary variations in their effectiveness, or any other systems which influence their performance, may account for some at least of the observable differences in people's intellectual capacities.

Certainly the biochemical aspects of intelligence seem to be a very

promising starting-point for the study of genetic effects. We know, for example, that the normal range of intelligence is not related to observable differences in the structure of nerve cells, neither is it heavily dependent upon the amount of nervous tissue within the skull. Furthermore, massive amounts of brain tissue may sometimes be destroyed without grossly affecting the level of a person's intellectual functioning. It is even possible to remove surgically an entire hemisphere of the brain and yet still leave the individual in fairly good shape. So, any genetic explanation of differences within the normal range of intelligence is unlikely to be successfully based upon gross structural considerations.

On the other hand, apparently miniscular disturbances in the brain's normal metabolism are quite capable of resulting in major psychological effects. A good example of this is the psychedelic drug LSD, where the effect of a few millionth parts of a gramme of this material on the brain may be to produce almost total mental disorganization. This particular example is all the more interesting as the drug LSD is a very similar chemical to those which normally occur in the brain. Not surprisingly, this is a point which will be taken up again in chapter seven when we come to discuss the interface of genetics with mental disorder. But even carbon dioxide, oxygen, and a thousand and one perfectly normal bodily constituents can, in either excessive or deficiency quantities, radically affect mental functioning. It seems, therefore, perfectly reasonable to assume that minor variations in the brain's metabolic functioning may account for much variation in what we measure and describe as 'intelligence'.

Of course, although there is an ultimate genetic regulation of the body's entire economy through the chain which creates, transforms and destroys all chemical substances, nevertheless such control is only relative. As well as the ingestion or creation of toxic materials, starvation or dietary deficiencies may also introduce conditions beyond the capacity of the genetic controls to offset them. However, we are now beginning to accumulate a body of knowledge on this subject - particularly from studies of children suffering from malnutrition.

Contrary to many people's expectations, the consensus of results shows that poor diet is of considerably less significance for intellectual performance than has often been supposed. The evidence suggests that, even in extreme cases where mental effects due to starvation are apparent, these are usually reversible through enriched diet. Future research *may* show that differences between the intellectual performance of apparently well nourished individuals or sub-groups is related

to their diets but up to the present time there is no evidence for this. For the moment, our best hypotheses as to why people differ in their intellectual capacities would therefore seem to lie in the fields of environment and gene-mediated neurochemistry.

Perhaps the topic which lends itself most readily to polarizing the argument as to whether intelligence is mainly affected by environment or by inborn factors is that of racial differences. Over a period of many years now there have been numerous arguments in support of the view that certain ethnic groups are innately, and therefore genetically, superior to other groups. Apart from situations like those which occurred in Germany and are still occurring in South Africa, where political doctrines are allowed to subvert science, nowhere has the issue of innate racial differences been so systematically studied and discussed as in the United States. And, in this pursuit, it is most usually people of negro origin who are singled-out for comparison with white Americans. Other sections of society, and particularly those of Puerto Rican and Mexican origin, have also been the object of similar research but it is the negroes who have most frequently been judged less intellectually able than the white population, and generally of lower intelligence than any of the other ethnic groups studied.

The evidence supporting the argument that American negroes, as a group, are of below average intelligence derives mainly from two sources. In the first place, it has been pointed out, negro citizens have achieved very little indeed in a society which offers a real possibility for improvement through its state educational facilities – a system which caters for all ethnic groups from the junior schools right through to the universities. If any particular group remains in poverty and ignorance, the fault lies in their own lack of ability, not the system – of which negroes have been a part for much longer than most of the white immigrant families. Naturally, such a line of argument has a certain reasonableness if taken at its face value, but the issues involved are far from being self-evident and require us to think especially about the effects of those environmental pressures which may act in relation to social acceptance and employment prospects.

Just by way of orientating our thoughts on this subject, it might be salutary if we first try to imagine having to account for the lowly social and cultural status of most of our own forebears as retainers, servants and serfs in very hierarchical societies. When brought closer to home like this, I suspect that most of us would wish to think about the widest implications of the situation, rather than jumping immediately to the simplistic explanation that our ancestors must have been inherently dullminded and otherwise inferior. More likely, we should argue, they

were simply the products of their contemporary religious, social, economic, military and technological epoch. A small shake-up in the ingredients, and the kaleidoscope of human affairs soon reflects a quite different pattern.

Nevertheless, the progress of American negro education has often been a topic for special concern. Given the opportunity to improve themselves through education, negro children are still not making the progress that had been hoped for. Even the 'Head Start' programmes of augmented teaching, which have been developed to offset possible deficiencies of whatever origin, have proved disappointing in the extreme – though whether their content is just too little and their context too narrow, is another issue still being hotly debated.

The original rationale behind these programmes was that if poor academic performance and mental functioning is really, as much environmental theorizing would have it, only a matter of relative educational deprivation in many of the predominantly negro schools, then academic augmentation should raise the level of both educational achievement and measures of intellectual functioning. But, as these hopes have not been very fully realized, two conclusions have tended to follow: on the one hand, it has been inferred that the observable average educational differences between negro and white children may, after all, depend upon more than just environmental factors. On the other, it has come to be felt that those intellectual qualities which may be fostered in the child within the classroom situation are evidently in a very much more complex interaction with other aspects of his development than has usually been allowed.

In many respects, this latter conclusion was only slightly more comforting than the former as it underlines the considerable difficulties which stand in the way of effecting change by means of any very direct policy. For example, if it is the case that many American negro youngsters are part of a subculture which commonly rejects the value of intellectual and educational qualities as a realistic road to adjustment within society, then it would not be surprising if they were to develop markedly different habits of mind and behaviour in relation to the predominantly abstract content of school work. Whether this is really the case remains to be established but, if forces like these *are* at work, then it is going to require intensive and long-term strategies to bring about the desirable changes. As always in these discussions, however, it is all too easy to confuse criteria of achievement and measures of basic capacity for, though they are related, they are by no means identical.

The problem is that the evidence available in discussions about

these educational and occupational issues is always inextricably intermixed with other imponderable, and unmeasurable, variables. Frequently it is difficult to know whether these variables have any relevance at all, let alone to know what this might be, and for whom.

From the point of view of a genetic-hereditary analysis, such confused and confusing sources are clearly best treated with great caution. They can scarcely be ignored, though, as they permeate every discussion of the subject. Even so, it is in the best scientific tradition to limit problems to a scale which can be treated experimentally. This means the careful matching of subjects for comparison, the application of standardized measurement procedures, and the quantitative recording of results for mathematical analysis. The weakest point in these operations is always likely to be that the matching of groups overlooks, or even creates, some systematically operating variable which affects the measured performance of one of the groups more than the others.

As we have seen in the case of ethnic group differences, innumerable possibilities may be invoked and will need to be checked as potentially confounding influences. The controversy surrounding studies reporting ethnic differences in IQ are often largely based on the assertion that the measuring instruments employed are not 'culture fair'. This is one emphasis in interpretation: another, the predominantly hereditary explanation, is most vigorously expressed in the United States by Arthur Jensen and in Britain by Hans Eysenck.

Inevitably, both of these scientists have been dubbed 'racists' because they do not subscribe to the more socially acceptable and predominantly political-economic school of thinking which interprets most psychological differences in terms of environmental context and determinants. However, though neither Jensen nor Eysenck seem to be anti-negro in any conceivable sense, they have quite understandably enraged the negro community by stating that they are unable to account for the major part of measured IQ differences without recourse to genetic explanations. Both have argued that, in view of the experimental controls involved in the better studies, and the type of intellectual measures which have been used to establish the presence of significant average differences, social and educational factors alone do not seem capable of accounting for the results. This conclusion is naturally subject to criticism of all kinds, but the one which has most general relevance to all studies of the inheritance of intelligence is the assertion that IQ tests do not, as is claimed for them, measure 'pure' intelligence. Rather, it is held, they are contaminated by educational achievements of a sort which place some children, and

most markedly negro children, at a disadvantage. By way of support for this thesis, protagonists have been quick to draw attention to corroborative studies – like the one reported by Otto Klineberg as long ago as 1935.

Klineberg had been particularly interested in the educational progress of negro children coming to New York from the southern states of America. Testing and re-testing revealed that IQ scores were not a stable phenomenon for these children, but that they rose with length of stay in the city. Such results do not in themselves account for the persistence of average differences between the children of long-established black or white families, but they are used to make the point that the measures of intelligence being used do not simply tap innate capacities; they are also sensitive to environmental opportunities.

The racial comparisons question is a vexed, and vexing, one. Like many of his severest critics, Jensen seems to be concerned with improving the relative position of American negroes by developing appropriate educational programmes. The acrimony arises not so much from a dispute about the *results* of existing comparisons of intellectual performance, but from their *interpretation*. Jensen argues a most carefully reasoned case for the genetic position which, in spite of its social unpalatability, he urges, should be accepted not only on its scientific merits but also for its social utility. As he points out, the correct identification of causality is a major step in the direction of correcting a problem.

However, many of his critics have rejoined that, even supposing he is absolutely correct in his analysis, the actual effects of ascribing genetic causes would be to make the negro position much worse, by feeding the fires of black inferiority feelings and aggression, as well as those of white prejudice. Better by far, it is argued, simply to deal pragmatically with the educational and occupational problems which exist: if certain individual children are experiencing greater difficulties, one should just get on and find ways of overcoming them. This point of view plainly has a good deal to be said for it as it directs the educational problem away from racial issues and underscores the fact that backwardness exists in all ethnic groups, and that average values should not be allowed to lure us from the perspective of seeing each individual as he is. All groups contain individuals who are retarded, just as they all have their share of the normal and gifted range.

Whatever the rights and wrongs of the current dispute, perhaps one of the most invidious things to emerge is the way in which we have allowed ideas to crystallize to the detriment of all concerned. Even the pressure to suppress unpopular research findings, or to ban or curtail

work in certain areas, is itself a most dangerous assault on our integrity as rational and free people. More than that, it is an ideal recipe for the sort of subjective science which is not only worthless but is also an ideal vehicle for propaganda and tyranny. That is the one side; its converse is that even the most sober scientific studies have proved dangerous in the hands of people who do not fully understand the nature of the findings – particularly if they are motivated by a commercial or political pressure to peddle 'opinion'.

In principle, there is nothing absurd about the notion that the races of man may differ in intelligence, just as they do in obvious bodily ways, and just as individual men of the same race do. Despite the fact that mankind is one single species, biologists have also pointed out that the different races (including whichever one we happen to belong to) actually comprise a number of sub-groups as a result of the intensive inbreeding of more of less isolated stocks. It is a phenomenon common enough in biology, giving rise to different *gene pools*, or frequencies with which certain genes are represented. However, objectors have argued that 'races' are imaginary things because absolutely pure forms probably do not exist and thus we cannot be specific in our classifications. Of course, there has been much intermixing and it may sometimes be difficult to distinguish a person's racial characteristics simply by looking at him. But nevertheless, because it is difficult to draw the line in some cases, it does not make it any less true that distinctive races exist. We should not be likely to mistake a Pygmy for a Scandinavian, and each group breeds its like. The physical differences are real enough and their genetic causes are real enough, the only question is whether it is also true that people drawn from these different gene pools also differ in their basic *intellectual* equipment.

If we take the very long-term neo-Darwinian perspective on the matter, this itself suggests an answer to the question of genetic differences. During the course of their evolution, the races of mankind have come to differ from one another in certain physical ways, largely because environmental conditions in different parts of the world have favoured some randomly occurring characteristics more than others. These have then been systematically selected, and preserved, in just the same way as has happened throughout the animal kingdom. Yet, as a species, the central problems of human survival have been much the same in all parts of the world: man has sustained himself through foraging, hunting, fighting, fishing and farming under all sorts of changing physical conditions. The mental qualities required to do this have been similar over countless ages and have only changed very recently as a result of uneven technological development. A

Lamarckian interpretation of adaptive change through acquired characteristics would perhaps be quite congruent with real intellectual developments having occurred as a result of historically different cultural influences. But, if we are to follow accepted wisdom and take the very much longer perspective of Darwinian evolution, it may strike one as unlikely that intelligence, which is a common substrate of all mankind's struggle for survival, should develop differentially.

Having said that, we are still left with the embarrassing fact of an accumulation of studies which seem to suggest that such ethnic differences do indeed exist. These average differences in existing measures of intellectual performance may or may not prove to have a genetic origin, but the first thing to be absolutely sure about before accepting this explanation is that the mental tests used are equally fair to all concerned. It may even be that the samples taken in contemporary tests over-emphasize certain culturally valued abilities whilst minimizing other, equally important, facets of human intelligence. Thus it may prove that, although groups differ in certain respects, they may still balance up evenly if we were to take a wide enough view of 'intelligence'.

In fact, the whole notion of 'measuring' intellectual capacity and potential is once again coming under severe scrutiny and criticism as a result of the fuss created by racial comparisons. Setting aside technical considerations for the moment, some circles are even discussing the question of whether IQ tests should *ever* be administered to unimpaired children, of any background, because the results of such studies may come to be used as the basis for a socially undesirable élitist philosophy. Historically, though, intelligence tests were largely the product of social need, having been widely developed in the early years of this century to deal with some of the problems which were becoming apparent in public elementary education. Widespread financial and cultural impoverishment among the working families of the new highly industrialized society, coupled with crowding in schools and a chronic shortage of qualified teachers, had resulted in serious problems for staff and pupils alike. One of the urgent problems posed was that of ascertaining the reasons why some children were failing at school, and indeed whether they would ever improve substantially within a normal curriculum and method of teaching. Nowadays we recognize that some children are 'educationally subnormal' and that they will not profit from trying to keep up with the work of their more able classmates. Most of these children have a struggle to master even such basic skills as reading and writing: for them a more intensive, though slower and more specialized,

programme in special schools has been developed to optimize their capacities.

The problem in mental testing, now as before, is to identify children with poor intelligence and not to confuse them with others who are simply backward in their present level of achievement. Backwardness in school may be due to a whole host of reasons – including emotional problems, difficult home circumstances, absences and physical illness, sensory problems such as poor vision or hearing, and so on. Specific problems such as these can, through appropriate interventions, usually be dealt with and the child enabled to offset his achievement difficulties. It is just as important to be able to identify this sort of case as it is to identify the child who will only ever be able to cope with a programme of more modest objectives. Intelligence tests were devised in order to make this discrimination – one which is frequently exceedingly difficult for non-specialized teachers who are also subject to parental pressures. What was needed were tests which would minimize the effects of learning and maximize measures of the capacity to learn and reason.

In Paris, Alfred Binet pioneered such tests in order to distinguish intellectually- from educationally-retarded children. The tests themselves were based on a range of apparently fundamental perceptual, memory and reasoning skills and tried out on a large sample of school pupils.Roughly speaking, items which could be done by most children of a given age group, but which were beyond most of those of a younger age, were used as indicators of the child's 'Mental Age'. In other words, a child of ten years who made an average score for ten-year-olds would be given a mental age (MA) of ten. If he was only able to produce a performance equal to that of the average nine year old, this would be his MA – giving an index of his present level of mental functioning and suggesting the degree of his retardation. At a later stage, and in order to offset many of the drawbacks of Binet's original mental age concept, MA was divided by the child's chronological age (CA) to give a ratio, and this ratio was multiplied by one hundred in order to give whole numbers. In our example of the ten-year old who scores to a nine-year-old level, the computation would be:

$$\frac{\text{Mental Age}\quad (9)}{\text{Chronological Age}\quad (10)} \times 100 = 90, \text{ or an IQ of 90 points.}$$

Many modern IQ tests have scores which are calculated somewhat differently but follow the tradition of setting the index for the average IQ at 100, and then comparing the individual with age-related

population norms. Where things start to get difficult is in agreeing just how far a given standardization population is an appropriate yardstick for assessing not only particular individuals, but also its constituent racial and cultural sub-groups. If the tests upon which final IQ levels are based depend upon specialized knowledge, or modes of thinking which are not equally shared by those people being tested, then the results are likely to be exceedingly misleading.

Most analysts of the situation have difficulty in accounting for the ways in which environment, within normal limits of variation in a culture, may significantly affect the sort of abstract problem-solving involved in so many tests. And where previous learning *is* involved, in however minor a role, test constructors have generally been very much at pains either to keep that requirement to a minimum, or to attenuate its effect by empirically establishing that the experiential components are widely distributed within the population for which the test was standardized.

Nevertheless, even following instructions usually pre-supposes a certain verbal efficiency, and many of the sub-tests are frankly verbal or dependent upon general knowledge. Beyond a basic facility with language, it is true that the previous knowledge requirement of most tests is a rather minor component, but it only takes relatively small differences when large numbers are compared to yield statistically significant results. And, if these differences are even possibly due to environmental factors, everything should be done to elucidate this point before invoking basic biological theories.

It is, moreover, both technically and socially more fruitful to work *within* populations than it is to undertake comparisons *between* them when attempting to assess the heritability of intelligence. The range of potentially confounding features is very much reduced when one's interest is more closely focused upon the similarities and differences among members of the same family, and this is particularly so in relation to data deriving from those members who are the most genetically close. However, the literature on family concordances is massive and growing so rapidly that it would be nigh impossible to summarize it in less than a volume of its own. What one *can do* is consider illustrative cases which have been widely quoted as good examples of the conclusions acceptable to either predominantly genetically orientated or environmentally orientated sides in the argument, and then consider their various strengths and weaknesses.

One of the more publicized and celebrated cases for a substantial dependence of measured intelligence scores upon environmental

factors is the recent work of R. Heber and his co-workers* in the 'Milwaukee Project' – a longitudinal study of infant development which has given rise to claims that the IQ of young children can be boosted by more than thirty points as a consequence of environmental manipulations. Such shifts in intellectual capacity are of such a magnitude that they can hardly fail to have a certain heroic appeal for even the most disinterested observer, and the very circumstances of the study are equally appealing in that they suggest a much more humane involvement with the people concerned than do the characteristically detached data surveys of genetics supporters.

The Milwaukee Project is, in fact, a mixture of intervention and assessment: the experimental group comprises children of low IQ mothers drawn from slum areas – a combination of circumstances which is generally found to correlate with low intelligence in the youngsters themselves. The infants in the experimental group were, during the day, exposed to an 'enriching environment' provided by skilled mother-substitutes at the research centre, while their real mothers received one of their scheduled doses of instruction and counselling. There was also, of course, a control group which did not participate in these special treatments but whose function was to act as a baseline against which any improvement in the intellectual abilities of the research group could be judged. After four years or so, the anticipated differences were very evident, though of a magnitude much greater than all but the most sanguine might have forecast.

As has been indicated, the results claimed for the Milwaukee Project have been splendid, but they have not passed without criticism: specifically, Ellis Page† has considered some of its less lustrous aspects. Of these, perhaps the most damaging criticism is an admission by the project director, Rick Herber, that the infants in the experimental group had 'fortuitously' received training in some of the test items that were later used as a basis for demonstrating the difference between them and the untreated group of children. Considerable differences in performance are therefore not unexpected – though their cause may lie more in the sphere of learned behaviours than of intelligence *per se*. Another serious weakness in the design of the study was revealed by Page when he pointed out that there was evidence of non-randomization in the assignment of children to the experimental or the control group. Differences in physical and physiological variables showed a non-chance distribution, suggesting

* Paper to 2nd Cong. of Int. Assn. for the Sci. Study of Mental Deficiency, Warsaw, 1970.

† *Educ. Rschr.* (1972) **1**, 8–16.

directionally operating differences in the children's backgrounds. Clearly, the effect of such physical indicators is incalculable in terms of their developmental connotations and adjustive significance.

Of course, one should not necessarily conclude from the fact that weaknesses have been detected in the Milwaukee Project that they automatically disprove the findings. I for one would be very surprised if the stimulation and enrichment programme had no effect on the children's intellectual development, but the criticisms made do suggest scientific reserve and that it would be prudent to repeat the study elsewhere, taking account of whatever defects may have become apparent when retrospectively viewed from the privileged perspective of hindsight.

In fact, after monitoring the subsequent progress of his experimental group, Herber himself* has admitted that the initial increases in IQ scores have been substantially eroded with time, and that it is now apparent that the programme of environmental enrichment must be sustained if measured gains are to be maintained. A considerable credit balance remains, though many of the causes and effects are obscure. However, the marked variability in reported IQ scores certainly does tend to support the widely held view that the tests used were overly sensitive to educational achievement, and that future research must be particularly careful about this.

In social and educational terms, the effects of the Milwaukee Project are both hopeful and admirable, despite the fact that it does not seem to have made an equally important contribution to our understanding of psychogenetics. But, if the partisans of the emphatically genetic point of view are drawing any strength and comfort from the discomfiture of the more environmentally orientated, this must surely be much attenuated by their own recent experience of hard-pressed attacks on the methodological integrity of one of their own celebrated and influential studies.

Even so, the study in question still merits a fairly detailed treatment for reasons other than the inquietude it has brought to the ranks of the prominently genetically minded. For, whatever its faults, most of the results presented are still broadly compatible with the work presented by many other researchers of the definitely hereditarian kind. However, the reasons *why* such a consensus should occur are perhaps best set aside for the moment while we consider the study, the data, and the investigator himself.

* Prog. Rept., Rehabilitation and Research Center in Mental Retardation, University of Wisconsin, 1972.

These controversial research findings were published by Sir Cyril Burt,* and are, apart from their key significance and classic status within the literature, particularly pertinent to our broader purpose of outlining the story of psychogenetics – for Sir Cyril was himself a source of historical connections in tracing the development of the subject. Such links are, of course, of no particular theoretical importance but it may be of some general interest to note that the young Cyril Burt was acquainted with the pioneer psychogeneticist Francis Galton who, it will be remembered, was the kinsman and younger contemporary of that other great influence in this field, Charles Darwin. But, as we shall see, it was Burt's influence upon his own and subsequent generations which is of more direct scientific moment: a story which posthumously, and not flatteringly, equates some aspects of his own contribution with that of the great founder of modern genetics, Gregor Mendel.

Burt's scientific career, much more distinguished than that of Mendel in his lifetime, was as a psychologist working mainly in the area of education. His professional concern with the assessment of intellectual performance and social and educational achievement was, as it turned out, closely matched by his deep interest in biological processes, so his genetic studies were a natural choice of research activity from the start. Even before the First World War Burt was already interested, and publishing, in this area and so his experience and thinking on the subject had an unusually long period of time in which to develop and mature. But the advantage of having spent a long period of time working within a particular problem area has more than conceptual advantages. In addition, Burt's appointment in educational psychology for the London County Council meant that he was able to amass records of assessments made over many years, and for several members of the same family: to follow up the progress of children seen by himself and his colleagues and, in due course, to see many of the progeny in their turn.

Revealing as this may be, the most significant advantage of working on the staff of a local authority lies in the availability of records relating to the fostering and adoption of twins. As we have seen, Galton's co-twin control method confers by far the greatest degree of experimental rigour in human genetic research: an advantage which is greatly enhanced where the psychologist concerned has extensive background information about the parents and their families, and the exact conditions of placement. The study which we are now about to

* *Brit. J. Psychol.* (1966) **57**, 137–53.

consider was rooted in work which Burt had begun four and a half decades earlier, and to which data from the intervening years had been added.

The 1966 report presented Burt's own massive compilation of data and compared it with the results drawn from a large number of studies which had been published by other research workers. Average correlations were given for pairs of individuals of different degrees of relationship, reared together or apart. Despite the relative rarity of twins brought up in different and unrelated families, the numbers which had been collected for Burt's own study are impressively large. Furthermore, the twins had all been separated during the first few weeks of their lives and so the results could not have been confounded by shared early environmental conditions. Moreover, the actual circumstances of adoption were such that comparisons should not have been contaminated as a result of pairs being placed in similar homes; in this instance, no systemmatic policy of placement was pursued.

As an educational psychologist particularly well-qualified in the testing of children and partialling-out the relative effects of intellectual capacity and cultural acquisitions, Burt was specially sensitive to the need for a carefully chosen and broadly based assessment. Intelligence tests which are primarily verbal in character, though an excellent choice for some purposes, are highly susceptible to the environmental circumstances of a child's development, so Burt sensibly used a *battery* of tests and assessments which would give him separate measures of educational achievement and verbal skills, as well as thoroughly sampling the capacities of reasoning, analysis and synthesis.

The most telling relationships to emerge turn, as usual, upon the different concordances for the monozygotic (one-egg, or identical pairs) compared with the dizygotic (two-egg, or fraternal) twins. Taking Burt's figures, we see that the correlations of concordance (where 1.00 would represent absolute congruence and 0.00 a total lack of relationship) show monozygotic twins to be overwhelmingly more alike in their intellectual capacities than are any other members of the family. This is true whether the monozygotic pairs are reared together in the same home – where the average correlation was 0.92, or whether they were brought up apart after the first few weeks of their life, where the average correlation was still 0.87.

In analyzing the internal detail of his results, Burt was able to show very clearly that though monozygotic twins reared together are exceedingly similar in both intelligence and educational measures, those reared apart are very much less similar on educational criteria

than they are on intelligence test results. In fact, dizygotic twins reared together are more like one another on the educational measures than are the identical twins reared apart – though intelligence test measures still show the much greater similarity of identical twins, even when they are brought up in different homes. Thus the case was made, though not for the first time, that environment may substantially affect the performance of people with identical genetic endowments but that, within the range of normal circumstances at least, it is not sufficient to change fundamentally those underlying capacities which we technically refer to as 'intelligence'.

Dizygotic twins, because they arise from two separately fertilized eggs and thus have no more or less similarity than any other siblings, may be expected on the hereditarian hypothesis to resemble closely the pattern from ordinary brothers and sisters. And, indeed, the results do show that the correlation of intelligence scores for ordinary brothers and sisters is very much the same as for dizygotic twins reared together – i.e., 0.52 to 0.55 in the former instance, and 0.53 in the latter.

As has been noted, IQ correlations were reduced somewhat in cases where monozygotic twins were brought up apart, and the same proved to be so for ordinary siblings – amounting to a drop from 0.53 to 0.44. Not large differences, but sufficient to argue a modest case for the determining effects of home, school, neighbourhood and so on, in the development of intellectual capacity. On Burt's analysis, this environmental leeway was conceived of as being something less than twenty per cent of the total contribution to the development of general intelligence.

Fleshing out the bones of the genetic argument somewhat, Burt also presented results for other, less closely related, members of the same family. By the nature of things, such data are much more complex than they appear, as some relatives will be much of an age, though rather distant in their blood relationship while others, though genetically closer, are of different generations, and so the results encompass a great range of genetic differences, individual experiences and circumstances. Isolating the hereditary component under these conditions might be expected to be a most difficult undertaking; and so it would be if what is measured in IQ tests was mainly the product of environmental influences. But if it is under predominantly genetic control, then it ought to be possible to predict results by calculating the theoretically expected degrees of concordance from the known principles of genetics. Should these predictions be confirmed, it follows that they will serve to validate the hypothesis that the functions measured are indeed under genetic control.

With this in mind, Burt set out to make these predictions, and to compare them with the empirical data collected by himself and other workers. But the predictions depended upon more than estimates of genetic chance: they were also adjusted in the light of evidence which indicates that the inheritance of intelligence is associated with a slight but incomplete dominance, and that human beings do not select their mates in a random fashion. Rather, the evidence shows that people tend to make their choice of partner among others who are somewhat similar in certain relevant respects. For example, there is ample evidence that people are more likely to select their marriage partner from their own socio-economic stratum and, as occupation is correlated with intelligence level, matings are not made on the genetically random basis that is often assumed by simplistic arithmetical predictions.

Though such factors are not gross in their effects, Burt quite rightly argued that some form of arithmetical correction is a necessary step in expressing the equation which yields the theoretical values. The much weightier components of the equation are calculated on the assumption that the mode of inheritance is multifactorial, or dependent upon a number of genes for expression, and that more or less distantly related individuals share a calculable average proportion of genes in common. Thus we have 50 per cent of genes in common with our parents, 25 per cent with grandparents, and 12½ per cent with our first cousins: proportions which yield expected correlations of 0.50, 0.25, and 0.125 respectively – prior to being adjusted for any of the factors which Burt saw as likely to affect the predictions.

In the event, the predicted results accorded very closely with the empirical findings of Burt's studies. First-cousin concordances were the least close fit, suggesting the need for more intensive investigation, but the general picture was remarkably clear-cut. More than that, though, the results of the large number of independent investigations which Burt summarized in his paper, when taken together with his own findings, appear to make a very persuasive case for the multifactorial inheritance of most of what we assess as intelligence.

There is, however, a fly in the ointment. As with the Milwaukee Project, it has been claimed that Burt's study is seriously flawed and must therefore be set aside until new evidence is available. This time, though, the charge is not so much concerned with experimental design and control, but a fear that the source of the data itself might be suspect. Since Burt's death, other researchers have been scrutinizing his work in the minutest detail, looking for chinks in work which seemed to make an impregnable case for the predominantly genetic

control of intelligence – and they found them. One of the sleuths, Leon Kamin, has published his findings and conclusions in a recent book entitled *The Science and Politics of IQ*: it makes very disturbing reading. More recently still, Leslie Hearnshaw has set the whole controversy in the context of a fine biography and so added enormously to our understanding of the background of this whole area of research.

There are several ways in which Burt's work has been questioned, not least in relation to the published correlations of concordance and the size of the sample on which they were based. It appears that the correlations retained a stability through different publications of the data which was inconsistent with the variability of the reported sample sizes. Furthermore, the procedures for making his original assessments, and the correction factors that he introduced into his analysis, have proved to be so hazy that exact replication is rendered impossible. Kamin himself is the best source for appraising the criticisms made, but sufficient here to note that they do amount to a serious question mark over Burt's contribution to this field.

The opprobrium associated with Burt's case has done a great deal of damage to the whole subject of psychogenetics, as well as tarnishing the memory of a man whose energetic and remarkable career had been a model for others. Exactly what happened will probably never be clear: his advanced age at the time when he published his crucial twin study has been suggested as one factor; his blinkered and partial approach to the data another. Certainly he was always a votary of the quasi-aristocratic status of brilliance – a view which guided him in his development of 'Mensa', that exclusive organization for which the only qualification for membership is the possession of a high IQ.

There are, of course, even less charitable interpretations of the defects in Burt's study, though many believe that they spring from an unconsciously prejudiced handling and selection of the data, perhaps compounded by unknown collaborators who might now, naturally enough, be disinclined to enter the arena of public debate. But, whatever the truth about the perpetrator, it is the work itself which should command our attention; and this is where we come to the remarkable parallel between the results produced by Sir Cyril Burt and those of the Abbé Gregor Mendel.

Mendel's work was also critically appraised after his death and statisticians have since concluded that it is actually inconceivable that Mendel's results came out as he published them: the distributions are *too perfect*. The laws of probability indicate that it would be an almost unbelievable coincidence if, on a first test, the very variable empirical

outcomes which are now known to occur should match the theoretical predictions as perfectly as they were claimed to have done. On this basis Mendel, like Burt, has frequently been supposed to have adjusted his results to conform to his preconceptions.

The collection of data, by both Burt and Mendel, probably involved a good deal of practical help from others sympathetic to their aims and aware of their expectations: a situation all too likely to result in the introduction of some bias, conscious or unconscious, when borderline decisions had to be made in the classification of cases. Be that as it may, Mendel's results and conclusions have since proved to be a perfectly valid paradigm for modern genetics which perhaps means that, though we have undoubted cause for concern about Burt's work, we should not be too hasty and throw away the baby with the bathwater. After all, Burt is not the only source of concordance results such as we have been discussing: the many examples which he cites are also in very close agreement, and so too are many studies undertaken since then. The problem is, however, that Burt's own study was the only one in which the comparisons made between separated identical twins included the crucial data about the socio-economic circumstances in which they were raised. Without this information, even the powerful comparisons made of differences found in twins of both types, reared together and apart, are open to rebuttal on the grounds that any observed differences in intellectual development might equally be interpreted in terms of unidentified, but systematically operating, environmental variables. This loophole in the genetic case must have been a great source of irritation to most workers in the field, and a great spur to finding ways of filling it but, because of his remarkable access to information sources, the task fell rather naturally to Cyril Burt.

Thus Burt's study, though not exceptional in showing increasing concordance rates for intelligence with genetic closeness, became a cornerstone of the genetic case that socio-economic circumstances are of demonstrably minor significance in the development of intelligence. Other studies have consistently shown the relatively much greater IQ similarities between monozygotic and dizygotic twins reared together, but they now lack an authoritative study which could claim to show that genetic similarity will assert itself in spite of great differences in environmental treatment.

I mentioned earlier the place of Cyril Burt as a linking figure in the development of psychogenetics, but the full significance of his position is only fully revealed when considering the people and issues which he himself influenced. If one had to nominate the two most prominent and controversial figures currently operating in the sphere of the

inheritance of intelligence, one would certainly put Jensen and Eysenck very high on the list – and each has been strongly influenced by Burt's work. In both cases, the results of the separated twin studies have figured very large in the development of their own ideas, and it is upon this substructure that highly charged arguments about the presumed causes of both racial and inter-personal differences have, at least in part, been based. Moreover, these two influential thought-leaders have themselves communicated something of their own conviction and enthusiasms on the basis of what has turned out to be controvertible research. And, in doing so, they have added to the polemical atmosphere of this subject in a manner comparable with that of those whom they would criticize for promoting immediate public acceptance of contrary studies like the Milwaukee Project. So, whilst not wishing to overstate the significance which attaches to this particular case, a little more diffidence when attempting to specify the relative contributions of heredity and environment would seem to be in order: a period of careful stock-taking and additional research being obviously due on all sides.

What the Burt study *has not* done is to discredit the conclusion that intelligence has a marked hereditary component, anymore than will faults in projects like the Milwaukee one undermine predominantly environmental interpretations. As Eysenck himself has noted in another context, theories in the behavioural sciences tend to depend upon a *web* of interlocked evidence rather than upon a chain. Weak points in one's theories are therefore not of the same crucial nature as would be the case on the chain analogy, where the weakest link in the chain would define the over-all strength of the whole. Even so, there will need to be some time spent in re-working torn threads in order to make good the damage occasioned by recent events.

Of course, disruptive as they have been, the foregoing affairs should not distract us from trying to make sense of the very ample evidence at our disposal; data drawn from all levels of analysis – from animal, pathological, familial and population study sources – which, taken together, indicate the significant interaction of *both* heredity and environment. As we have seen, the evidence so far suggests a very substantial hereditary component in those studies taking IQ as a criterion, though clashes continue to occur over the appropriateness of test data as a measure of intelligence. It is argued, for example, that what is assessed by the tests is not a measure of intelligence *per se*, but simply a quantification of whatever intelligence tests measure! However, even if this were so, it would still be necessary to concede that they tap important psychological properties of one sort or another;

ones which, on the basis of the concordance data, would still seem to be of a markedly hereditary origin, and which are demonstrably very closely connected with intellectual performance.

This sort of criticism is obviously very difficult to sustain as all reputable intelligence tests have been required, during their development, to be validated by demonstrating a high degree of correlation with behavioural performances – particularly at school, or in terms of predicting later educational or occupational success. Pragmatically, such tests are excellent: overall, they give remarkably accurate forecasts of how well people will do in many important undertakings, despite great variability in their lives and opportunities.

Does this then mean that acceptance of the genetic basis of intelligence also implies pre-determination of the course of our lives? Maddeningly enough, the answer has to be both yes and no.

Should we have the misfortune to be born with an extremely low level of intelligence, then there is no way in which we can expect to join the ranks of the professions. These require not only the filling of a role but a successful handling of innumerable examinations, each of which would tax all but the most able in their demands for remembering information,analyzing, synthesizing, and generally engaging in the abstractions of reasoning and logical thinking. In other words, such opportunities demand an abundance of just those qualities in which the person of low IQ has demonstrated his relative lack. If the gap between a person's intellectual level and that required to succeed in a given pursuit is very large, no amount of effort and coaching can make up the difference.

For example, even given the most skilled help, some people will forever fail to learn how to read or write. In a sense then, we might say that such individuals are almost certainly predestined for a life of unemployment, or employment in some relatively uncomplicated form of physical labour. Those with somewhat more potential may be able to get as far as the more repetitive forms of industrial or office employment, but may soon reach their upper limit because of the actual complexity of the jobs, or because formal qualifications are required as a condition for advancement. So the answer to our question of whether a certain amount of social and occupational predetermination may result from our inherited intellectual qualities is probably, in most cases where they are very low, a qualified 'yes'. This situation is perhaps best thought of as a mental parallel to physical impairment – where people, whether their incapacity is due to a club-foot, defective heart valves, blindness, or whatever other cause,

must generally accept certain limits in their own performance when physical demands are great.

However, both by definition and empirical demonstration, by far the greatest number of us bunch together as the intellectually *average* – and within this range predetermination has much less effect. Towards the lower end of this range of distribution many people will have considerable difficulties with abstract thinking, but the majority have sufficient capacity to allow for very wide achievement possibilities. As we have already seen, although they are correlated, intelligence and attainment are distinctive qualities, and should be treated as such. No doubt we could all think of cases where interest and determination, with or without good education and environmental fortune, have been sufficient to bring about great success in the not especially gifted. Similarly, average or even exceptional gifts may result in failure where motivation or environment are negative forces.

Nevertheless, heredity obviously is a powerful factor both in determining our intellectual capacity and modulating our achievement potential – though gauging the magnitude of its effects with any degree of precision presents real difficulties. Estimates vary for a number of reasons but, as might be expected from what has gone before, one simple cause of inter-study variability lies in the extent to which the various intelligence tests employed in different programmes of research have been tapping learned achievement. The estimate of environmental influences in measured IQs is usually at its greatest when the tests used are especially dependent upon verbal skills and general knowledge. Conversely, the genetic element usually reaches its greatest magnitude where the tests employed most intensively tap abstract modes of thinking which are less dependent upon previous learning.

Then there is the matter of accounting for the fact that the coefficients of concordance for monozygotic twins are never unity: there is always some degree of difference in the intellectual measures, despite the pairs sharing a common environment and genetic programming. We have already drawn attention to the difficulty of being certain that apparently similar environments are not in fact significantly different in some ways and, earlier still, to the differences which might arise as a result of dissimilar intra-uterine or birth conditions. There is, however, yet another reason why not only twin concordances may fail to reach their expected levels, but why all concordance rates may be artificially depressed. This is because no test is perfectly *reliable.*

In other words, there is always some discrepancy between results,

even when the same person is re-tested on the same, or a parallel form of the same, instrument after even a brief interval of time. This fact alone is sufficient to account for a moderate depression in the concordances reported for identical twins, and so has been used to suggest that the hereditary component of intelligence measures may be even higher than might be supposed from the already very high concordance rates that have been published. On the other hand, the fact that the concordance rates for identical twins show a marked drop in cases where they have been reared separately suggests that environment and developmental circumstances are not without influence in the determination of intelligence.

No serious worker in the field actually demurs from the view that intelligence is a product of the *interaction* of heredity with environment, but there is earnest disagreement about the relative contribution each makes to the mixture. At least part of the problem is the basic difficulty in comparing or matching environments for, as we have seen, it is not even safe to presume the absolute similarity of environment in cases of identical twins brought up in the same home. And because the full range of potentially influential environmental circumstances can never be expressed in precise numerical values, it is not possible to achieve an exact figure from our equations and so broad estimates are necessary.

This is the crux of the matter: inexact measurement and poor concept definition is always likely to be a cause of vehement disagreement and acrimony even when, as in this case, opposing views tend to turn on judgements of magnitude and not upon fundamental differences. In the event, though we may never progress far beyond fairly rough estimates, these do have the merit of emphasizing the point that any calculation of the heredity-environment equation is always likely to be a relative index, such an index always being dependent upon the particular population sampled, the tests used, and the assumptions made in analysing the results.

More than this, it must always be borne in mind that the sort of computations which we make are very rough generalizations; averages which are made up of many, and very variable, individual cases. If, for example, we accepted an estimate of 75 per cent for the heritability of intelligence, we should be most careful not to slip into the way of thinking that this implies a genetic component of about three quarters in every case: our estimate is simply a composite figure which will vary to some extent from person to person. For each of us, the genotype will be more or less completely expressed depending upon our environmental fortunes. But, if we are to try and settle upon a fairly widely

agreed average value for our estimate of the inherited component, it is clearly not sufficient to refer to any single study, or even to any small group of studies based on the same test, or upon very similar samples. Rather, we must look for a *range* based upon the consensus of many well-controlled and somewhat heterogeneous investigations. A somewhat conservative estimate, based on the literature, suggests that this is probably of the order of 50 to 80 per cent – a range which, though broadly acceptable, is still indeterminate enough to permit ample scope for disagreement as to which end of the range is likely to be nearer the true mean. Either way, the contribution is a large one.

The import of these conclusions is not, as many people have hastily assumed, that our intellectual capacities are almost completely settled before we are born: what they do suggest is simply that we all have different potentials for growth if raised in a uniform environment. The enrichment or impoverishment of our environment can, and does, influence the course of our development, with the result that there is still a good deal of room for most of us to rise above our relative limitations. Of course we may from early childhood onwards either decide, or be compelled, to accept the indications of our relative position in the hierarchy of intelligence. But even the differences we see in the performance of genetically similar twins and other siblings raised together or apart speak voluminously for the significance of developmental circumstances and the way in which they measurably affect our capacity to store information and to process it efficiently through analysis and generalization.

In fact, it is not at all difficult to demonstrate empirically how substantial increments in mental performance can be achieved if intellectual interests can be fostered within us – and if we can sustain them through the focusing of our motivations, the improvement of our ability to concentrate, and the development of effective methods of study. The pay-off is not only in terms of knowledge accumulated, important as this is in determining intelligent behaviour, but also in terms of improved intellectual technique and the broad capacity to handle difficult problems. Gains of this sort generally presuppose learning opportunities and good teaching, but these are things which we can do something about, not only to raise the intellectual ceiling for the less able but for everyone.

As we have seen, the evidence of recent research is such that even the most doctrinaire supporter of environmental determinism could hardly deny that intelligence, or at least that quality which correlates so well with educational criteria and is measured by our IQ tests, is under a substantial degree of genetic control. But our destinies would

still not be preordained by our portion, even if each person's share was *entirely* a matter of heredity. Success and failure in life depend upon much more than the intellectual qualities which we can measure in arriving at our quotients. For example, creative and performing skills, whether artistic or more practical, are frequently the basis of not only successful but also intellectually and emotionally enriching lives. Intelligence test scores take slight account of these attributes which, in the normal range anyway, have very little to do with our measured IQs.

Most definitions of intelligence, whether technical or common-sensical, tend to assign some degree of importance to the capacity for generating successful adaptive adjustments to the environment. Unfortunately though, psychologists often lose contact with their touchstone of reality when they begin to devise the sort of tests which are calibrated with easily quantifiable activities – particularly educational achievement. Important as such talents are, they are not the be-all and end-all of human existence. The ability to make and maintain fruitful human relationships and to rear children well are not the prerogative of the highly intelligent, nor do they have a monopoly on happiness, mental health, or even the ability to make money: far from it.

In the end, then, the degree to which we are likely to accept that 'intelligence' is a hereditary characteristic will almost certainly be conditional upon our own beliefs as to its nature. The empirical evidence leads us inescapably towards the conclusion that those characteristics which are measured by the more culture-free tests, and which most thoroughly sample memory and the abstract abilities of analysis and generalization, are very significantly influenced by genetic inheritance. But the capacity to make successful adjustments in the real world depends upon the involvement of other mental characteristics too – aspects of temperament, personality and character.

All things being equal, a retentive and analytic mind should always confer an advantage, but there are many circumstances in life when a relatively greater share of other personality qualities – such as courage, perseverance, or a sociable and friendly temperament – will prove more adjustive. These attributes are something quite distinct from those measured in intelligence tests and so, in the following chapters, we shall again be posing similar questions in order to establish how far other personality characteristics are moulded by environment, and how far they are determined by heredity.

Chapter four

GENES AND NORMAL PERSONALITY: GENERAL FACTORS

In this chapter, and the one which follows, we shall be considering the extent to which normal personality may be dependent upon inherited characteristics. It is a topic which may be regarded from both a general perspective – by which we mean a focus upon those acquisitions which have a universal human relevance – and also in terms of familial inheritance. The two are obviously interlinked and interactive so, by separating them into two different chapters as has been done in the present instance, one is only seeking the equally related practical advantages of simplicity and moderately sized chapters. The effects of heredity upon human personality being at the very core of the whole subject matter, the alternative of attempting a greater degree of abridgement did not seem desirable.

For the present, then, we shall in the main be concerned with general issues and the inheritance of universal determinants, and with setting the scene for what is to come in the following chapter. So perhaps we should begin right away by considering what on earth we can mean by 'normal personality'.

Individually the concepts of 'normal' and 'personality' are hard enough to delineate but conjoined they present an even more formidable problem for handling in any concise fashion. Yet we may perhaps justify some degree of brevity and simplification in these matters as our present purpose is not so much definitional or concerned with theory-building as it is with establishing the credibility of certain of the relatively crude associations which have been claimed. The question of what is 'normal' is a marvellously complex area of logic, statistics, rhetoric and cross-cultural comparison. But, in the end, we are generally driven to accept the relativity of our ideas for what is normal in one time or place – for example, homosexuality in Ancient Greece or head-hunting in modern Amazonia may be seen as exceedingly abnormal in another place or time.

Just as cultural comparisons show that any particular pieces of behaviour abstracted from their cultural or sub-cultural context are, in themselves, unreliable criteria for determining an individual's normality or abnormality, so too statistical observations need qualification. In the previous chapter we saw that a score on an appropriately culturally-standardized intelligence test which falls substantially below 100 is usually the mark of abnormality. Yet a score on measures of intelligence, creativity, or any of a host of other socially valued qualities which may be equally deviant from the mean in a positive direction, is *not* a mark of abnormality. In other words, statistical deviation is only relevant in relation to the particular adjustment demands of the culture, or even to an individual's own particular circumstances. These few remarks hardly even hint at the complexity of achieving a satisfactory conceptualization of 'normality', though the guidelines of what constitute the typical range and distribution of psychological and behavioural characteristics within a specified society are at least a beginning.

Another way in which making a clear-cut distinction between normality and abnormality runs into difficulty is with those people who show minimal outward signs of abnormality but who, perhaps because of their feelings of depression or anxiety, seek treatment for their subjectively intolerable state and thus define their own abnormality. How many others suffer in exactly similar ways but continue to cope, and are thus apparently 'normal' we can only guess at. In fact, the great majority of psychological dysfunctions, and even the deviancies of many quite seriously impaired individuals, are subtle in manifestation and mainly internally experienced.

In the long run, then, we are generally obliged to adopt a notion of normality which is simply the residuum of those who have not been defined, either by themselves or by social and medical authorities, as in need of psychiatric treatment. And, though such a definition of normality falls very far short of the scientific precision one would like to employ, the realities leave us with few, if any, alternatives. Abnormalities of intelligence have already been referred to in chapter three and we shall have cause to deal with pathological developments in psycho-sexual differentiation in chapter six, as well as the more classical psychiatric concerns in the chapters which follow that. So, perhaps we may identify our 'normal' range as that which is not included under the other headings. And, if this seems to be a less than rigorous criterion, I must concur.

'Personality' also demands that we abandon rigorous criteria. Whatever else it may be, we can at least be clear as to what it is not: it is

not a *thing*, only an inference and a summation of certain heterogeneous observations. Regrettably tenuous though the concept is, psychology is far from the only science which has to tolerate inferential, as opposed to direct observational, knowledge. Indeed, as the subject matter of a science increases in complexity, so the inferential element seems to grow and the most advanced scientific areas, such as astro-, geo- and atomic physics, must all explain many basic structures and processes in terms of what may only be inferred from regular patterns of behaviour – whether celestial, volcanic, or the effects of accelerated particles. Personality, like sub-atomic particles, or indeed genes themselves, cannot be observed directly but must be conceptualized on the basis of behaviour in relation to various environmental circumstances.

Unfortunately the universe knows no more complex mechanism than the human brain, and one still cannot begin to imagine the intricacies of its transactions with the world about, so the number of inferences which must be made in the study of mind is consequently that much greater than in any other area of science. In the case of atoms, we can make models which we may feel confident are fairly accurate physical representations of what actually exists 'out there' in nature. But in the case of personality, we are operating within a much more abstract and imprecise realm. Our problems therefore are not only those due to the rudimentary state of our technology: it is the nature of the material itself which makes it likely that we must always depend heavily upon surmise and inference. In fact, it is often argued that it is psychology which will one day define the furthest limits for the scientific method. Not that scientific experimentation will not continue to prove applicable in many ways, but our progress in this area often makes it seem that the methods which have been adapted from the physical sciences may not carry us far beyond the study of physiological processes and a rather limited degree of statistical prediction. Whatever the future holds, there still remain plenty of intricate and important questions, particularly in psychogenetics, which lend themselves to the classical scientific methods.

Our focus of study is, then, the human personality – with all its ramifications. If we were to try and describe its constitution, I suppose that we would normally refer to such basic structures as an individual's intelligence, values, opinions, social and physical skills, aptitudes, motivations, temperament and so on, when considered as interacting with his own ever-changing environment. There are, of course, many other terms in the equation, represented in the individual both consciously and unconsciously, and different theorists would give

different weightings to the ways in which the components should be combined and interpreted. But basically we are dealing with no more than a handy if somewhat treacherous belief that these many qualities may be usefully combined and will yield a pattern with properties which are fairly stable over time and which may therefore be useful in making predictions.

Moreover, the sort of predictions which we make can be either about individuals or about groups, and may therefore be based upon assessments made of a single person or upon measures of a more limited range of characteristics derived from groups. Either way, prediction is still a hazardous business as it depends not only upon our own very fallible ability to assemble the whole range of relevant qualities and then measure them accurately, but also upon the workings of the enigmatic links which bind the whole together. Of course, not all students of personality are necessarily concerned with prediction, and some even scorn the idea that it is either possible or desirable. In some ways, this is true of many of the theorists we shall encounter in this chapter, though seldom in those to be encountered afterwards.

So our terminology is as equivocal as the aims of the theorists are diverse. For those who enjoy intricacy for its own sake, attempting to resolve the many meanings of the term 'personality' is alone enough to guarantee unlimited hours of delight. However, this present book is not concerned with theories of personality as such; only with the much more modest aim of establishing whether there is any acceptable evidence to show that genetic inheritance has any part to play in shaping those psychological qualities which help to determine our typical pattern of relationships with others.

Even to state this somewhat diffident aim is to invite having many doors closed smartly in one's face; and not entirely because of strongly held negative prejudices. There are equally powerful positive convictions at work in this sphere, some of which are encouraging towards this type of investigation whilst very many more are not. This divergence of opinion arises to some extent as a result of the influence exerted by such influential and productive individuals as Burrhus F. Skinner in the United States and Hans Eysenck in the United Kingdom.

However, whereas Skinner's type of investigation suggests that minimal importance should be attached to bio-genetic factors, Eysenck sees these same sort of processes as being of basic significance both in the understanding of the common qualities of personality and as an important cause of individual differences. Both points of view

have many active supporters and it has proved all too easy for contrary ideological positions to become entrenched. Yet it is becoming increasingly clear that many of the most fundamental issues in personality theory, and indeed in the whole range of social and behavioural sciences beyond psychology itself, will only be satisfactorily treated if a more close, critical and impartial attention is paid to the empirically-based research evidence which is now being generated within both the biological and psychological contexts. Happily, these two disciplines are nowadays tending to draw ever closer: a fact which is in part due to the influence of the third party, genetics.

Nevertheless, the exceedingly cautious, not to say cold, reception that most biologically orientated theories seem to receive from psychologists should not surprise us, for learned responses are quite demonstrably of major importance in the formation of personality. For at least half a century now, learning theory has been very successfully and elegantly applied to all branches of human adjustment, both normal and abnormal, and it has unquestionably produced a very pretty system indeed. The introduction of genetic variables was unwelcome to behaviourists because their acceptance would inevitably lead to the blurring of outlines in a system which is dedicated to the belief that both social and psychological control could (and should) be achieved through the application of a limited range of conditioning principles.

J. B. Watson's original boast that he could mould any sort of person from an unimpaired child is still an article of faith for many behaviourists following in the footsteps of B. F. Skinner, Watson's distinguished successor. And Skinner is still projecting this belief vigorously, as in his testament *Beyond Freedom and Dignity* and his best selling science-fiction novel *Walden Two*, which portrays a society in which the individual is little more than the malleable product of its own psychological engineers. In both books man is conceived of as an organism with general properties, whose genetic differences could safely be largely ignored by a system which stresses similarity and sees individual differences as predominantly a consequence of environmental shaping.

Not only do many psychologists and others hold that the technology described is quite possible, and in accord with our knowledge of the principles of learning which have been clearly shown to apply to the development of certain personality characteristics, but it is also widely admired as a model worth emulating. We should perhaps pass over this last point while noting, however, that if our review of the genetic involvement suggests any substantial contribution to personality

development, then we are in confrontation with a system which stands at the core of much contemporary psychological thinking.

But, alas, one must admit that the bio-physiologically and genetically orientated approaches to personality have, until very recently, generally given rise to much more fragmentary empirical efforts than those deriving from the animal laboratories of the behaviourists. In fact, because of their powerful attraction as a rigorous scientific alternative, some of these behaviouristic approaches, based upon the principles of classical conditioning as developed by Ivan P. Pavlov and the instrumental conditioning of Skinner, have done more to limit the development of genetic approaches than have most of the direct criticisms put together.

That the biological and behavioural perspectives have both suffered as a result of much quite unnecessary polarization is all too obvious. Protagonists on either side apparently wished to create their vision of man from their own basic axioms and prejudices – an unprofitable, if understandable, method relieved only by the emergence of some interesting conflicts between intention and outcomes. For example, a great many of the supporters of behaviourism are attracted to the system very much because of its total emphasis upon environmental conditions in the development of behaviour patterns – with all the seemingly implicit possibilities for the perfectability of mankind.

Yet Skinner uncompromisingly projects an image of man which is actually of a continuously programmed *robot* whilst, astonishingly, still enjoying the support of a good many 'liberal' or 'socially committed' people. This is all the more strange as so many of these behaviourist supporters, whether American Skinnerians or Russian Pavlovians, are often quite morally outraged at the suggestion that man's behavioural tendencies may sometimes be influenced in a general way by his inherited constitutional make-up. Reactions are sometimes even violent where it is being claimed that psychologically important structures and processes may be subject to the very same genetic fortunes which cause individual variations in colour of eye or hair, tallness, baldness, or any of the other readily observed inherited features.

In my own view, behaviourism represents not only an inadequate conception of man because it explains so little of his actual psychological activity, it is also a demeaning one. Not only does it fail to handle the problems of the unconscious, it even rejects our primary observations of consciousness, and particularly the will. Just as bad, in this present context, it opts to overlook the entire ethological perspective on our distinctive species characteristics, as well as the

genetic and other sources of individual variation. Clearly, value judgements are at the core of such a conclusion, but it still surprises me to find that so many of the same people who may reject psychogenetics for its (relatively mild) implications of determinism can also embrace behaviourism, with its total commitment to a form of automatism. However, I can only think that the social or personal acceptability of psychological theories drawn from biological observations must, in some degree, depend upon the extent to which they *seem* to de-emphasize predetermination – whether or not they actually do so.

It would be far too time-consuming to review thoroughly the development of ideas relating to the inheritance of personality characteristics, but a few paragraphs might be helpful in elucidating some of the long-standing sources of antipathy to views on the interdependence of mind and body, as well as suggesting something of the origin of several contemporary streams of thought. The danger is that it is all too easy to lose direction in the morass of ancient philosophy dealing with the themes of psycho-physical dualism and free will.

However, if we were to limit ourselves to just the *familial* inheritance of psychological characteristics we should find that the rigorously worked-out intellectual antecedents of this idea are considerably shorter than are the folk-notions, or the culturally widespread justification of rank by the supposed inheritance of character through 'blood'. There were, of course, other straws blowing in the ancient wind but, broadly speaking, it is true to say that scientific notions of psychological inheritance began to emerge most plainly from the writings and influence of Charles Darwin.

Actually, Darwin's contribution was particularly innovatory in that it was conceived and propagated within a society so philosophically committed to empiricism – and thus to the Lockean doctrine of the *tabula rasa*, or the child as a negative form which developed only by means of sensory experience and the most rudimentary of survival drives. In the event, the nineteenth century proved to be more impressed by observation than philosophical speculation and argument – though it turned out that Darwin's own speculations were widely admitted as part of the larger scientific package-deal. Whatever their merits or demerits, and most of his specific assertions about the inheritance of personality traits would not be well received today, the views developed in *The Descent of Man* and elsewhere proved to be remarkably seminal in his own time. And here one is not thinking only of such major protagonists of ideas as Thomas Huxley or Francis Galton but also of all the other less well-known personalities of

philosophy and biology who enhanced the evolutionary view of man's psychological and behavioural mechanisms: a process which has led to the emergence of theorists like Lorenz, who seek to explain the bulk of man's behaviour in terms of the evolutionary aquisitions of our species. We can trace similar influences on the thinking of many psychologists from Freud onwards who, though they have been more particularly focused upon the developmental causes of individual differences, have nevertheless started from the basis of less well-differentiated instincts.

So this is perhaps the first division of subject matter to which we should attend: the distinction between generally shared inherited qualities, the instincts or quasi-instincts, and other qualities of mind which indicate that our individual characteristics have been more particularly determined by our immediate progenitors than by our inheritance of a uniformly, or randomly, spread property of our species. In the former class, Desmond Morris and Robert Ardrey have been particularly vigorous in their popularization of new forms of instinct theory: accounting for the fundamental (and even detailed) nature of modern man by reference to his presumed evolutionary history and the survival pressures which would, again presumably, have favoured the conservation and growth of certain psychological qualities.

For Ardrey particularly, sovereign among these general qualities are the aggressive instincts which are triggered by any form of real or assumed territorial competition. Morris, too, makes a good deal of our origins, and thus our 'natural' selves – that is, we are still *really* territorial carnivores beneath our veneer of civilization. But, like many of the ethologists, he is also concerned with the inheritance of a whole gamut of things from sexual and child-rearing arrangements to the flexibility and exploratory behaviour which has resulted in the pre-eminence of our own, otherwise ill-equipped, species. In other words, an alternative type of psychology is emerging, based upon theories about the inheritance of a relatively small number of species-typical dispositions.

It may be somewhat vexing for psychologists to observe this emerging challenge to their own ways of thinking but, undeniably, our everyday experience of the common qualities which we all share, irrespective of place, time, or our stage of cultural evolution, speak eloquently for a 'human nature' which is only *modified* by our particular forms of social learning. We may differentiate as groups and as individuals, but we do so from a common starting-point. Infants born into one culture may quite readily be reared in another and

brought up to behave in a way which is quite identical with their fostering group, and almost unrecognizably different from their group of origin. Or at least they may sometimes *seem* almost unrecognizably different if viewed at the superficial level of appearances. But, as one probes a little more deeply, it soon becomes apparent that the things which seemed originally to mark such clear behavioural distinctions – like clothes, domestic conditions, spiritual beliefs, technical and scientific achievements, etc., – are only the slightly different methods by which groups strive to accomplish the same broad ends.

If we allow the reasonableness of this line of argument, we may find ourselves growing closer to the notion that the basic similarities of all human society strongly indicate the biological transmission of psychological and behavioural characteristics, just as individual differences are held to result primarily from the varying strength of our acquisitions *within* a given culture. However, this notion of biogenetic pre-shaping of our social and psychological natures is bitterly rejected by most psychologists at the present time.

Nevertheless, it may help if we consider the implications of the arguments currently being developed from ethological lines of thinking insofar as they relate to psychogenetics: and very challenging implications they are too, particularly if we take as an example books like *The Imperial Animal* in which two anthropologists, Lionel Tiger and Robin Fox, elaborate the old themes to a point where one must either take them seriously or else be prepared to take issue with them from their unjustifiable reduction of complex technical problems to superficial generalizations. That they are quite innocent of the scientific background to many of the psychological situations that they so engagingly simplify is all too apparent, but it would be a pity to throw away the baby with the bathwater without first examining it for signs of life.

Their 'baby', or rather their special angle, is that every species has its own *biogrammar* – a behavioural language or innate repertoire of actions which is so completely programmed that, if a human couple could survive in the absence of all cultural support, they and their descendents would inevitably produce a society which would be, in most essentials, similar to the ones we know.

Needless to say, this proposition is not based upon observation but supposition. The details of the argument need not concern us particularly beyond noting that the case is conventionally made in terms of a speculative view of evolution coupled with the assumption that man has not progressed in any fundamental fashion since he evolved into the successful hunter of the Upper Palaeolithic period.

Therefore, the argument runs, the culture which would be developed by our new little 'Adams and Eves' would be typical of that period of our evolution – just as all contemporary societies are also variations on that theme. And those of us who happen to find ourselves born into an advanced technological world are, so we are told, going to be very much preoccupied with the 'incarceration of our hunting selves', despite all our rational attempts to find other centres of meaning and value.

In other words, this is yet another statement of the ethologist's orthodox view that social behaviour and society, all societies, are rather superficial inventions shaped to serve our primitive hunting natures, the biological 'facts' of our evolutionary character ensuring that, whatever the distracting cultural elaborations may suggest, all societies broadly conform to the stone age hunter's 'reality'. However, Tiger and Fox's arguments (like all of their sort) cannot reasonably be ignored as they are not simply unsubstantial assertions, but interpretations of evidence. We may dispute the status of the evidence, the hypotheses generated, and the conclusions deduced from them, but they do bear on the interaction of nature and nurture far too directly to be loftily overlooked by the evolving science of psychogenetics. After all, behavioural coding is an everyday fact of most infrahuman species: each has its own distinctive biogrammar such that sheep invariably act in a 'sheep-like' manner, rats form their societies in 'rat-like' fashion, and so on. One can disrupt the behavioural patterns of young creatures by manipulating their environment and their opportunities to interact with and imitate others of their kind, but many behavioural routines will still emerge regardless. Behavioural programming in phylogenetically lower animals is not a matter of dispute. Biological mechanisms must therefore have evolved to code for such incorporeal outcomes. The question really comes down to whether *human beings* inherit such a behavioural programme to interact with the learning routines which we may so much more readily identify in action.

Actually, though, one generally gets the impression that proponents of the ethological type of argument have very little to say on the subject of *interaction.* Most often they seem to be intent upon prosecuting the extremist case that nurture (all that is learned or culturally acquired) is simply a more or less direct expression of nature. In so doing, they tend to overlook the vast body of scientific knowledge in the disciplines they claim to have revolutionized. Whether this is due to arrogance or ignorance is not always easy to discern but, not infrequently, the advocates of ethological theories explaining human behaviour have

contrived to be as psychologically naive as they are definite in their views. Well, they are not the first, and hopefully will not be the last to tread an innocent path through the complexities of other disciplines. And, as so often happens in such cases, the over-all result has probably been a considerable gain for all the human sciences. The rustic and hybrid vigour of ethology, while presenting certain dangers to those whose personal need for simplifications and unifying theories outruns their critical acumen, has nevertheless been something of a tonic too. A stimulant, though by no means a panacea.

Actually, 'irritant' might also be an appropriately descriptive word; at least judging from the agitation caused by an increasingly pervasive and sophisticated application of ethological observations. When Edward Wilson published his *Sociobiology: The New Synthesis* in 1975, he stirred up a veritable hornet's nest by lending his considerable academic authority to the growing notion that much of human personality, society and culture could be viewed in terms of evolutionary and genetic processes. And, though only the last chapter of this book applied directly to human beings, it was sufficient to focus attention upon these powerful new directions in science, several of which touch extremely sensitive ideological issues. The subsequent arguments and counter-arguments make fascinating reading and fortunately, the more important amongst these have been brought together in an excellent confection *The Sociobiology Debate*: a source which must surely be one of the very best starting-points for anyone interested in either the scientific or the ideological aspects of the subject.

At the present time, there is little sign of the controversy abating: indeed, the opposite is true. If there has been any chance of a lull, it certainly disappeared with the publication of Wilson's next book *On Human Nature*, in which he expanded upon some of his most challenging arguments and added some others for good measure. Central among these is an idea, which he shares with Richard Dawkins, author of *The Selfish Gene*, that even apparently altruistic behaviour can be viewed as the gene's way of surviving. For example, the bee or termite that sacrifices its life for the colony ensures the survival of the gene-pool from which it derives. So, the gene is prior to the individual, and it modifies and regulates behaviour appropriately.

In exactly the same way, it is argued, human parental, family, tribal and national behaviour reflects a good deal of this gene-directed personal service and sacrifice. Whereas classical Darwinian theory placed its emphasis upon the survival of the fittest, modern thought is drawing attention to the unconscious processes through which the

sacrifice or compromise of an individual's own interests might allow his genes an even better chance of survival in the large number of related individuals who also carry them.

So, the position has been reached in which a great deal of observational material has been marshalled to argue that much of what is fundamental to human personality – whether it connotes competitiveness, aggression, co-operative or altruistic behaviour – is influenced by the genes. Hence, as people like Wilson point out, though we have ample evidence that human beings depend to a large extent for their own adaptations on the learning and teaching of successful strategies, we must always consider the biologically coded and psychologically unconscious tendencies which may also operate to shape our actions. As with the case of sex-role differences, one can consciously and rationally override dispositions, but one should be aware of the processes, and never underrate their robustness: they may or may not be ideal guides for the present, but they have ensured the survival of our biological ancestors over millions of years and are thus not likely to be peripheral to our organization.

Theorists in this mould undoubtedly fulfil a most important function by continuing to remind us of the principles derived by Darwin and Mendel in the last century. The fact is that, of late, psychological thinking has become altogether too rarified, or at least overly concerned with the abstruse and miniscule details of adjustment whilst being too divorced from the mental implications of our common biological nature and adaptations. And, though one would be well advised to view the prototype psychological interpretations of ethological observations with some caution, they have at least offered alternative perspectives from which to view some of psychology's problems.

It may well be that, as more empirical work is completed and theoretical formulations become better worked-out, we shall find more substantive reasons to accept many of the ideas expressed. If it appears that mankind is indeed subject to generalized programming which, though less specific than one generally means by 'instinct', nevertheless predetermines a vast range of social activities – from language to initiation ceremonies, and from marriage rules to religions – then there is no doubt but that we shall have to pay more attention to the possible *mechanisms* of transmission.

Personally, I see no objection at all to making a start on these 'how' questions. But, while feeling that a presumptive case has been made for *some sort* of psychological inheritance, I find myself concerned as to the nature of the content and the assumed mechanisms of selection and

transmission. And this is one of the real causes for disquiet about the ethological type of argument; the time-scale allowed for so much of our present behavioural programming is painfully short to account for the evolutionary selection of those specific psychological changes which were supposed to have taken place during our interlude as Stone Age hunters. At least this is the case so long as we are continuing to think in terms of Mendelian principles rather than some unspecified process of acquired characteristics. In my opinion, this is one of the weakest links in the argument as presently formulated. Inherited psychological processes are not an unreasonable supposition, but their possessing a quite highly structured Upper Palaeolithic adaptive nature may be.

Closely connected with these notions of inherited action dispositions, though not always seen to be so, is the rapidly developing belief that language and the capacity to symbolize in certain fashions is innate. In this particular case, for many reasons other than the potentially greater time-scale that would be operative, evidence for this type of inheritance is currently much less suspiciously received by all concerned. Most importantly, of course, it is acceptable because it is often thought to imply very little about the predetermination of behaviour: a far from warranted assumption. Be that as it may, what is certain is that a new line of evidence is being developed to demonstrate that some of man's most basic psychological characteristics are in part innate, and thus presumably the subject of normal hereditary variability. This new line of exploration has already differentiated itself as *psycholinguistics* – and is concerned with the relationship between language and the users of language.

Many animals communicate, and some have wired-in programmes of calls and gestures which the young need never have heard or seen before in order to make them, or to respond appropriately to others making them. Insects, being so wonderfully pre-programmed, offer the best range of examples but birds probably hold out a more familiar and learning-related model of the process. Brought up without any opportunity to imitate others of their kind, many birds will still produce a very passable version of the calls of their species. But, in some species, the language developed by a young bird may become considerably modified as a result of its mimicking, more or less convincingly, the calls of alien species among whom it has been brought up. The variety of avian species produces a comparably rich number of combinations and permutations in this theme. Additionally, at the within-species level, there is also the phenomenon of 'local languages': distinctive patterns emerging in isolated breeding groups as the norms gradually shift. Clearly, the rate of genetic change in any

reproductive gene-pool is likely to proceed at a much faster rate in a small group than it is in the larger one where greater numbers produce a comparably greater stability.

So, at each of these many levels of expression, different species of birds can be used to typify the individual consequences of different imitative learning opportunities, as well as group effects like the genesis of 'local languages'. Together, they suggest something of the multiple biological processes which must be at work to account for the diversity of outcomes due to the interaction of biological determination and learning. And, once the biological possibilities have been demonstrated in these ways, we can scarcely ignore the question of whether they might have any parallels or homologues in human terms.

Well, quite obviously, particular languages are not subject to genetic inheritance in human beings: whatever our origins, we learn the appropriate linguistic skills of our milieu or country of adoption. Indeed, so self-evident is the fact that we have to learn a language that it is still sometimes quite difficult for people to think of the process as anything but one of our many learned cultural acquisitions. Even the genetic programming which is a necessary condition for our highly specialized adaptations of the larynx and those parts of the brain controlling language skills seldom suggests the idea that there may also be an operating programme built into us all, in just the same way that a structural developmental sequence is built in. And so it might have continued whilst the study of world languages was mainly focussed upon the *differences* which exist between them. For generations, students of comparative linguistics had, by marvellously sophisticated means, gradually elucidated the evolution and chronology of languages. And, ultimately, with this understanding came another: the differences and divergences may be far less fascinating than what is implied by the similarities. After all, it was found, every known human language could be codified within a single system and could thus be shown to possess certain universal properties.

The comparative linguistic aspects are important in themselves as they suggest a common denominator in the working of all human brains, regardless of particular cultural acquisitions. But of perhaps even greater interest has been the dawning realization that an individual's mastery of language proceeds at a rate which is quite inexplicable without recourse to some notion of pre-preparedness. This relates not so much to what is involved in the learning and reproduction of a vocabulary, but rather to the ways in which the tremendously elaborate structure and potential of language is so readily developed. Features such as these strongly suggest that language is to

some extent innate, and thus neurologically 'wired' for certain types of behavioural activity. As Noam Chomsky, one of the pioneers of psycholinguistics summarized it...

> We must postulate an innate structure that is rich enough to account for the disparity between experience and knowledge, one that can account for the construction of empirically justified generative grammars within the given limitations of time and access to data (1972, p. 79).

Thus the case is beginning to emerge that language, one of the most fundamental components affecting our psychological worlds, and so our personalities, is in some part an inherited capacity; and, just as our intellectual gifts have been shown to depend very much upon the variable genetic packages received from our parents, so too may the inherited structures and biochemical processes mediating language play a significant part in the development of this important substratum of personality. In this sense, then, the perspectives of familial inheritance, as well as species characteristics, may be appropriate.

In this we may see a parallel with the case of those processes which, for practical reasons, we somewhat arbitrarily conceptually isolated from all other reciprocal psychological functions as 'intelligence'. Actually, of course, language and intelligence are so closely associated that, under normal conditions, it is often difficult to make any satisfactory distinctions: but then intellectual qualities are also an inseparable part of our personality. And indeed we may trace another close connection here, in respect of the developmental aspects of processes unfolding and growing. For, just as Chomsky has developed the idea of language capacities having been inherited largely in the form of a mode of mental functioning, so Piaget has attempted to show that intelligence too is not simply a matter of inheriting cognitive structures but also of inheriting a *modus operandi*. In other words, neurological systems also have associated with them programmes of operational sequences which are subject to genetic transmission.

Furthermore, the evidence which has been adduced in relation to the linguistic and symbolization characteristics of man goes far beyond having just cognitive relevance. The operating characteristics of these functions are obviously intimately bound up with all of our processes of judgement and decision-making, and thus of our wider personality characteristics. And, at the very broadest level of influence, the brain's capacity and proclivities for language and symbolization not only makes the development of culture possible, it quite certainly also helps to form the shape of that culture. Thus, these apparently internal characteristics form part of a much more general interaction between ourselves and our environments.

One final way in which developments in psycholinguistics are related to our contempory re-thinking of the question of human instincts derives from a methodological analogy. It having been shown that universal features may be inferred for all language forms because they can all be classified within a single common rubric, some of the anthropologically orientated ethologists have found it equally profitable to elaborate the principle. They have done this by pointing to the way in which apparent cultural and psychological diversity can also be categorized and set into a universally applicable schema. Thus they argue for a generically common behavioural acquisition (a mode of adjustment which is inherited by us all in the form of something like Tiger and Fox's 'biogrammar') and which is invoked to account for the more standardized aspects of our personalities. No doubt we shall witness the emergence of alternative views about what is fundamental in man's innate psychological programming but, for the moment, we seem to be fixated on certain aspects; ones which are well enough summarized in Tiger and Fox's own words ... 'agricultural and industrial civilizations have put nothing into the basic wiring of the human animal. We are wired for hunting' (p. 39).

We may be forgiven if we decide to reserve our judgement on this matter which, if true, would imply a genotype which under favourable conditions would always result in the personality phenotype they ascribe to the hunter. The matter of female 'wiring' is in any case one of the perennial problems of this type of argument as it raises conflicts between our theories and our experiences. But this is a matter we can safely leave to the next chapter. Psycholinguistics itself may or may not conjure alarming spectres for modern psychology, but some of the current ethological arguments certainly do. Until quite recently, most contemporary psychologists would probably have said that the subject of human instincts was quite definitely a dead letter; that the confusions of the past had now been revealed as no more than primitive reifications, semantic artefacts, which covered certain pre-scientific archaic ideas. The older instinct theories of personality, as developed by say Freud or McDougall, who also assumed a 'human nature' and the genetic transmission of psychological characteristics from generation to generation, had been pretty well dismissed from modern thinking.

Or rather, to be more exact, the authors of these ideas had become dated and unfashionable; their connections with instinct often being one of the major causes of their neglect. Of course, there were generally several other reasons why most of the old theories were discounted, but high on the list was a shift of opinion in what should constitute

acceptable investigative methods in a subject which was ever more being seen as grounded in the principles of learning. Terms such as 'instinct', which could not be operationally defined in human terms, and an increasing taste for reductionist experimentation based upon models derived from the physical sciences, had all but resulted in the total dominance of behaviouristic systems. Yet the dynamic characteristics of the human personality could not be encompassed so easily. Though not highlighted, broader concepts were still required and so have persisted as 'instinctoid' or, more often, as 'primary drives', 'innate needs', 'survival needs', 'biological determinants', or whatever. And so, though it is not really fair to suggest that ethological ideas are now threatening to re-introduce, *de novo*, discarded ideas of instinct into psychology, it is making psychologists face up to an issue on which they are rather fearful of being drawn. And this is particularly so if it might mean making an unequivocal statement of position: sitting on the fence can become as habitual a posture as any other.

Of recent times, this sort of confrontation and challenge has often been neatly avoided, for, unhappy as psychologists generally were about the scientific status of instincts, those who found some such concept inescapable discovered that many of the otherwise attractive instinct-based theories of personality could still be salvaged with a little ingenuity. In the main, this has simply boiled down to the process of describing the putative innate processes in a more psychologically acceptable language. McDougall's specification of individual instincts and their associated emotions has so far proved too specific for much re-working but Freud's formulations have left more elbow room. His *life, death,* and *sex* instincts have already proved suitable for this sort of reconciliatory effort and it has turned out to be quite possible to substitute the original terminology for a seemingly much more scientific and modern version, while still achieving a fairly satisfactory re-statement of the original ideas. As Neal Miller has shown, the task is not too difficult: replacing the 'instincts' of the 'Id' by the 'primary drives' of the 'organism' is hardly more than an exercise in re-naming as the substituted terms refer to virtually the same observations and inferences as those described by Freud. Even so, this was enough to make the whole system seem very much more attractive to those who had been troubled by the way in which the biological concept of 'instinct' invoked the ideas of behavioural predeterminism which are so empirically justified in biology and so controversial in psychology.

Yet though these conciliatory efforts may sometimes appear to be little more than disingenuous ploys for retaining indispensable, if unpalatable, concepts they have also served the useful purpose of

maintaining necessary conceptual differences between the biologists and the psychologists by highlighting real dissimilarities in the way in which common terms have been used. There is no reason to suppose that because two disciplines have historically used the same term that they must have had exactly the same referent in both contexts. The psychological processes were, arguably, only superficially similar to those observed in animal biology. Thus it makes sense to select an alternative terminology rather than squabble over the 'real' criteria involved. After all, each discipline is concerned with a different type and level of observation, analysis and explanation, and its concepts should reflect what is most salient and useful for that discipline; and terminological distinctions do help to avoid misunderstandings, just as cross-disciplinary comparisons and analogies so readily cause them.

But, however much one adjusts the terminology, the sphere of instinct remains a difficult one for the psychologist. Whatever sympathy he may entertain for theories which evoke such ideas, the modern psychologist is likely to have been trained along rather specific experimental lines. For many, the methodology defines the subject, and experimentalists are quite properly careful in their approach to innate characteristics for, though stereotyped behavioural *acts* yield satisfactory empirical criteria when studying such simple creatures as insects and other lower animals, the long childhood and unique possibilities for learning encountered in the study of human beings inevitably means that we can only study *tendencies*, not acts. And, as tendencies must be inferred from very variable acts, the psychologist is naturally made somewhat nervous by the subjectivism which he may introduce into his data. But, though he does well to be cautious, the alternative is simply to deny or ignore what most of us might feel to be true – that mankind has indeed characteristic qualities which, though they may vary in their expression from person to person and from circumstance to circumstance, nevertheless represent the basic instinctive substratum of human energies and orientations.

The mixture will vary relative to the particular aspect or situation under consideration, but personality can only ever be thought of as an outcome of the many interactions between nature and nurture. Understandably, because biologists have a greater familiarity with the nature aspects whilst psychologists are more at home with the workings of nurture, both have tended to overestimate the contribution of their own favourite element in the equation. As a result, the theoretical models with which we work are generally lopsided and partial towards the field of study within which they were generated. Psychogeneticists, coming as they do from either or other disciplines,

are now trying to integrate these views in a number of ways, and this is provoking some new thoughts on the old question of human instincts.

If general behavioural dispositions, whatever we may choose to call them, *are* innate in man, they must inevitably influence man's personality, and not only in a general sense; they may also be expected to have a *qualitative effect* on the individual. This would arise from the variety of moulding forces to be found in both the subject's personal environment and the cultural milieu generally. Together, they would act by differentially moulding the phenotype. Moreover, the genotype itself can be expected to carry that degree of variation which is typical of all forms of genetic coding.

It seems, then, that personality theories of the future may have to pay much more attention to both native inclinations and genetic processes than is currently usual. It may even be that the species-genetic approach will ultimately prove to be a useful corrective to a focus which is primarily set upon the quirks of individual social adjustment. Regrettably, though, we shall have to curtail our discussion of the extent to which instinct theories are relevant to psychogenetic theorizing: the field is just too wide for a satisfactory brief account. But, though our own attention must be limited, there seems no limit to the rich possibilities for scholars and scientists interested in reconciling personality theories with the viewpoint of biology and evolution.

However, there are some theoretical formulations like that of Carl Jung, which present such extraordinary difficulties that one may wonder whether such an attempted reconciliation of the biological and psychological content is even worth attempting. That one would try at all is a mark of how important these ideas may be for the subject of psychological inheritance. But, if the psychologist is disturbed by the biological features, he is likely to be no less so by their psychological content. Jung has, in the main, chosen to ignore the methods and findings of scientific psychology in favour of the illuminations to be derived from his own type of 'depth analysis': an eventuality which makes it particularly difficult to judge or integrate his conclusions. Yet though idiogaphic approaches of this sort create problems, one can nevertheless make a case for them in terms of the many valued insights they have yielded for those interested in understanding individual dynamics, abnormal and normal; and though Jung's methodology may not have led to a body of scientific data, nor to a scientific theory of the sort most scientists would accept, his ideas have commanded respect – if for no other reason than that they have been based upon a vast amount of observation and remarkable erudition.

In probing the human mind, Jung found that the dreams of his patients revealed recurrent themes which are echoed in the legends, mythologies and cultures of peoples in all times and in all stages of social evolution. Common elements occurred even when these connections could in no way be accounted for through learning or cultural influences acting upon the individuals under study. The unconscious mind as revealed principally, though not exclusively, through dreams seemed to contain a stock which is common to all men. This discovery, arising from his scholarly delving into cross-cultural comparisons of the dreams, myths and the wider aspects of cultural symbols and institutions, led him to conclude that all men share a *collective unconscious*: psychic dispositions which go far beyond the service of basic survival and reproduction, and which are organized into units which he terms 'archetypes'.

These ideas of the unconscious mind, the archetypes, are held to represent an inborn potential for having the same sort of experiences as those laid down by our ancestors over hundreds or thousands of generations: they are distillations of the human experience. Inevitably, these common features are both earthy and ethereal, concerned with the cycle of mortality as represented in the experiences of sex, birth, the struggle with the environment and death, and with the cosmological, theological and magical belief systems which were universally fashioned to structure the universe. The archetypal images involved are not only supposed to provide some sort of model for understanding the world and life itself, but are also held to comprise a series of symbolic guides which influence our behaviour towards our parents or children, members of the opposite sex, leaders, and so on.

Jungian psychology is exceedingly elaborate and one would not wish to become involved in any extensive discussion at this stage but the point should be made that there is, at least potentially, a most important nexus between this still influential psychological system and psychogenetics. And, if the system has any substance at all, the relationships cannot be ignored by anyone interested in developing a wide-ranging approach to the subject.

Of course Jung's ideas, being grounded so firmly in his studies of arcane religion and mythology, psychic phenomena, dreams and madness, are hardly very attractive to scientific investigators of the more sober behavioural world. But, despite his novel sources of data, Jung pointed out with some asperity that the theories do not depend upon the objective reality of such things as gods or whatever else may be represented as an archetype. Rather, they depend upon the *psychological fact* that generations of human beings have interpreted

their experiences in certain ways, and that there are thus predispositions to continue to see the world in terms of these repeated images: these aspects of the collective unconscious are passed on by heredity as well as by cultural means. The difference is that the archetypes of the collective unconscious are the universal qualities of the human mind whereas the culturally transmitted ones are only local variants which express the same ideas in different symbols. Yet, according to Jung, through dreams, visions and other non-normal levels of consciousness we may all connect into the common experiences of our species which, whether we are ever aware of them or not, nevertheless help mould our lives.

In other words, it is being claimed that the psychological features characteristic of the mental content and processes of all human beings may be transmitted from generation to generation in much the same manner as one's anatomy and physiology. The great difference is, though, that the transmission of collective or racial memories in Jung's sense requires a non-Mendelian genetics to account for it. It requires a Lamarckian type of process, in which characteristics acquired during the lifetime of parents could, if they were sufficiently often repeated, become stabilized and passed on to their progeny. This is a possibility which we considered in the first two chapters: the scientific objections to this were well known to Jung. Yet he stuck to his assertion that the 'facts' which his method of psychological investigation disclosed were only interpretable in terms of some sort of theory of acquired characteristics. It seemed to Jung that though conventional genetics might be unimpeachable in most physical applications, it should only be regarded as reflecting *a* truth, not the *whole* truth.

Nowadays we have become more aware than ever before of the many ways in which peoples, isolated from one another geographically or temporally, resemble one another. Moreover, the regularities to be found both in human nature and its cultural and conceptual artefacts are such that notions of cultural diffusion may seem quite inadequate, Kon-Tiki-type demonstrations notwithstanding. Until recently, apart from a few psychologists and anthropologists, psychological qualities were seldom thought of as being due to processes of a collective unconscious: such an explanation would generally have been regarded as untenable because of the lack of objective evidence. But, if the rules of scientific rigour do not change, tastes *do* in both the creators and the consumers of science. For whatever reasons, the fact is that we are now witnessing the rapid growth of a new generation whose interest in non-normal states of consciousness is leading to a re-discovery of Jung, to a restoration of his theoretical fortunes, and to a sympathy with his

concept of the collective unconscious: a sympathy not yet justifiable in orthodox scientific terms.

Nevertheless, some of Jung's ideas relating to inherited psychological characteristics are now beginning to find more scientifically substantial support to add to the introspective confirmations claimed by the exponents of the meditative and psychedelic movements. At a less dramatic level than the collective unconscious itself, one might think particularly of theorists such as Gordon Allport and Abraham Maslow who have apparently successfully followed Jung in adopting a teleological explanation of man's behaviour. That is, the view that present behaviour is only *partly* explicable in terms of the causal influence of past events. Like the more recent versions, Jung's teleological view also claimed that the present can only fully be understood by reference to the creative urges towards 'self realization'. These yearnings, tendencies and dispositions are unconscious and generally inchoate processes by which our innate nature makes itself known, and through which evolution itself works.

Of course, despite a gradual acceptance of some of Jung's ideas (like the attitudes of introversion and extraversion to which we shall be turning in the next chapter), currently it seems that most of what Jung wrote is destined to remain as an arcane system of more value to the cultural anthropologist and the world of literature than it is to the behavioural sciences. And yet one should never be too confident in dismissing differently formulated ideas because they do not fit in with our own mould, especially when they are the creation of brilliant scholarship and life-long experience. In science, the currently orthodox is only ever a stage of development: a more or less adequate explanation which has, in every case before, crumbled because of the exceptions and observations which have led on to new discoveries which cannot be accounted for in terms of existing theories. Even the mighty 'laws' of physics and astronomy have not proved exceptions to this much greater rule.

Naturally, the teleological and dualistic implications of self-actualization and, most distinctively, the quasi-Lamarckism implied in Jung's description of the collective unconscious – with its genetically acquired archetypes, primordial images and behaviour patterns – are the main reasons for scientific rejection. And yet, even with this apparently insurmountable barrier to orthodox acceptance, a certain amount of support has nevertheless begun to develop. One might almost say that the 'biogrammar' described by Tiger and Fox is only another way of partly expressing the archetypes and the collective unconscious. And even the teleological problems of self-actualization

may become more scientifically acceptable now that they are beginning to be expressed in terms of genetically-coded biological drives manifested through the unfolding of species-typical developmental sequences. This aspect of enhancing the respectability of one formulation by cloaking it in the terminology of a more acceptable one is, of course, not without parallel in the case of the Freudian instincts.

To summarize, then: in the course of this chapter we have looked at several of the many lines which have been proposed to argue the case for a limited type of psychological inheritance. Specifically, we have been concerned with questions concerning the broadest kinds of acquisition, those which, though perhaps quantitatively variable, are qualitatively represented in all mankind. As we have seen, the existing evidence leaves a good deal to be desired and, in some instances, apparently conflicts with scientifically better established principles of genetics. However, the more general impression conveyed (to me at least) is that the evidence available is suggestive, and that it certainly encourages the further examination and refinement of hypotheses which link generalized human qualities with evolution and heredity.

Chapter five

GENES AND NORMAL PERSONALITY: FAMILIAL INHERITANCE

'Well, she's got her mother's looks alright, but she took her father's temper.' How often have you heard variations on this theme? Certainly I must have listened to such comments and explanations hundreds of times. Sometimes no more is meant than that the children have come to 'take after' one or more of their family, coincidentally or through imitative learning. But it is often quite clearly being asserted that a child has inherited some, if not all, of its personality characteristics from recent or distant members of its ancestral line. Evidently the notion of inherited psychological features is not as alien as we might sometimes suppose: at least not when people are responding directly to their everyday experiences rather than trying to conceptualize the actual connections between the chromosomal strands within a fertilized egg and the later social behaviour of a human being.

Of course, the fact that such homespun ideas are commonplace and widely accepted has nothing whatsoever to do with whether they are true or not. It was once absolutely obvious to all but a few aggravating scientists that the earth was flat, and that the sun orbited the earth. Nevertheless, if people are to abandon a belief which seems to accord so closely with their 'common-sense' experience, the reasons for doing so must be better than just arguing that common sense has been wrong before. Strong arguments will be needed to shake this particular belief because it is an everyday sort of experience that members of the same family frequently exhibit really marked resemblances to one another, both physically and temperamentally.

In fact, it is helpful to begin with just this idea in mind as it is undeniably the case that closely related individuals do have a greater tendency to resemble one another physically: the question is, though, whether there additionally exists some sort of connection between appearance and personality. If so, this might at least suggest some kind of common link which might connect our psychological characteristics

with our genetically influenced physical constitution. It is, after all, a very venerable idea that mind and body should be linked: a notion celebrated in fable, poetry and drama, in such terms as

> Would he were fatter! But I fear him not. Yet if my name were liable to fear, I do not know the man I should avoid so soon as that spare Cassius. He reads much; he is a great observer and he looks quite through the deeds of men: . . .

When Shakespeare put these words into the mouth of Julius Caesar he was expressing a belief which was already well systematized in Ancient Greece: antique observations and surmise which have passed through several less than admirable stages to emerge as modern scientific theory.

We need not concern ourselves with the history of this idea, or with its more discredited ramifications – systems like *physiognomy* which sought to identify character from the face, or *phrenology* which pursued similar aims through the 'bumps' of the head. Sufficient to note that the association of physical appearance with psychological characteristics has not emerged into the twentieth century without the creation of a good deal of antipathy. In fact, so vehement have been the criticisms, and often so justifiable, that it is still difficult to introduce the issue in some quarters. Even so, the relatedness of appearance and personality have so regularly been reported that it would be intellectually dishonest not to consider the evidence.

Here again we find that the content of the chapters tends to overlap: in the present instance because physical typologies have such strong connections with theories of psychopathology. This is particularly so with respect to the German psychiatrist Ernst Kretschmer, who may justifiably be thought of as the father of modern scientific typologies linking mind with bodily form. What he accomplished was a demonstration of how membership of one of his three main body types – which we may very crudely typify as fat, muscular, and thin – related to a differential susceptibility towards such mental disorders as manic-depressive psychosis, epilepsy, and schizophrenia. In fact, however, Kretschmer did not himself develop the concept of the three main physical types; this had been done by his predecessors in the nineteenth century. Nor can he take the credit for developing the more rigorous scientific approach of making exact measurements and applying statistical methods to the correlation of derived mental and physical types – most of the credit for this must go to a later group of American psychologists led by William Sheldon. Kretschmer's own unique contribution lay in his admirable clinical observations of

certain body-mind relationships which have been refined, but not made obsolete, by the work of his successors.

We shall return again, in chapter seven, to the work of Kretschmer and the significance of such work for the understanding of mental abnormality but, for the present, we shall restrict ourselves to more recent attempts to correlate body-type with *normal* personality. Consequently, we shall be particularly concerned with the work generated by Sheldon's methodological advances, and with the criticisms to which this pattern of research has become heir. The position is, broadly speaking, that Sheldon and his associates have claimed to have isolated certain major factors of physique and temperament, and that they have demonstrated the existence of certain very marked correlations between qualities of mind and physical constitution. The question is, though, are these real and psychobiologically-based relationships or are they, as some have argued, no more than artefacts of the research assumptions and procedures employed.

Naturally, people do not come in discrete physical categories; continuous variation is the rule. This fact had not escaped Sheldon – as it sometimes does the critics of typologies – and what he was looking for was a limited number of factors or dimensions in terms of which he could measure each individual and compare him with all others. From a consideration of the nude bodies of thousands of male, and subsequently female, students, the anthropometric parameters of measurement were finally adduced. The procedure was very ingenious, depending upon making a host of exact measurements from each of the three specially standardized photographs – taken from the front, the side, and the back of each subject. Emerging from these studies came Sheldon's conviction that anyone could be individually typified by reference to their placing on a seven-point scale for each of the three primary components of physique – *endomorphy, mesomorphy* and *ectomorphy*. Very crudely, these may be typified as soft and fat, powerful and muscular, and thin and fragile.

Thus a sequence of three figures could be used to define the *somatotype* – an estimate of morphological characteristics relating to the comparative prominence of each of the primary physical components, the order *endo-*, *meso-*, *ecto*morphy being conventionally followed in expressing a somatotype so that a sequence of 262 would, for example, represent a predominantly mesomorphic (muscled) type; a 651 would be predominantly endomorphic (fat), but would also be an above average well-muscled type, too. By contrast, a 227 would have little fatty or

muscular development but would be markedly ectomorphic (linear and fragile) in his basic development. All of us are a mixture of these three basic components, even though we may not predominate very much in any one of them.

In similar fashion, Sheldon made ratings of the temperament of his subjects and again found that statistical analysis revealed the existence of three major clusters of traits which he termed *viscerotonia, somatotonia* and *cerebrotonia*. With a degree of crudity to match the one which I have used to typify the physical types, we might also respectively stereotype these as orientated towards food and people, vigorous activity and dominance, and self-consciousness and solitariness. These too could be rated on a seven-point scale, in which 1 is equal to the minimum presence of the component, and 7 denotes the maximum value. Of course, Sheldon's own descriptions of the qualities involved are very much more comprehensive and meaningful and should be consulted in the original by anyone interested in this line of research – as should they by anyone moved to criticize it on the basis of what they have read here. However, our present purposes should be reasonably well served by these admitted simplifications.

From our own point of view, the real fascination of this research lies not so much in the correlations which have been claimed to exist between the mental and the physical qualities, but in their implications regarding genetic inheritance. If the correlations are proved to be very large, and unless we can find an alternative environmental explanation, the genetic determination of both physical and mental characteristics might seem to be a foregone conclusion. And indeed the relationships claimed have been very impressive: in 1942 Sheldon reported the following correlations for a group of 200 male college students, where 0 would represent a total lack of relationship and 1 an absolute correlational match.

Endomorphy with Viscerotonia	$r=0.79$
Mesomorphy with Somatotonia	$r=0.82$
Ectomorphy with Cerebrotonia	$r=0.83$

As may be seen, these values indicate a massive degree of correlation between bodily form and temperament. In consequence, they have been a bloody battle-ground for the past thirty-odd years.

What Sheldon has claimed – that fatness tends to go with a sociable, easygoing nature; that powerful physique tends to go with an active and competitive nature; and that thinness tends to be associated with shyness and an inhibited nature – is not likely to strike most of us as being very improbable or hard to believe. I suspect that

most of us would have grouped these characteristics together on a 'common sense' basis without benefit of any prior exposure to constitutional theories. One might not expect the associations to be an invariable rule (which is getting towards what Sheldon's results suggest), but one does expect the associations to occur. This being so, it is not surprising that much disagreement turns upon the *degree* of association rather than upon whether any exists at all.

The real bone of contention is the likely *cause* of correlations of such great magnitude: an issue just as relevantly viewed from the methodological perspective as from the psychobiological. For example, Sheldon's figures have generally proved to be much higher than those of other investigators and this has often been accounted for in terms of probable methodological contamination – perhaps the result of Sheldon himself being party to the rating of both physical and temperamental traits. One need not labour the well understood hazards of unconsciously coming to see what one expects to see, or of permitting a common denominator in the form of a rater who makes both of the measurements which are held to be independently assessed. Such points are well made, and may well account for correlations which, compared with more recent studies, seem somewhat inflated.

In Britain, R. W. Parnell has been responsible for some of the most wide-ranging and large-scale research which, though it follows methodologically fairly closely in the footsteps of Sheldon, has also developed the scope of application in a number of interesting ways. For example, he has explored the relationship of body-type to academic performance and the student's choice of subject and profession – and, even more important, has made significant contributions to the study of somatotyping in childhood and old age.

Space limitations make it impossible to refer to more than a few of Parnell's studies, but I found it particularly interesting to see how body-type related to the refreshingly objective criterion of choice of university faculty – and thus of occupation and style of life. These studies, involving nearly three thousand students attending the University of Birmingham during the years 1952–56, produced a rich crop of statistically significant results. For example, student engineers and doctors tend to be muscular types, while mathematicians, lawyers and honours arts students tend to be more linear. There were also apparent typological *disinclinations* – such as the linear types not going in for mining. In fact, a number of stereotypes emerged as seemingly better grounded in fact than fancy!

Other investigators have produced results which differ more or less

substantially from those of Sheldon: few report negative results but almost all have found that their degrees of correlation were very much lower than Sheldon's. In his book *The Structure of Human Personality* Eysenck penetratingly compares many of these investigations and teases out their weaknesses and unjustifiable assumptions and, in fact, argues for a radically different factor-structuring of the extant data. But, though his treatment of the results and methodologies of somatotyping research is uncompromisingly tough, even Eysenck is finally left with correlations of body-type and personality which, though much more modest than Sheldon's, still need some explanation.

Sheldon's own explanations of the data are somewhat indefinite, and even rather heterogeneous, depending upon the sources consulted. He does, however, propose a genetically-based theory concerning the ways in which the genotype (which cannot be observed directly) influences morphological, or gross bodily, development in the interactions with environment. But it is fair to say that Sheldon seldom regards the environment as having a particularly important determining effect under normal developmental circumstances – and not necessarily so even under quite severe circumstances of starvation, dietary anomalies, or accidents of congenital or later origin. Just as Sheldon argues that starving a mastiff will not create a poodle, so one cannot expect an underfed mesomorph to become an ectomorph. Many morphological characteristics persist despite considerable environmental variations.

His argument proceeds upon the assumption that the various body-types result from differential embryonic development; specifically, the genetic unfolding of our bias towards one or another of the three major embryological layers from which the body develops. These layers, the ectoderm, mesoderm, and endoderm are respectively concerned with the development of skin and surface features, the musculature, and body fat – and, of course, it is these which give their name to Sheldon's physical types of ectomorph, mesomorph and endomorph. Thus psychological characteristics are ultimately linked with embryological development – a genetically determined process which, in turn, is dependent upon the individual differences in instructions borne by the personal genotype.

In addition to the three main types, Sheldon has also drawn attention to other, secondary, properties of the somatotype. Of these, the most objective is *dysplasia* – a more rigorous re-working of Kretschmer's concept referring to the degree of consistency or anomaly to be found in a single body. An example of this might be the

muscular trunk of a mesomorph coupled with the fragile limbs of an ectomorph. Less objective, though still mainly rooted in the ratios of anthropometric measurements, is the index of *gynandromorphy*, referring to the extent to which a member of one sex physically resembles members of the other. This might, for example, be manifested in a male through the prominence of such feminine characteristics as rounded hips and buttocks or small and delicate features. The third, and most subjective, of these auxilliary components is *texture* – a global aesthetic judgement of an individual's total configuration, including attention to such aspects as facial features, texture of hair, or of skin.

Taken together, these indices can add a good deal to our over-all description of a person's bodily form and might therefore be supposed a welcome additional dimension for analysis. Sad to say, though, there is a tendency to omit these secondary characteristics from studies: presumably because their more subjective nature is likely to attract heavy criticism and perhaps jeopardize work which is more difficult to fault in its major aspects. But, for whatever reasons they are omitted, it seems fairly obvious that these qualities are likely to have a good deal of effect upon a person's social transactions and fortunes. Moreover, they are, at least in very large part, genetically determined – like any other physical characteristic.

Reference to being treated according to our appearance opens up the whole question of how far we may, or may not, be considering the genetic inheritance of personality. Sheldon himself was somewhat equivocal in his conclusions – as indeed would be any prudent person in view of the several attractive casual possibilities. It may be, for example, that one's physique, including the more subjectively assessed secondary qualities, is essentially facilitative in developing the typical mental characteristics which correlational studies suggest are related to certain physical types. To possess a body which is large and powerful is, for a man at least, to open up some opportunities for social development which are not so easily achieved by the small and frail. If one is in the latter category, physical aggressiveness is a less realistic adjustment than would be the development of a cautious and reflective temperament. Failure to see realistic limitations might be expected to result in frequent punishing, or certainly non-rewarding, outcomes whereas adjustments which were more congruent with one's physical endowments would be more often rewarded – and thus conditioned as part of one's personal style.

The sphere of delinquency, where the individual is frequently in aggressive competition or conflict with others, is one where a more

robust physique might prove an advantage – though it will not necessarily prevent one from being caught! Empirically it transpires that such an association exists: Sheldon himself studied a large sample of male delinquents over a period of years and found them to be substantially higher on mesomorphy and lower on ectomorphy than his reference group of students. Over the years, many studies have replicated these findings for both males and females and, though the magnitude of correlations differ, most of them have revealed a clear association between mesomorphy and delinquency.

Similarly, the woman who wishes to play the sex kitten or the sophisticate is likely to find it more hard-going, and perhaps not worth the effort or even trying, if she is short and dumpy. However, as our own everyday experiences attest, the rule which seems to be suggested – that a particular bodily form will result in a particular personality or temperamental type – is by no means an invariable one. At one time or another, we must all have come across the little dictator, fiercely demanding respect and prominence. In such cases, Adler would say that a real or assumed organ inferiority had led to neurotic over-compensation – the alternative reaction of mobilizing excessive psychic energy to offset an intolerably felt sense of inferiority. Whether this is common or not, and whether the ploy fails much more often than it succeeds, it certainly does happen, thus underlining the richness of human adaptive potential and the intricate interpretative possibilities of constitutional theory.

This shaping of personality adjustments through our transactions with other people and with our own physical limitations and assets may, of course, also involve additional pressures to conform to a social stereotype. For example, if we believe that fat people are basically sociable and uncompetitive, we may conduct our encounters with them on this basis and thus help make our expectations come true. Tall and beautiful blondes are always saying just how difficult it is to be taken seriously and, as the alternative is not always too disagreeable, many of them give up the unequal struggle against social preconceptions and follow Confucius' advice!

Sheldon is very sympathetic towards the 'social stereotype' casual explanation which, though he would not give it an undue prominence, he accepts as a contributory factor, and in no way contrary to the notion that personality differentiation also depends upon certain genetic givens. The working of these inter-personal and intra-psychic determinants are accounted for as *indirect expressions* of the genotype, rather than the direct operation of genes which work equally upon the shaping of physical structure and psychological characteristics.

Nevertheless, his conception of an indirect effect is of a very powerful and predictable form of determinant – as his correlations suggest. For example, he regards the particular circumstances of childhood as of minor significance in most cases because the temperamental and physical differentiations occurring at each stage of development are so often the *cause* and not, as it may appear, the effect of environmental circumstances. Thus the endomorphic adult is not the result of over-eating in childhood; it is that the endomorphic child naturally emphasizes eating and the kind of adjustments which are associated with the endomorph's life-style.

The question of whether physical appearance is a stable characteristic anyway has been hotly disputed. If it proved to be only a function of exercise and diet, and to be very variable over the years, much of the force of the constitutional argument would be lost. The constancy of the somatotype is therefore a key issue. The matter is not, however, easily resolved as it implies long-term studies, and these tend to be extremely unpopular because of the practical and financial difficulties they create. As a compromise, Sheldon himself has followed-up several hundred of his subjects after a period of years, re-somatotyping them and looking for signs of change. Changes there were; middle-age brings its own unlovely spread of fat, or even 'scrawniness', but Sheldon claimed not to have observed a single convincing change of type, despite the superficial outcomes brought about by changes in diet, metabolism and exercise. Gaining or losing weight may make it look as though the individual has changed but, surprising though it may seem, the measures upon which somatotyping is based are not much dependent upon subcutaneous fat or slackness in the muscles. In the absence of more persuasive counter-evidence, it would appear that we must accept a high degree of stability over time despite the fact that many critics have expressed their own doubts as to the invariable constancy of these physical components.

Predictions which might test the assumptions of somatotyping would be so much easier to make if we could classify children accurately. Unfortunately, children's growth patterns and their 'spurts' are variable in timing and duration and thus so too is the relative development of bone, muscle and fat. Consequently there is a tendency to rely heavily upon correlations between adult personality and physique, a valuable but far from ideal method of tracing the possible genetic links. As always, what we really need is a massive amount of information about the outcomes of matings taking place within normal families over a period of many generations. Once again, though, we have to accept less than optimal conditions, but at least the

pilot study undertaken by Parnell has done something to open up this field. His sample size is sadly rather small, consisting of only forty-five healthy families – though this has been complemented with data from some 2,000 patients undergoing mental treatment. However, the patient group presents a more complex methodological and interpretative picture and so perhaps we would be wise to devote our limited space to the smaller group of normals. But, for anyone particularly interested in the wider aspects of the study, Parnell's book *Behaviour and Physique* will provide a wealth of additional material.

The first intriguing result to emerge from the study of the forty-five families is that the choice of marital partner seems to be connected with physique – and thus, perhaps, with temperament. It appears that muscular (mesomorphic) men and women tend to favour one another, though they also compromise this choice for other attractions. Furthermore, such women tended to have more children, whereas the least muscular men tended to have smaller families. Most curiously, Parnell also found that sex-ratios of the children born seemed to be related to the physical type of their parents. Two muscular partners tended to produce an excess of boys; a non-muscular and rather fat pair produced an excess of girls, and linear males did not influence the equations at all. These sex-ratio results were also supported by the data from the much larger patient group.

So, body-type seems to affect not only the individual's personal adjustment but also his or her choice of mate, their fecundity, and the sex of their children. This latter point may seem a little difficult to understand in genetic terms as a woman's body-type could in no way influence those events in the male which produce an equal number of sperms carrying the X and Y sex chromosomes. What *may* happen however is that, depending upon the woman's constitution, the structure or metabolism of her body may be more or less favourable to the foetus depending on its sex. We need not dwell too long on these problems beyond noting the existence of this further suggestive evidence as to the far-reaching effects of heredity upon a child's development.

The really striking contribution that Parnell has made is to compare the somatotypes of children with that of their parents. Forty-five families is all too sparse a sample, yet the trends which emerged were remarkably interesting. He found that if he graphically plotted the somatotype of individual mothers and fathers, in up to three quarters of all cases their children could be plotted on a line which ran between the two. This phenomenon, which Parnell called the 'Parental Line Principle' proved to be statistically significant, at a modest level.

Now the 'Parental Line Principle' must still be regarded as highly speculative, and certainly not as a 'principle'. However, the work so far is helpful both in delineating an important nexus between constitutional theories and psychogenetics and in highlighting a potentially most important growth point in research. If the 'principle' proves to be substantial, then our understanding of the processes by which personality may be linked with body-type, not only in a general way but also in terms of direct familial inheritance, will have taken a large step forward. Only a considerable amount of research will settle this issue, it being particularly desirable to test predictions made from parental body-types as to the somatotype of children yet unborn – and, ideally, to pursue these studies through several generations. For the present, though, we must content ourselves with such limited research as we have, together with the fingerposts which point the way forward.

A final reckoning has yet to be made with regard to the linking of body with mind, and in respect of the part which familial inheritance may play in determining individual differences. The claims currently being made may well prove to be much exaggerated as the correlations presented often seem to account for much more of the psychological variance than studies of other kinds suggest ought to be possible. Most psychologists would not argue against the existence of some such linking of somatic and personality characteristics, but they would be unwilling to accept that the evidence presently available has made a stronger case for claiming that constitutional variables are, in some sense, *basic* to any thorough understanding of people's psychological adjustment. Sheldon is, of course, not of this number. Instead, he favours the idea of the human body being to the psyche what the skeleton is to anatomy – a basic point of reference from which to elucidate more subtle structure, and the framework upon which characteristic forms are differentiated. But, if somatotyping studies do indeed comprise the anatomical groundwork for psychogenetics, we must needs look elsewhere for our physiological guidelines.

If we provisionally accept the hypothesis that the correlations between temperament and physique are probably not completely accounted for through the workings of such things as stereotypes and their interaction with social rewards and punishments, then we are required to consider the nature of the possible *physical mechanisms* mediating the psychological variables. The difficulty is, though, that such is the range and possible source of the individual differences to be found in relation to any and every human physiological process, that progress would be slow indeed without a theory to guide our

investigations: a rationale is undeniably necessary for concentrating on what can only be a fraction of the innumerable possible processes which may influence personality characteristics.

Theoretically-based studies of the relationship between genetically mediated biological variations and personality outcomes are rare, but they are on the increase - like the one directed by Gordon Claridge in Glasgow. Claridge and his associates have been exploring theoretical relationships which go far beyond the modest aim of studying twin pairs and relating zygosity to personality characteristics - though fortunately for us they do this too. The theoretical aspects of this work, though innovatory to some degree, are in large part modifications and developments of Eysenck's ideas. So, rather than risk tedium and confusion by discussing both accounts in equal detail, it may be profitable to place the greater emphasis upon Eysenck's more firmly established work. Yet, despite the pressing necessity of having to make selections in this overview of the field, serious students of personality would also be well advised to look at the excellent monograph which has been published by Claridge's team. For the moment though, we must content ourselves with a very brief account of their findings, rather than with their wider theoretical speculations.

However, it should be mentioned in relation to the Claridge work that the correlational material at its core was specifically developed to uncover the *processes* by which many of the determinants of personality are mediated, and the *degree* to which they are affected by heredity. Also, the theory - being of the Eysenckian variety - is a nervous typology and thus attempts to reconcile our present understanding of the nervous system with our knowledge of genetics and personality. It almost goes without saying that the physiological mechanisms postulated in such theories are themselves only provisional hypotheses, even though they presently fit the known physiological facts rather well. Nevertheless, the nervous system is so immensely complex, and so little understood, that we may expect the continuous modification of all theories with the passage of time: the question which we cannot answer in advance is whether these changes are likely to be mainly refinements or whether they are more likely to represent complete changes of direction. At the moment, the various typological theories, though differing in many details, share a considerable amount of common ground, whether they originate from Pavlovian or western laboratories. At root, they are all concerned with how the nervous systems control the balance of excitation and inhibition affecting mental activity.

Intercorrelations of personality and physiological trait measure-

ments indicate that the great diversity of phenomena at the observational and descriptive level may be reduced to a very few co-ordinating processes. Terminologies vary according to the theorist – for example, Pavlovians place their emphasis on concepts of 'weak' and 'strong' systems; Claridge favours 'tonic arousal' and 'arousal modulation'; whilst Eysenck prefers to work with his own major parameters of 'extraversion', 'neuroticism' and 'psychoticism'. Of course, they each refer to somewhat different things, but beneath the many superficial differences lies a basic similarity in the way in which their common problems are conceptualized. One would not wish to minimize the differences inherent in the various approaches, but a book on psychogenetics is not the best place to become overly involved in some of the most recondite of contemporary psychological issues. Instead, we shall briefly look more specifically at the differences Claridge's group found to exist between MZ twins – the one-egg or 'identical' pairs – and DZ twins – the two-egg couples whose hereditary similarity is no greater than that of ordinary brothers or sisters.

The subjects for this study were volunteers in the age range 16 to 55 years, and consisted of forty-four MZ pairs and fifty-one DZ. Their zygosity, i.e. whether they were really MZ or DZ, was minutely checked and each type was compared and matched for their similarity of background circumstances. At the same time, intellectual abilities were extensively tapped, revealing the expected pattern of greater similarity in the MZ twins. But the real interest of the project centred upon the relationship of measures to be derived from self-administered personality tests or inventories and physiological measures of known personality correlates.

In the former category, two out of the four inventories derived directly from Eysenck (the EPI and the Sociability/Impulsivity Scale), and a third from a closely connected factor analyst, Raymond B. Cattell, whose 16 Personality Factor Scale was used. The fourth test – the Foulds Hostility Scale – was casually included and, though it proved to discriminate between MZ and DZ twins, factor analysis showed that this was largely due to the test also sampling aspects of extraversion and neuroticism – qualities adequately assessed by the other tests and indeed comprising the very essence of the EPI. Nevertheless, the results suggested that certain aspects of hostility may be under some degree of genetic control.

Of course it is not possible to go over all of the results in detail, and it is sufficient to note that though measures of anxiety and neuroticism failed to display more than mild associative evidence of hereditary

control, extraversion – and most noticably its component of sociability – did seem to be markedly determined by genetic effects. It also emerged that the personality similarities observed in twin pairs were to some extent related to the age of the twins on testing and to the number of years since they ceased to live with one another. Such environmental effects, though of great theoretical interest and a source of caution to those who believe it possible to compute accurate indices of heritability, nevertheless were not of great magnitude and did not change the general picture. The characteristics studied indicated a substantial amount of genetic influence, even after the authors had allowed for age, length of separation effects and the several other considerations which they discuss in connection with the methodological problems of twin studies.

As has already been mentioned, the physiological measures were chosen because of their already established relationship to certain personality characteristics, particularly those connected with the individual's state and properties of arousal. One of these tests, the 'sedation threshold', involved the injection of barbiturate into each of a pair of twins whilst they performed the simple task of doubling the value of numbers presented to them. Their threshold was determined by the point at which they failed to be able to respond: at this stage the injection was terminated and the dose recorded. The results were extremely clear in their genetic implications – the differences between DZ twins being massively greater than in the case of the genetically identical MZ pairs.

The significance of this finding, from a personality point of view, is that it refers to the individual's arousal characteristics and thus to the integration of major personality dimensions such as extraversion, anxiety, neuroticism and psychoticism. At this point it would be tempting to get more involved with the nervous typological theories as such, and to relate the research findings to presumed underlying physiological systems. However, the researchers have themselves done this elsewhere and our present purpose is served quite well enough by the data, in that they plainly reveal hereditary components underlying generalized response characteristics of undoubted importance to personality functioning.

The more wide-ranging investigations of physiological response also bore upon this aspect of nervous arousal. Measures were many and varied, but amongst the battery of individual tests was included an electro-encephalographic recording of the brain's 'alpha wave' activity and the subject's heart rate. Both indices were recorded at rest; the results showing such marked differences between MZ and DZ pairs as

to suggest a strong degree of genetic determination. However, unlikely as it may seem to those of us impressed by the scientific appearance of physiological recordings, it is nevertheless true that such data, obtained from active and reactive subjects, are much less constant in their manifestations than are the more conventional psychological measures which are so often criticized for *their* lack of stability. Accordingly, the analysis of this kind of physiological recording is far from straightforward, so the results must needs be teased out of the data by advanced mathematical techniques. Raw differences between twin groups may stand out quite clearly without much help from statistical procedures, but such treatments do help to identify the presence of any generalized linking component which may easily be overlooked when treating the significance of test results taken one at a time. It can also help to weed out the quite alarming artifactual effects which may be encountered.

In the present instance, factor analysis revealed four main factor groupings; that is, common components shared by several tests. However, though the exact nature of these constructs will be of greatest interest to those wishing to compare the theoretical treatment of personality dimensions by Claridge and Eysenck, they also concern the present discussion as one of them – 'arousal modulation' – appeared to be subject to almost complete genetic determination. But, because these factor names only have meaning in terms of the tests which emerge as being inter-correlated, it must be left to the interested reader to explore this line further. Sufficient for the present simply to note that what Claridge referred to as 'arousal modulation' refers to the arousal characteristics of the brain as assessed by, for example, EEG recordings of the brain's electrical activity.

The Claridge study has gone quite a considerable distance in this sort of exploration of genetic involvement in the response systems underlying generalized personality factors. In so doing, considerable objective evidence has been amassed to show that many of the personality, or associated psychophysiological, tests tap far more basic and diffuse qualities than those represented by individual traits alone. This is encouraging, although, as we have seen, the actual number and nature of these underlying determinants will require much more exploration. It seems to me that Claridge and his co-workers have yet to develop their case far enough to suggest the modification of Eysenck's much more broadly-based factor theory, but their twin studies have undoubtedly added substantially to our knowledge of the hereditary basis of certain personality characteristics.

To summarize satisfactorily Eysenck's factorial theory in a few lines

would not only be extremely difficult, but also probably completely redundant for most people reading this book. However, as our backgrounds may well vary quite considerably, perhaps it would be in order to refer to one or two aspects which are most obviously connected with psychogenetics, whilst particularly recommending to those not familiar with the work, *Personality Structure and Measurement* and its companion *The Biological Basis of Personality*.

Working as he does in an Institute of Psychiatry, it is often mistakenly supposed that Eysenck is principally concerned with mental disorder and that his concepts are more related to pathology than to normality. But though it is true that Eysenck has made many material contributions to psychiatry, he has also made many more to general psychology through his use of diagnosed mental patients as members of *criterion groups*, or clear and extreme cases used for definitional and reference purposes. There are, of course, marked quantitative differences between the psychological tendencies manifested in the test results obtained from members of the normal population and those in the psychiatric criterion groups, but research shows that there is no absolute qualitative break, only a gradation of differences from the most impaired to the most robustly 'normal'. What factor analysis has shown through the examination of this distribution is the presence of continua, or what Eysenck refers to as 'personality factors' or 'dimensions of personality'.

Eysenck's factorial analysis of vast quantities of both psychological and psycho-physiological test material has consistently led to the identification of three higher-order factors which have been termed 'extraversion', 'neuroticism' and 'psychoticism'. These latter two are perhaps unfortunately named as the qualities tapped by them do not, as their titles so strongly suggest, apply only to the mentally ill, but to us all, however 'normal'. Patients of varying clinical severity tend more or less towards the extreme limits, whereas the normal and stable stretch along the continua towards the opposite end, with most of us spread somewhere in the mid-range.

Similarly, extraversion is also one end of the continuum which has introversion as its opposite pole. Most of us lie somewhere between and, once again, clinical patients have proved helpful in establishing the criteria for distinguishing the extremes of extraversion and introversion, and for eliciting the relationship of this factor to the operation of others. For example, Jung long ago made the observation that neurotic patients of the hysterical type are likely to be extraverted, whereas neurotic patients of the depressive and anxious type are typically introverted: a conclusion which has often, and in many ways,

been confirmed since. Thus, if one compares the scores of, say, diagnosed hysterical and depressive or anxious patients on tests purporting to measure extraversion-introversion, the results should indicate significant differences between them if the tests work, and if the wider underlying theory is correct. In the event, the several tests which Eysenck has developed all make this discrimination, and so the measurement of extraversion may be regarded as criterion-based in relation to another well-established theory.

Of course many other criteria, derived from normal as well as neurotic subjects, have also been used over the years, and have resulted in exceedingly robust theoretical constructs and methods of measurement. An account of the various ways in which factors were originally identified and subsequently verified, like the process of developing ever more precise tests to assess them must, regretfully, be left for further reading. But it is worth noting that, even though many researchers would dispute the wisdom of Eysenck's decision to base his theories upon so remarkably limited a number of higher-order factors, rather than extracting a larger number of descriptively useful factors or traits, fewer would deny the sheer conceptual strength of the constructs which he has chosen to emphasize. Moreover many, like Cattell - whose own factor analytic treatment has been used to create a much broader spectrum of descriptive traits, have also extracted factors very similar to Eysenck's as part of their account of the major components of personality. And because so much common ground exists between theories which have used different tests and analytic techniques, we may feel more justified in selecting the heritability of such psychological constructs as extraversion and neuroticism for special consideration.

Eysenck's elucidation of the biological basis of his proposed constructs, irrespective of whether it proves to be substantially justified or not, is nevertheless remarkably satisfying because it quite specifically identifies the activities of two different nervous systems as modulating the psychological manifestations of extraversion and neuroticism. Supporting evidence for this hypothesis is drawn from a wide range of empirical sources and is used to show that the qualities which (his own) personality tests measure as 'neuroticism' are closely related to independent physiological measures of autonomic nervous system activity, whereas scores of 'extraversion' are more closely related to the activities of the central nervous system. It transpires, for example, that neuroticism measures derived from factor analytic studies relate to such other more directly observable behaviours as strength and duration of psycho-physiological stress reactions, i.e.

indices of heightened responses of heart-rate, blood-pressure, sweating, etc. Similarly, measures of extraversion-introversion relate to such things as perceptual vigilance, flexibility of perceptual sets, and persistence.

The inter-correlational evidence, though by no means always unequivocal, is held by Eysenck to support strongly the assumption that personality can very largely be understood (and even predicted in group terms) on the basis of these two major dimensions of extraversion and neuroticism – and that such data clearly point to their originating in the central and autonomic nervous systems respectively. The web of evidence for this is extremely complex, depending upon observations made in neuro-anatomy and physiology as much as in psychological research. But, though details of the hormonal and structural mechanisms proposed as mediators cannot be treated here, it would seem that our present physiological understanding of their operating characteristics is quite compatible with the roles assigned to them in Eysenck's account of personality integration. For example, the *reticular formation* seems to have many of the neurological properties required for the regulation of extraversion and introversion. Research on cats had shown that a slight electrical stimulation of this network of nerves situated in the central part of the brain stem had the effect of greatly arousing a sleeping animal. Further investigations led the researchers to re-name this area the 'reticular activating system' (RAS) because their results indicated that it was the centre which modulated the activity levels of higher brain centres. In fact, it transpired that cortical areas were totally dependent upon its activity for their being in a state compatible with awareness. Without RAS input the individual, human as well as animal, will relapse into unconsciousness.

In man, the reticular area of the brain stem had long posed unsolved questions as to its function. About the size of a little finger, it is composed of a dense network of nerve fibres which receives input branches from the main sensory nerves passing upwards through it to higher centres, and also has its own nervous processes which ascend directly to the various areas of the cerebral cortex.

With all the wisdom of hindsight, we see that the reticular formation is ideally constructed to fulfil the purpose of a sensory filter and modulator. Incoming sensory signals are monitored from the branches coming from the main sensory fibres: the reticular system then acts as a 'censor' – suppressing some inputs by means of its own inhibitory signals to the cortex whilst boosting others by the production of additional facilitative activity. Plainly, there must be a continuous dialogue between the upper centres and the reticular

formation so as to modify one another's switching states. The details of this activity need not concern us at present, though the process naturally relates to Claridge's 'arousal modulation'; and its connection with 'extraversion' makes it doubly relevant to our inquiry into the heritability of personality determinants.

In Eysenck's view, and now in the view of many psychologists who would not consider themselves to have much more in common with his theories, one's relative extraversion and introversion is greatly dependent upon cortical arousal, as mediated by the reticular system. Strange as it may at first seem, introverts are typified by a much more intense state of cortical arousal than are the behaviourally more obviously active and reactive extraverts. Having intrinsically lower levels of internal stimulation, extraverts tend to seek out stimulating situations in the external world, whereas introverts tend to avoid additional stimulation because their internal mechanisms are chronically switched to a 'high gain' position.

As has already been mentioned, 'neuroticism' measures – which really refer to the degree or persistence with which one responds to emotion-provoking situations – are thought to depend more particularly upon the autonomic nervous system. The nerves, muscles and glands which comprise this system are predominantly of the automatic variety, regulating such things as heart-rate, blood-pressure, activity of the gastro-intestinal tract, sweating and so on. Together, they contribute to many of the direct expressions of intense states of emotionality – for example, palpitations or dizziness due to elevated blood-pressure, that 'sinking feeling' in the pit of the stomach, or the flush of anger and pallor of fear. The physiological workings of the system are better understood than are the cause-effect links between emotion and autonomic activity, but it is certain that measures of neuroticism are somehow intimately linked with the workings of the autonomic nervous system.

Eysenck's thesis, in respect of both extraversion and neuroticism, is that they are not only subject to the same underlying structures and functioning as any other physical attribute of man, but that this variation is of substantial genetic origin. With considerable conviction he has argued that the weight of evidence (not excepting some of Claridge's work) indicates that the psychological processes identified by his own (and others') factors of extraversion and neuroticism are physically rooted in the nervous systems described. He is therefore talking quite specifically about psychological variation arising from genetic differences acting upon these particular systems.

So, there remains the question of how convincingly neuroticism

and extraversion can be shown to connect with heredity. The answer in both cases being that they can to a very substantial degree. Twin studies have time and again produced positive heritability indices – both with adults and with children – but, because they are so numerous, examples must needs serve.

The neuroticism dimension is generally investigated in connection with clinically disturbed individuals, so more will be said of this in chapter eight. However, a good example of a sophisticated approach to the subject is a classic investigation conducted by Eysenck in collaboration with D. B. Prell.* Their study was particularly ingenious in the way it integrated findings from a group of 21 diagnosed neurotic children with 100 pairs of normal twins – 50 pairs being MZ ('identical'), and 50 pairs of DZ (fraternal). All the children were given 17 objective tests of neuroticism and the results were examined to establish that the test measures used were capable of differentiating between clinical and normal groups, and that they were satisfactory discriminators of the neuroticism factor. Having established this, test scores for the MZ twins were compared with those for the DZ pairs. Intra-class correlations between MZ twins were made – yielding a coefficient of the order of 0.85, whilst that for DZ twins was 0.22: the derived heritability coefficient producing an estimate of 81 per cent hereditary contribution for these measures of neuroticism.

Actually, this over-all estimate proved to be higher than could have been derived from any one of the tests taken by itself, and this implied degree of inter-relatedness was held by Eysenck to be powerful evidence of an underlying biological reality. Be that as it may, there has been no shortage of other studies clearly indicating genetic involvement in neuroticism – though the heritability estimates naturally enough vary according to the tests used and are, typically, less marked than in this classic early study.

Extraversion, too, yields an embarrassment of experimental riches so, once again, we shall have to select an example and leave it to those who are interested enough to seek out further material. Fortunately this is not at all difficult, as Eysenck's own great flow of books and articles acts as a sort of data bank on the state of heritability studies relating to both neuroticism and extraversion – it is, however, one which the cautious individual will be sure to compare with other, less selective, sources.

By way of an example, then, we might consider the very exhaustive

* *J. ment. Sci.* (1951) **97**, 441–65.

studies conducted by H. McLeod and D. Blewett under Eysenck's own direction and reprinted in his book *Eysenck on Extraversion*. As subjects they used four groups of school-age twins, ranging from twelve to fifteen years of age. Half the children were MZ twins, the other half being DZ. They were divided up into equal sized groups of thirteen pairs; one group consisting of MZ males, one of DZ males, one of MZ females, and the remaining one of DZ females. In making inter-group comparisons, it was therefore readily possible to allow for the direct influences, and the various interactions, of sex and extraversion. What is most impressive about this study is the great range of tests involved, including not only more or less straightforward measures of extraversion, but also a host of other tests of emotionality and cognitive behaviours to allow for a more detailed appraisal of interactions.

The data derived from this battery were subsequently factor analyzed and, from the fifty-two variables included, there emerged the three main factors of intelligence, extraversion and neuroticism. Intercorrelations of the factor scores showed no significant relationships between these three for either MZ or DZ twins. In other words, each factor may vary independently of the others: a finding which has often been repeated subsequently, and with subjects drawn from very varied backgrounds and age-groups.

The most important finding, at least from our present point of view, derived from the comparisons between the scores of MZ and DZ twins on each of the three factors. In every case there were significant differences between the scores of the MZ and the DZ groups, this being most markedly so in the case of extraversion. In conclusion, then, this study not only adds further empirical support to many others bearing on genetic influences in extraversion, it does the same for the other two characteristics – neuroticism and intelligence – upon which we have dwelt at length, and shall again.

Twin studies like these produce excellent evidence about the heritability of personality, but most of us are likely to feel even more reassured by data deriving from situations where it is possible virtually to exclude the potentially confounding effects of twins responding to the stereotypes which are projected at them, and/or their being subject to other shared social-psychological experiences or pressures. Twins raised separately is one of the arrangements most favoured by some researchers but, as we have seen, even this paradigm has been subject to the criticism that adoption agencies (or agents) may consciously or unconsciously operate a policy of placing children in homes which are as compatible as possible with the biological parent's

backgrounds: each of the two children thus being placed in a similar environment.

Such dangerous sources of experimental bias are now quite well understood, and the prevailing circumstances would usually be very carefully scrutinized for directionally operating influences before any such study was undertaken. But there are also other powerful methodological possibilities for research, one of which is to compare *unseparated* twin pairs whilst they are still infants, and so (arguably) considerably less subject to the moulding factors of their parents' behaviour, or their own interactions which may lead to their emphasizing similarities or differences in personality and life-styles generally.

A nice example of this latter type of research strategy is a study which was conducted by D. G. Freedman,* in which the focus was upon the behaviour of infants during their first year. Modest in scale, the study involved only twenty pairs of twins – whose zygosity was unknown until the end of the study, when it transpired that nine pairs were of the 'identical' MZ type, and the remaining eleven pairs were 'non-identical' DZ. The children were observed in a number of standardized experimental situations and their behaviour filmed, the ingenious part of the study being that the judges who rated the children's behaviour only ever saw one of any twin pair and, in any case, zygosities had not been determined at this stage. The results indicated that on two of the basic components of later interpersonal style – smiling and fear of strangers – the MZ twins were significantly more alike than were the DZ pairs. Such a finding is naturally very compatible with studies exploring the heritability of extraversion-introversion in older subjects, and it might well be taken as suggestive of the very early emergence of this much more general personality factor.

Long-term follow-up studies are clearly needed to explore possibilities such as these, and to elaborate the theoretical connections between genetics and the early development of personality. However, though the exigencies of selection and presentation may seem to suggest that studies like this represent essentially 'one-off' correlational approaches, in reality a good deal of other work has been, and is being, done – research which I believe will prove to have a very productive interface with the theoretical formulations of Eysenck and his co-workers.

To have ended this chapter on the topic of extraversion is

* *Science.* (1963) **140**, 196–8.

particularly appropriate as it has recurred throughout in so many different guises – as indeed it will continue to do so in the remainder of the book. Not only is it one of the major theoretical constructs proposed by Jung and Eysenck, it has also emerged as a basic dimension from the constitutional studies of Kretschmer and Sheldon, and thus links anatomical and physiological findings with neuro-psychological theories of the Pavlovian type. The various strands of evidence, when taken together, have gone some considerable distance towards elucidating the actual mechanisms which probably underlie this particular psychological dimension and, in so doing, have made it that much easier to think concretely about the genetic processes which themselves must underlie the physical and psychological levels of observation.

In selecting the evidence for the heritability of personality characteristics, I have not had much to say about a whole range of extant studies which have been concerned with the development of more specific attributes. For the most part, these seemed to me to be interesting in the sense of reporting thought-provoking correlations but, because of the omnipresent need to be selective, of less immediate importance than those investigations which have a clear relationship to psychological theory and other active forms of research.

The literature suggested for 'further reading' will direct interested readers towards a more catholic selection of the available work, but I have here chosen to emphasize the two, somewhat related, issues of the connection between gross morphology and temperament, and the heritability of those broad personality factors beloved of Eysenck. It seems to me that, though these particular approaches may ultimately recede from the foreground of psychogenetics as our knowledge becomes more extensive, if *they* do not withstand the tests of critical attention and further research, the prospect for less broadly based studies looks decidedly poorer. Moreover, the topics which have been chosen for elaboration are those most relevant to current investigations of pathological functioning and so this preamble may prove to be a useful scene-setter for the treatment of mental disorders in chapters seven and eight. But, before we get to that, there is the even more intriguing question to be posed of whether the genetically mediated physical differences between males and females have any similarly produced psychological counterparts.

Chapter six

MALE - FEMALE DIFFERENCES

Because of the apparently clear dichotomizations imposed by sex, it might appear that genetic studies of male-female psychological differences would be more straightforward than in many other areas. After all, the last chapter closed with the observation that when tangible physical characteristics are regularly associated with typical psychological attributes, genetic causes seem more plausible than when they depend solely upon mathematical links. This being so, sex-related differences might seem to augur clearer cases, but, in addition to the all-pervasive effects of gender-role moulding, a new and serious methodological limitation actually makes such studies unusually difficult.

The complication is that the principal strategy available for human research - comparing concordance rates for MZ (single-egg) and DZ (two-egg) twins - cannot be used for the very good reason that MZ twin pairs are always of the *same sex*.

This is a considerable blow, as one would ideally wish to make comparisons between individuals who shared an identical genetic endowment, yet differed in respect of their sex. As we shall see later, this is not a complete contradiction in terms: there are very exceptional circumstances which, with certain limitations, make it a possibility. However, in most cases the best that one can hope to achieve from twin studies is a comparison of the developmental histories of opposite sexed DZ twins who, though perhaps sharing more similar environmental circumstances than most non-twins, are genetically no more similar than ordinary brothers and sisters.

Certainly there may be some advantages in being able to specify apparent similarities in the environment of the twin boys and girls whose development is to be compared, but this nevertheless leaves open the real possibility of there occurring psychologically important differences in the communicated values, ideals, expectancies and

general subject matter emanating from parents, siblings, the wider family and people generally.

For example, it may be that girls, being both smaller and the subject of popular beliefs about their delicacy and tastes are, from the start, treated more gently than boys. If so, differences which may or may not be reality-based are, from infancy onwards, likely to become reinforced through social interaction and strongly consolidated into the self-concept. At the onset of puberty the process is accelerated still more as children become increasingly subject to intensive pressures aimed at making them adopt the stereotypical gender differentiations which are customary within their culture. We must accept that a good deal of psychological differentiation based upon treatment can, and does, take place very early in a child's development, so that what looks completely *natural* may only be *normal*. There is, of course, a vast literature concerning the presumed effects of socialization on the development of sex differences, but it would be impossible to do it justice in the present context. Instead, a modest selection has been made from research findings which emphasize experimental rather than uncontrolled observation of males and females in society.

Not that social and cross-cultural observations are in any way less important sources of data, it is simply that extended treatments of this sort of sex-related material is currently very easily available elsewhere. But one does have to be even more cautious than usual when weighing this type of evidence, as the 'facts' reported are so readily interpenetrated with opinion and doctrinaire beliefs. Moreover, other people's research, however objective it may or may not have been, is putty in the hands of the ideologically-minded when experimental control and quantification is at its minimum. Dogmatic beliefs can turn any type of research (including experimental) into a vehicle for prejudice, but whereas repeatable scientific studies may be checked by others of a contrary point of view, material which depends upon unique events and idiosyncratic interpretation allows far more scope for the workings of subjectivity.

One of the most interesting, and classic, examples of observational data bearing on the male-female issue is the anthropological work of Margaret Mead in New Guinea. Looking at three tribes, she reported what amounted to limited sex-role reversals in one of these groups, and pointed to the effects of different cultural traditions in each of these tribes to account for male predominance in the one, female dominance in another, and more equivalence of role in the third. This report has, for decades now, been used as 'proof' that male-female differences are of an entirely environmental origin. What is so fascinating about this

example is that it has found its way into innumerable basic textbooks in psychology and sociology, and has been generalized to such a degree that, for many, the possibility of important psychogenetically caused differences between the sexes has become inconceivable. For such people, the total dependence of human personality upon socializing forces has become a basic 'principle' of the behavioural sciences. The remarkable thing is that a small-scale study, employing highly subjective observational techniques where the tribes proved to be ephemeral and the situation was essentially non-repeatable, should be given such ready acceptance in the face of overwhelming historical and cross-cultural evidence pointing the other way. That the opposite case should receive such acceptance is, of course, very hard to imagine in the present climate of opinion.

However, turning to the prior question of whether there *are* any important psychological differences between men and women or boys and girls, we might do worse than begin by spending a few minutes, very subjectively, considering the sort of differences which seem to emerge when we think about males and females known to us personally. My guess is that few of us, whether we ourselves are male or female, will find it difficult to compile quite extensive lists which, though not always appropriate in every respect to each individual under consideration, do seem to suggest quite marked differences in the interests, feelings and activities of males and females.

The nature of the differences which we may have noted is not, of course, likely to be uniformly applicable over all age groups – particularly if our spectrum was extended to include infants, school-children, young adults, and people in the middle and later years.

In childhood and youth, we are all familiar enough with the little girl's interest in dolls and her intense, if playful, sense of family responsibility, caring and loving. Equally typical is the small boy's dedication to fishing, football, and what (or who) he can wing with his air-gun. Aggression, fighting and competition are (alas) the hallmarks of young male culture, and they find their fantasy expression in the widespread masculine preoccupation with dominance and violence – whether heroic stories and legends or the more contemporary cowboy, crime or war film. Later in life, these initial interests may find concrete expression in wars and violent antipathies or, statistically more likely, in transformations into the many forms of status competition. Similarly, doll's-house play is soon re-directed to real home affairs and perhaps, more sublimely, into an interest in the human family generally – at least, the greater participation of women in social, educational, and medical activities is strongly in accord with this tendency.

Of course, generalizations like these should not blind us to the equally obvious fact that there is a great deal of overlap between the sexes, with some males evidencing typically 'feminine' interests whilst some females exhibit typically 'masculine' patterns. And, just as individuals vary in the degree to which they approximate to the norms of their sex, so too can quite substantial sub-groups – like 'gay liberation movements' – which may develop reference norms more congruent with those of the opposite sex. Plainly, stereotype ideas have no more place in regard to sexual group membership than they have in any other branch of psychological investigation. Exceptions to apparent rules are abundant, yet the typical characteristics which have suggested these 'rules' are undeniably there to be seen.

There is no psychological substitute for the direct observation of people going about their lives in the ordinary way. If our interest is in sex differences, we shall have no difficulty in observing a host of behaviours which suggest widely divergent psychological characteristics, at least in terms of mental content and behavioural tendency. Again, we are thinking about the characteristics of groups, not particular individuals. On a statistical basis, there is no doubt that adult females are more likely to spend proportionately more of their time thinking about loving or nurturant themes than are males, or that a male is more likely to perpetrate a violent or delinquent act than is a female. In both of these types of activity, we are observing the workings of psychological differences; this is a matter of fact, though their origins have yet to be traced.

Valuable as direct observation may be, it really only gives us a very general impression and even that is inevitably based upon a limited perspective and experience. Ideally, we should seek to supplement such observations with more precise measures of the processes and determining variables which seem to be involved. One way is to consult surveys of the sort conducted by governments, health authorities, employment services, educational services, and so on.

Looking at the achievements of women, we see that they differ from those of men. At school and in higher education the two sexes follow markedly different paths and so, in later employment, they reflect very dissimilar patterns. Boys and young men much more frequently enter programmes of advanced education and progress, via higher qualifications, through the upper levels of science, technology, medicine, and engineering. When girls enter higher education, it is relatively unusual for them to enter these fields, instead they are more likely to be found in teaching, nursing, or social work of one kind or another.

Many explanations of these facts have been advanced, most of them

stressing the anticipated demands of family life and the role pressures on the young of both sexes to conform to social expectations. It is argued that girls tend to 'fade out' in later stages of school because they have less support from home, or else that they perceive educational competition and striving to be a hopeless proposition anyway as their destinies will lie in motherhood and raising families. Boys, on the other hand, are thought to be responding to the dual advantage of parental and social support for their efforts, coupled with the perceived reality of having their future much more firmly in their own hands.

No doubt there is often a great deal of truth in such interpretations but the observed differentiations may, additionally, be due to influences deriving from both the specific demands of the tasks involved and the countervailing attractions of the alternative. It is not, after all, self-evident that the attractions of becoming a full-time wife and mother are necessarily any less powerful than those associated with becoming a scientist, engineer, or member of any other traditionally masculine occupation. However, this latter tack rapidly leads one into fruitless arguments about the possible effects of unmeasurable social expectations, whereas the likelihood that choice of academic subjects and occupations may be connected with differential capacities is one that is at least somewhat more readily amenable to empirical investigation.

Intelligence tests may, at first sight, seem to be unhelpful in this respect as the average IQ score is similar for both males and females. However, a simple mean of 100 conceals a number of real sex differences, some of which have led test-makers into manipulating the content of their tests in order to minimize the actual differences which appeared during their development. Of course, one is not suggesting that intelligence tests have been subject to any sort of unethical activity, but they have tended to be shaped to fit a particular preconception and to do a particular job.

Taking as an example the work of one of the most distinguished of all intelligence test constructors, David Wechsler, we may trace how such preconceptions may work to obscure measurable sex differences. The task which Wechsler had set himself in constructing his intelligence scales was to develop an accurate measure of 'general intelligence'. He was therefore concerned to gauge intellectual abilities in a way which would not selectively favour or disadvantage any sub-group for reasons other than actual differences in their *overall* capacities. So when, in the course of standardizing his test items, differences in the pattern of male and female competencies began to appear, Wechsler pruned items in order to balance their means and

distributions. The result has been the emergence of tests of excellent general utility, but at the cost of some confusion about whether these measures support or contradict the proposition that males and females are similarly intelligent.

Most people seem to assume that, because representative groups of males and females obtain the same average score on these intelligence tests, their intellectual capacities must be the *same*. And so they are in a general sense, but, as Wechsler himself came to recognize after many attempts to minimize the differences, averages tend to mask considerable dissimilarities in internal composition. The unevenness of male and female scores becomes apparent when we contrast performance on two of the key measures of general intelligence – that is, verbal versus visuo-spatial abilities. There have now been very many studies, based on a number of tests and drawn from several cultures, which quite clearly point to the superiority of girls in the former, and of boys in the latter.

Other studies of verbal development also quite regularly show girls to be superior to boys on all aspects of linguistic development, whether it be vocabulary, grammatical usage, reading, writing, spelling, or whatever. There is some empirical doubt as to whether girls will later excel boys in verbal reasoning, but all the evidence points to their intellectual development being very heavily biased towards the use of language. It has been suggested that, because girls mature more rapidly than boys, and are talked to more often by their mothers, these observations of youthful development might only be an artefact.

However, it transpires from other studies that the female infant is more verbally expressive from the very earliest stage of baby 'babbling'. Mothers may well spend more time communicating with girl children, but observation shows it to be a very interactive situation. Moreover, measures of verbal skills at an early stage of development prove to be much more closely related to later measures of general intelligence than is the case for boys – a finding which would be expected only if boys and girls emphasize different processes in their intellectual development.

And, just as there is an early divergence in verbal development, so too is there in relation to visuo-spatial problems – such as those posed in geometry, map and plan reading, playing chess, or perceiving forms which are camouflaged or otherwise obscured by distracting visual cues. Naturally, there has been a good deal of research to determine the reliability of the sex differences reported for tasks such as these, and an equally vigorous search for explanations.

One of the more concise recent summaries of this type of

comparative research was made by Max Coltheart.* In it, he reports how, with tasks involving visualizing the way simple figures would appear if rotated or folded to form a solid object, somewhat less than a quarter of the females tested were able to surpass a score exceeded by half the males. Findings like these, though they have been reported from many parts of the world, do not suggest any obvious explanations.

From the point of view of significance to real life, Coltheart observes that such tests not only correlate well with practical abilities requiring spatial abstraction of the kind frequently encountered in mechanical design and repair, they also sample capacities which are of great importance for the practice of many technological subjects other than engineering. In consequence, if there really are fundamental differences between males and females in the relative emphases of their intellectual development, we may also realistically expect to see this manifested in choice of career and later achievements.

This is not to say that given occupational areas can therefore be reserved as the more or less exclusive domain of either men or women: the results mentioned refer only to sex-typical characteristics, not to individual cases. Many individual females will exceed the performances of average males in visuo-spatial areas, just as many individual males will surpass the female averages in verbal skills. Nevertheless, the overall distributions suggest that we should not expect to see all occupations manned on the basis of equal numbers of each sex. If ability is to be the criterion, rather than some form of enforced quota system, then we may expect to continue to see different proportions of males and females at all levels in architecture, engineering, and many of the other branches of science and technology which rely heavily upon visuo-spatial capacities.

One line of thought is that, allowing the observed differences, they may arise from the different experiences of male and female children, who are systematically attuned towards their culturally expected sex role. For example, also included in the grouping of visuo-spatial abilities is mathematics (not computation but numerical reasoning) and it has been argued that girls do less well in this area because their motivations are weaker due to their seeing themselves as less likely to go into jobs requiring mathematical skills. Unfortunately, this sort of argument is very difficult to put to the test in order to determine what is cause and what effect. However, as we shall presently see, there are plenty of other situations in which the determining effects of

* *New Behaviour.* (1975) **1**, 54–7.

motivational differences are less easy to postulate in a convincing manner.

So far, we have been concerned with the phenomenological aspects of such male-female differences: now we must examine the evidence to see whether they reflect anything more than social influences upon development. The verbal differences have so far proved difficult to handle in more than a speculative way but the visuo-spatial differences are strongly suggestive of genetic influence. In fact, the hypothesis lends itself rather well to the possibility of validation through checking the predictions that can be made. This was very neatly done by R. E. Stafford* who compared the scores of parents and children on visuo-spatial tests and looked at them as possible evidence of the working of a recessive gene. And, if we suppose that this gene is carried on the female X chromosome, a number of deductions would follow from our knowledge of genetic principles.

Because we know that males have only one X chromosome whereas females have two, a recessive gene carried on this chromosome would have a marked, and predictable, differential effect upon the phenotype.

A recessive characteristic carried on the X chromosome is always expressed in the male – as with haemophilia and some deficiencies of colour vision – though only manifest in the female if she carries the recessive allele on both of her X chromosomes. In consequence, the phenotypic appearance of such recessive characteristics is always far greater in males than in females. But the most marked effect of this type of inheritance is the absence of father-to-son transmission: a consequence of the effect that sperm cells which carry an X chromosome always result in the birth of a daughter, never a son.

On the hypothesis that the mode of transmission of characteristics affecting visuo-spatial performance are indeed the result of a recessive gene borne by the X chromosome, we should predict that scores on appropriate tests would show good correlations between mothers and sons, and between fathers and their daughters. Positive, though weaker, correlations should exist between mothers and their daughters, and there should be *no correlation* between the performance of fathers and sons as the father does not transmit an X chromosome to his son. In the event, Stafford's empirical results gave every reason for confidence in accepting this genetic proposition as accounting for at least a substantial part of the differences observed to exist between males and females.

* *Percep. Mot. Skills.* (1961) **13**, 428–55.

However, commenting on this line of psychogenetic reasoning, Coltheart makes the point that visuo-spatial ability differs from other recessive X-linked outcomes like haemophilia or colour blindness in that the genetic component is not all-prevailing in determining the phenotype. For, although environmental circumstances will not affect one's colour vision or capacity to clot blood, visuo-spatial characteristics have been shown to improve as a result of experience gained through training in engineering and geometry.

It follows then that the selectively given training that is normally directed towards boys is likely to produce results which are far more extreme in their outcomes than they would be for genetic reasons alone. Put the other way, it is likely that the genetic sex differences in visuo-spatial capacities combine with the effects of differential motivation towards, and experience of, complex mechanical systems to produce the marked composite effect apparent in women's typically very low scores on tests of mechanical comprehension.

Studies have sometimes shown that no more than one woman in twenty can achieve a score above the male average. However, though environment may substantially modify such male-female differences, it has not created them, nor would equivalent opportunities given to both sexes remove them. Whether we ourselves wish to modulate these effects by changing environmental circumstances is a question of value and opinion, but the genetic component involved in visuo-spatial differentiations would seem to be a matter of fact.

Of course, such a conclusion is generally regarded as being extremely contentious by those who see no rational reason to account for the emergence of such biologically based differences. Yet the sexes are undeniably very differently constituted and adapted for different biological ends. The two sexes of our species, like the majority of mammals, exhibit considerable divergence in physical characteristics other than those directly concerned with procreation and the nurture of infants. The male is not only larger in gross size but also has proportionately greater development of muscle and bone, plus a metabolism and vascular system which is geared to a more active mode of life.

Surprising though it may be, there are some who still claim that most of the physical differences are almost entirely a reflection of differing life-styles: presumably they have never had much to do with athletics. Even in conditions where youngsters train strenuously from childhood onwards, the ratio of muscular power differences between males and females remains much greater than the gross mass ratios – a fact which is expressed in both superior strength and physical

endurance. Even the briefest acquaintance with the serious world of sport and athletics should highlight these differences, which are sometimes lost sight of by those of us who live fairly sedentary existences. But, if still in doubt about the magnitude of these differences, it might be helpful to consult the record books on everything from track events to swimming, or from weight-lifting to skiing. The result will be the same.

Biologically orientated psychologists, and ethologists of the Desmond Morris variety, are quite clear on this issue: the palpable facts of physical dimorphism are hardly likely to be unrelated to the evolutionary development of our species. Nature selects, emphasizes, and conserves that which has survival value. Not even the deepest of the 'depth' or 'humanistic' psychologists could surely suppose that mankind is any exception to this rule. The only real questions concern what has been (and still may be again) the adjustive behavioural correlates of our physical differences, and whether these have any *psychological* relevance.

Desmond Morris's reconstruction of our evolutionary and early prehistoric course, though apparently provocative, was actually the conventional picture which had generated no heat until it was thought to reflect adversely upon women's status. Much of the argument since then has been tedious sexist polemic with little adherence to the scientific evidence, and even many scientists have not been entirely innocent of gilding the lily from time to time. However, the conception of our evolution through more advanced primate forms being accompanied by a sex role differentiation into male hunter-defenders and the more domestic and settled mothers of our species is surely in no way improbable. After all, this pattern has been typical of our entire recorded history, and is still the normal state of affairs in less technologically developed peoples.

Relative ease and abundance, coupled with a mode of production and distribution far removed from the small-scale hunting and gathering economies of our forebears, makes the expression of some impulses and behaviours inappropriate. Yet something may still remain to be transformed into hobbies like fishing, shooting, archery, etc; or into sports like wrestling, boxing, kung-fu, or whatever, and even into more dangerous tribal activities such as nationalism and war.

Other primates, lacking our own levels of intelligence, continue to live their more simple tribal lives – with their males emphasizing the roles of attack and defence, whilst the females get on with their more attractive and gentle pursuits. Both functions are of equal importance for, admirable though the female role is, the less attractive

characteristics of fighting for the right to reproduce, driving away other groups competing for the same resources, and defending the band against outside attack, are also about the same issues as those concerning the females, i.e. species survival. All evolution is geared to this one end, our own species not excluded. But we understand enough about the processes of evolution to know that change takes time, lots of time, and it is not likely that any appreciable genetic change could have taken place in our species during the miniscular amount that has elapsed since we began on the path of cultural evolution.

From such perceptual differences as men's enhanced visuo-spatial capacity to spot relationships and see form though camouflaged, through to the physical characteristics which allow for a more vigorous and competitive existence, there is ample evidence for a primitive genetically-transmitted masculine behavioural differentiation which is compatible with a hunter type of adaptation. Women, too, have developed along their own lines of specialization, in which language and the social requirements of community life have been especially prominent. The archetypal roles, much as they have been described by Morris, still persist even today in a few remote parts of the globe where small tribes continue to maintain their tenuous balance with nature. In such precarious circumstances, notions of exploitation and male chauvinism are (for me at least) unconvincing as causes of role differentiations: the selective effects on survival are too stringent. No doubt cultural influences are operative too, but all the evidence points to the evolution of behavioural specializations which are congruent with the differing physical specializations of the sexes.

In fact, anyone who has a broad interest in animal behaviour can scarcely fail to make this sort of comparison with other species. And, of course, we become aware of what was always obvious – that male animals are not only bigger, they are generally temperamentally much more formidable too. If we compare the bull with the cow, the dog with the bitch, the billy with the nanny goat, the barnyard cock with the hen, or whatever, we will readily see the emergence of characteristics which are somewhat parallel to our own differences in sexual behaviour and aggression: all without a hint of cultural determination.

However, evidence such as this is often thought to be somewhat circumstantial and, in any case, relevant to animals well removed from ourselves in evolutionary terms. Ideally, one would really like to know how boys and girls would compare with one another if they could be brought up in a neutral culture by sexless attendants. Hopefully, this experiment should prove beyond the resources and ingenuity of even

the most ruthless seeker after truth and so we must settle for controlled experimental studies on other mammals, and particularly on other primates.

In Wisconsin, the psychologist Harry Harlow has long been concerned with the social differentiations of monkeys and, in particular, with the degree to which these arise in the absence of models from which infants learn. As Harlow observed, if one simply watches young rhesus monkeys interacting in a colony, it soon becomes apparent that there are striking differences between the sexes in the aggressiveness of their behaviour. Males play rough fighting games, issue more serious challenges, and more commonly adopt postures of threat and hostility. Females are very markedly more passive in their activities. Of course, it could be argued that this may be due in large measure to the infants imitating older monkeys, and particularly copying those females who are themselves inhibited by their own adaptations to infant care. In other words, the casual explanation may be construed in a way almost identical to the environmentalist account of human sexual differentiation.

But Harlow's experiment in which infant rhesus monkeys were reared apart from older animals proved to be a very revealing procedure. Instead of their mothers, the young monkeys were provided with a wire and cloth 'surrogate mother' to cling to, and the social behaviour of these youngsters was then monitored when they were later allowed to mix with older animals. The results showed that, from as early as three weeks old, the aggressive and threatening behaviour of male infants steadily continued to exceed that of the females. As Harlow himself commented, it is hard to believe that the behavioural differences were communicated by the inanimate surrogate mothers.

The conclusion that appears to follow, is that males and females are from the very earliest stages of their lives subject to different behavioural determinants which, if not environmental, are most likely to be hormonal. Furthermore, as the phenomena are sex-linked, it makes sense to concentrate on the relative influences of the sex hormones – the male androgens and the female oestrogens. Fortunately, we now know a good deal about the ways in which endocrine activities affect the sexual and aggressive behaviour of many animal species, including our own. Human beings, though, are not subject to the same experimental procedures and so we must glean most of the principles of neuro-endocrinology from explorations undertaken on other mammals. However, the natural dysfunctions which afflict some people, together with the effects of more or less successful attempts at

hormone therapies, have indicated our essential similarity to other mammals and, most markedly, to other primates.

Before considering the evidence from human dysfunctions, perhaps we might just examine some of the animal evidence bearing on the crucial question of whether or not the brain is psychosexually neutral at birth. Harlow's study suggests that, at least in the case of rhesus monkeys, the brain is *not neutral*: were it to be so, both males and females should have developed similarly as they were brought up in equally neutral circumstances. But, it may be objected, behavioural differences may result simply from differential hormone activities which, though these are genetically determined, do not necessarily imply the further step that there is a more fundamental difference in the 'wiring' of the central nervous systems. Fortunately, this is a question which has proved capable of being resolved through experimental procedures. All that is required is that the hormonal balances of both sexes should be manipulable before and after birth.

Animals at many phylogenetic levels have served as subjects for these investigations and pregnant females of such diverse species as the cat, rat, sheep, and monkey have all at some time been injected with sex hormones to see how the young are affected. Male animals must, in any case, always develop in the circulating female hormones of their mothers, and so must differentiate individually by producing their own male hormones. In consequence, the focus of experimental attention centres upon the effects of exposing female young to the uterine experience of hormonal conditions which they would not normally encounter. So, if a pregnant animal is injected with the male androgenic hormone testosterone, one can later follow up the effects on her young.

As might be expected, the young males are not much affected by this procedure but the females are, being more or less physically masculinized. And, though they remain genetically female, their degree of hermaphroditism may considerably affect the appearance of the external genitalia such, for example, that a clitoris may enlarge to resemble a penis, although these pseudo-male structures cannot, produce the testicular androgenic hormones. However, as such masculinized animals develop, they will also begin to exhibit behaviour characteristics associated with the males of their species and may, for example, fight with the males and attempt intercourse with other females. The pseudo-penis may even be employed to some purpose though to no reproductive effect.

Several experiments of this type have even produced genetically female hermaphrodites with quite exceptional propensities for

aggression, and for attempting matings with others of their own genetic sex. The critical period during which hormonal interventions will produce long-term effects may vary with the species, but the pattern remains pretty much the same.

This sort of evidence suggests that, as the testosterone to which the foetuses have been exposed rapidly metabolizes and disappears from the system, it must induce some other changes of either a hormonal or structural type. And, as the genetic female is not capable of herself producing the gonadal hormone testosterone, it seems most probable that the experimentally induced hormone simply performed one of the functions which it normally serves in the genetic male – that of 'switching on' certain genetic patterns which are coded for in both sexes, and which modify not only physical development but also certain behavioural dispositions controlled by the brain itself.

In her excellent book *Males and Females* Corinne Hutt presents the evidence to justify her use of the dichotomous concept of 'male brains' and 'female brains'. She cites anatomical and histological research which indicates differences in the development of some cortical areas but, perhaps more importantly, she describes how research into the neurological structure and hormonal sensitivity of the hypothalamus, a key system in the brain concerned with the control of drive and emotions, has revealed important male-female differences. It has often been remarked that, for mankind anyway, the major sex organ is the brain, not the genitals: research now suggests that this view may express a literal as well as a metaphorical truth.

The hypothesized permanent sex differentiation of the nervous system, which is supposed to take place at a very early developmental stage, can be checked readily enough by conducting hormonal interventions in animals – a procedure which has frequently been adopted with creatures at all stages from conception to maturity. On the hypothesis of early brain differentiation at a 'sensitive' period, it follows that if the behavioural tendencies so often described as being biologically sex-specific are indeed so, hormonal intervention during the sensitive embryonic and foetal stages should have marked and permanent effects, whereas later artificially-induced changes in circulating hormone level should produce quite different results.

Of course, androgens or oestrogens can be injected into both males and females, and even into already masculinized hermaphrodites, and it is also possible to observe the effects of deficiencies due to removal of the sources of gonadal hormones – the testes and ovaries. Additionally, one can inject hormones of either type into animals which have been neutered at any previous stage in their development. With so many

options, it is not surprising that the quantity of research generated far surpasses our own present needs in making general points, but those particularly interested in this aspect will find useful starting points for their own investigations in the books by Hutt and by Money and Ehrhardt.

Anyone looking further into this matter of hormonal manipulations will see, though, that any existing lack of agreement in results is not only minor in form but principally connected with the type of animal studied and the developmental stage at which the intervention took place. For example, in some of the more primitive mammals like the rat, sexual differentiation of the brain does not take place until a short while after birth. Consequently, interventions at this stage will have marked results, i.e. females treated with androgens in early infancy will later show typical male aggression whereas males castrated at about the stage of weaning will evince reduced aggression, and those castrated at birth will grow up to resemble typical females in their level of aggressiveness. However, if an adult male rat is castrated, this will cause only a small reduction in his aggressive behaviour and, similarly, the injection of male hormones into an adult female will not appreciably change her aggressiveness either.

But in primates, the sensitive period during which hormones will indelibly affect the developing brain by determining sex-typical tendencies occurs during the early stages of pregnancy. Injections of the male hormone testosterone will certainly masculinize the female foetus and cause her behaviour to develop along typically male lines at a later stage, but such injections given to females *after birth* will not produce these results. By the same token, the castration of male monkeys at birth will not markedly alter their development as normal males – except in the reproductive sense, of course. It would seem then, that the primate pattern of psycho-sexual differentiation occurs before birth and that the consequence of normal testicular production of testosterone is to act upon the hypothalamus by modifying its function of regulating not only many of our hormonal activities, but also the associated physiological processes and biologically-based motivations as well.

So far as may be judged at present, it appears that femaleness is the basic biological pattern of most, if not all, mammalian species – as this is the template from which most modifications arise as a result of additional instructions. Oestrogens are principally (though by no means exclusively) concerned with such processes as the menstrual cycle and lactation, whereas experimental studies involving hormonal manipulations indicate that androgens are responsible for most of the

physical and psychological sex differentiations to which we have alluded. The potency of androgens is astonishing when one considers the magnitude and permanency of changes which may be created in genetic females by exposure to these substances at a critical stage of development.

Valuable though it is from a scientific point of view, it is unfortunately the case that we can find many human examples and parallels for most of the experiments we have been describing. Castration in males is a misfortune which may occur at any time before or after puberty. At one time, in the great churches of Europe castration was an acceptable means of preventing the onset of puberty from robbing choirs of their soprano and alto singers; whilst in the Middle East, castration at any stage was a desirable (not to say enforced) qualification for the guardians of harems. But, apart from unverifiable and untrustworthy tales about either the effeminacy or fierceness of such people, little of scientific value has accrued from these practices. More recently though, there have been a number of follow-up studies of cases involving testicular atrophy or removal. Generalization is difficult, but such evidence as exists gives no support for the contention that the consequent lack of testosterone in a genetic male will result in effeminacy: it will affect the onset of puberty if castration occurs early, and probably the strength and continuance of libidinal urges and sexual activity if occurring after puberty, but both conditions are rectifiable by hormone replacement therapy.

Unfortunately, though, the humane considerations which cause physicians to provide hormone replacements for the physiologically deprived also make it difficult to assess the roles of the various components involved in determining normal sex differentiations. Such chemical interventions will take over some of the processes which would normally be induced by the sex chromosomes but, because it is a matter of delicate judgement to assess how complete this take-over may be, it is also a hotly disputed issue. On the other hand, one of the relatively more clear-cut examples of how physiological and behavioural sex differences can be triggered by a quite minor genetic sequence, has also sometimes been a consequence of hormone therapies. Though not a common occurrence, therapeutic miscalculation can be a useful source of information – as in a recent case where women with a tendency to miscarry in pregnancy were administered a synthetic analogue of the sex hormone progesterone to help in retaining the foetus. Now it happened that the drug involved also contained chemical derivatives of the male hormone testosterone and, depending upon the dose taken and the stage of pregnancy

reached, the affected female infants were more or less masculinized; the more extreme cases resembling boys at birth. Similar results can also occur when no outside intervention has taken place, as in the *adrenogenital syndrome*, where a genetic abnormality in the mother's metabolism causes a gross acceleration of puberty in male infants and the masculinization of females.

At birth, depending upon the only evidence normally available, children are assigned to one sex or the other on the basis of their external genitalia. Inevitably though, mistakes sometimes occur, particularly in the sort of pathological circumstances described, and the child is assigned to a gender other than his or her actual genetic sex. Only many years later, when puberty fails to follow its normal course, do many of these cases come to attention – thus apparently providing us with exceptionally fine opportunities for observing the relative influence of genetics and the learning of a sex role.

In reality though, such observations are extremely treacherous as the conditions are not as simple as they are sometimes construed. Children whose genitals develop anomalously have usually been subjected to intra-uterine hormonal irregularities which means, for example, that a child who is brought up as a male but later turns out to be genetically female was nevertheless subject to biochemical processes similar to those controlled by the genes in male infants. Clearly, the causes of failure to achieve normal sexual differentiation are likely to be quite varied, though it seems reasonable to conclude that the greater the degree of physical masculinization, the more likely are pronounced psychological sequelae. In fact, later follow-up studies of markedly androgenized girls have shown them to be quite conspicuously boyish in their activities and interests, preferring boys' toys to those of girls.

In their book, *Man and Woman, Boy and Girl* John Money and Anke Ehrhardt have presented an excellent selection of what is an absolutely fascinating literature on the psychological effects of genetic or developmental abnormalities in sexual differentiation. In it, they also record the outcome of many cases where surgical and hormonal treatments have been used either to change or to reinforce people's gender roles. Summarizing results in this field is, however, complicated because of the frequent lack of comparability between cases. For example, there may be considerable differences in the age at which clinical interventions were made; differences relating to the prior knowledge by either the patient or the parent that the anomaly existed; the degree to which the patient has resembled, or can be made to resemble, normal members of a given sex; and so on. In consequence

of this lack of clarity, there is still a considerable amount of room for discussion about the degree to which sex-typical behaviour is due to genetic considerations and the degree to which it is learned.

Opinions about the meaning of the accumulated data vary, though my own reading of it suggests that the observable behavioural differentiation between the sexes is very much a function of the neurological differentiation which takes place either under the influence of the male sex chromosome in normal cases, or else under conditions where genetic action is simulated by the abnormal presence of its main chemical executive – testosterone.

There also sometimes occur gross chromosomal abnormalities which bear on the question on how far genetics govern gender role. These may take the form of a child being born with only one of the usual pair of sex chromosomes, or else one or more additional chromosomes. *Turner's Syndrome* is the only example of the former case, and is due to the possession of a single sex chromosome – a female one; male chromosomes, by themselves, are not capable of producing a viable organism. Such children are unmistakably feminine, despite the likely appearance of later deficiencies in secondary sexual characteristics and reproductive capacity. Hormone therapy in the form of oestrogen will ensure the onset of puberty through the development of such secondary characteristics as breasts and body hair, though it will not correct the sterility. However, behavioural studies indicate that girls suffering from this syndrome are likely to adopt the feminine gender role quite successfully.

Additional chromosomes may be of either sex but, whatever the combination, the possession of a single male chromosome is sufficient to produce a phenotypical male. For example, *Klinefelter's Syndrome* may occur in males as a result of the presence of one or many additional female chromosomes, but despite certain feminizing effects on appearance, there is no evidence that the psychological effect is one of effeminacy.

Yet, if the additional chromosome should be an extra Y, there results the so-called XYY 'super-male'. The title itself is something of a misnomer as such individuals are generally far from being super in most important senses: in fact, the reverse is true as they seem prone to both intellectual and social problems. We shall be returning to this example in chapter nine so, for the present, we can curtail our discussion of this very interesting syndrome, limiting our comments to those most apposite to the question of whether genetic material has any markedly determining effect upon masculine behavioural tendencies.

The tag 'super-male' actually arose from the observation that an

unexpected number of XYY cases were detained in penal establishments and mental hospitals and that they were typified by not only excessive physical growth, but also by a marked aggressiveness and generally untamed nature. As we shall later see, these observations have been the subject of considerable scrutiny; scientists are still weighing the evidence and seeking to establish whether the genetic-behavioural correlations will continue to hold if the observations are drawn from more widely-based population studies. We shall have to wait some time for a final opinion, but the results so far have undeniably had a considerable effect upon thinking about the genetic mediation of masculine aggression.

The technical and financial problems of this type of genetic survey are formidable, as I can attest from first hand experience of my own interest in females with an extra sex chromosome (the XXX Syndrome). Women with this abnormality are more likely to be identified in mental hospitals of one sort or another, though, unlike the XYY Syndrome, there does not seem to be any typical history of aggressive behaviour. An interesting hypothesis is that such women, who are usually physically well-formed, might be regarded as the female equivalent of the XYY 'super male' (*sic*), though likely to act out their abnormalities in terms of more feminine traits. Female delinquency often takes the form of sexual misdemeanours or an atypical promiscuity which signals a degree of rejection of social and interpersonal values equivalent to the male's aggressive activities. Thus the behavioural differences may well prove to be functionally equivalent, though channelled into actions more congruent with the sex of the individual and the hormonal functions controlled by the additional chromosomal material. Once again, though, it will take some time (as well as tact and ingenuity) to test this hypothesis.

Turning for a moment to the question of how far fluctuations in the circulating sex hormones influence day-to-day behaviour, it again seems that men and women are differentially affected. Women will be all too familiar with the monthly cycle induced by their sex hormones, whether or not they recognize the source. Apart from the obvious physiological effects, though, there are also psychological effects operating at many different levels – from reductions of mental energy to lassitude, pre-menstrual tension, and the inhibited behaviour, embarrassment and sense of inferiority which so often accompany the physical effects of menstruation. Conditions like pre-menstrual tension are now widely recognized as important sources of psychological variability in women, though, sad to say, most of the other effects tend to be shrugged off, or allowed only a minimal significance,

in the scientific analysis of differential physiological determinants of typical sex behaviours.

Yet, though such manifestations may involve a considerable cultural element in the shaping of attitudes and taboos, that they are also grounded in, and interactive with, certain physical facts is attested by their universality. Particular effects may have a local flavour about them, differing in different times and places, yet the generic similarities remain and are dependent upon the same physical facts. Ultimately then, even outcomes like these may be viewed as part of the total genetic package; moulds of the psychological characteristics which additatively characterize masculinity and femininity. In more advanced societies, a whole female-orientated technology in the many forms of pharmacy, medicine and cosmetics has joined with education to reduce greatly the psychological consequences of the monthly cycle and the reproductive function. Even so, this is still an area which demands much more analytical attention that it has ever achieved in the past.

Another activity which is affected by the menstrual cycle is the sexual appetite for, although the human female is always potentially receptive, research has shown that copulation actually follows a pattern of troughs and peaks and it can be no coincidence that the implied eroticism is correlated with changes in the woman's hormonal programming. Of course, sexual activity is a highly idiosyncratic matter, and is related to a whole host of social and psychological determinants. Nevertheless, one should not overlook the more basic physiological determinants which also partially determine the strength and nature of erotic drives and satisfactions. Men and women are at their most different in respect of physical sexual equipment so we should not be surprised to find similarly genetically mediated differences in their psycho-physiological expressions of sexuality.

One way in which this difference is expressed is that men have no apparent cyclical component to their sexual drives, though a cycle might be suggested by the motivational peaks and troughs which may be due to sensitivity to the accumulation or exhaustion of seminal fluids in the seminal vesicles and prostate gland, or in the sperm-storing tubes of the epididymis. And, in fact, most men have a more or less considerable refractory period after ejaculation which suggests that these hours or even days would allow for the refilling of depleted cavities and the attendant re-setting of the erotic 'trigger'.

However in some men, or under particularly stimulating conditions, the time between orgasms may be very short and may be due to testicular reserves acting in combination with the conditioned

physiological excitement. But women are not restricted to the limited delights obtainable by men: they do not reach a state of satiation in the same way and may be capable of upwards of fifty orgasms within an hour if stimulation is maintained and physiological exhaustion does not supervene. In other words, it is women and not men who have the most finely tuned mechanisms for sexual pleasure – despite the male's overt emphasis upon his own potential.

Only recently has it been fully realized that the female sexual drive is a force with considerable potency. It has generally been assumed that female sexuality was of a relatively low order compared with the male's, and that her participation in intercourse was most often motivated by a sense of duty or affection, or else as the instrument of having children and retaining a husband. No doubt this was, and still is, frequently so, and yet now that repressive taboos about women's sexuality are beginning to crumble as a result of changing attitudes, knowledge, and the sense of freedom which comes with economic independence and the ability to enjoy sex without the anxiety of unwanted pregnancies, a new pattern is beginning to emerge. Survey studies like those of Alfred Kinsey have obliged us to revise radically our thinking about most masculine and feminine drives and pleasures, whereas laboratory observations and other studies of the sort conducted by William Masters and Virginia Johnson have brought many revelations as to the mechanisms by which males and females achieve physically and psychologically satisfying experiences.

So, we are now beginning to come to terms with the certainty that our old stereotype of female sexuality was wrong, and the possibility that we may also be wrong in our more recent hypothesis that, without the pressures of social conditioning, there would be no appreciable basic difference between the sexual drives of males and females. The Kinsey Reports of the 1940s have been superseded by our later intelligences – suggesting not only a narrowing of the gap in most areas where comparisons were made of male-female sexual activities, but also indications of a far superior sexual sensitivity and drive in many cases.

The psychiatrist Mary Sherfey argues that the maximizing of fecundity which she thinks is implied by this great sexual potential is congruent with evolutionary principles in that it would even serve the evolutionary ends of conferring a reproductive advantage on those females who are most active and receptive. Implied in this conclusion is, of course, more than a general statement about biological principles; it also suggests that the sensitivities and motivational processes of women may differ from those of men in ways which have a

fundamental significance for personality development. The premises and the conclusion may, or may not, be correct: but that there are grounds for examining the psychological consequences of our sex and sexuality is beyond doubt.

Evolutionary arguments are not always easy to sustain in relation to human sexuality as we are, in a number of ways, a unique class in nature. Our own species is not only remarkable by virtue of the fact that the females are always receptive and able to experience orgasm, but also because both sexes have evolved erogenous zones to an extent which has no parallel in the rest of the animal kingdom. Sexual pleasure has evidently been, and still is, a major consideration in our evolution and adaptation.

And, apart from the female's greater potential for genital arousal, she is also more richly endowed with additional erogenous possibilities. Her over-all tactile threshold is lower and she may much more readily experience orgasm as a result of stimulation of parts other than genitals. In fact, as an extreme example, recent studies have shown that the nipples respond to the same neuro-hormonal mechanisms as the genitals, and that they may even produce orgasm in the process of breastfeeding. This is a particularly significant observation as it points to yet another process by which our species is genetically influenced to achieve powerful social bonds, though this time between mother and child rather than the woman and her mate.

In the most general sense, the orgasm may be thought of as a psychological reinforcement for behaviours of the greatest possible biological importance: it rewards, and thus ensures the continuance of, activities which would otherwise lack the sort of immediate positive reinforcement necessary to condition behaviour. What we suppose to be of a purely cultural and psychological origin may often be found to have many more genetically mediated determining and regulating mechanisms than we imagine. It may not seem quite 'nice' that breastfeeding may be sexually stimulating, but one can see that it would help to effect a bond between mother and child which would offset the disadvantages of physical debilitation, loss of freedom and a greatly increased vulnerability for the nursing parent. All physical processes have evolved out of their adaptive value for the survival of the species, not the individual, and the genital orgasmic capacities of the male no doubt do much to encourage the otherwise hazardous and unrewarding process of combat before initial mating and the protracted period of hunting and protecting necessary to support his mate in the raising of their extremely slow-developing young.

Fundamental adaptations such as these are not likely to be deleted

very readily: society may modify its practices in mating and child-rearing but genetic change takes very much longer than the term of our advanced civilization. Men continue to be dominated by the primacy of their genital drive, whilst there is no reason to expect that women will cease to enjoy and seek out their much more diffuse sexuality – of a sort which is so obviously compatible with the intensive body contact involved in child-rearing.

No doubt this latter comment will evoke the wrath of those who wish to proclaim women's relative independence of her physical characteristics, but I am sure that human ingenuity can achieve this in other ways than by denying or overlooking the observable facts and their (alternative) interpretations. The intermediate steps in sexuality may differ for the two sexes, with all that this may imply socially and psychologically, but both males and females are finally subordinate to the great biological programme for reproduction: this imperative, together with its many and varied manifestations, is a sovereign example of genetic coding, however much rational and psychological analysis may surround our activities.

We have wandered into a highly speculative field, nonetheless important or relevant for that. In fact, all the ground that we have covered so far contains a considerable speculative element because of the limited methods of genetic analysis available. However, if constant differences between the sexes are discernible over great periods of time and in all viable societies, and if these differences are compatible with appropriately chosen animal studies in which cultural contaminations may be minimized or absent, then the participation of bio-genetic influences in the behavioural outcomes would seem to be a reasonable hypothesis. Further, if the results of experimental work on psychological capacities are congruent with our broader biological and cultural-historical observations, we may begin to feel that this consistency allows for reasonably confident speculation. Some comparisons are difficult to make according to these criteria, though some of the most fundamental issues – aggression, pair-bonding and child-rearing – can be moderately well treated this way.

As we have seen, males emerge from these various analyses as more vigorous and active, more aggressive, more genitally oriented, and showing greater capability in most object-relationships. Females also emerge as having typical characteristics – a relative sexual passivity combined with a rich erotic potential, a gentler nature, and a more powerful attachment to family and particularly children: their special gifts appear to lie in the spheres of language and inter-personal relationships.

Whatever the combination of current or intra-uterine hormonal activity, physical development, or social learning, the outcome is a typical psychological differentiation, not a dichotomization of man and woman. As we all know, some women are formidable viragos whilst some men are paragons of gentleness and, just as some women excel in mathematics or engineering, there are some men who find the fulfilment of their talents in more typically feminine areas. Sex differences there undoubtedly are, but they are in the realm of averages, not absolutes, and consequently they do not permit us to assume anything about any given individual's nature. Of late, people have rightly been becoming more sensitive to the need to treat others as they are, rather than as members of a class. Yet vulgar modes of thinking have always tended to obscure this distinction and have quite unreasonably projected onto the individual all the stereotypical characteristics of their group, whether this be nationality, social class, age, or whatever.

In the case of sex differences, most societies are now beginning to reject the restrictions on opportunity which have been laid upon the individual and, in so doing, have understandably emphasized the similarities and denied all except the undeniable differences. However, though the extreme case is dialectically most effective in the sphere of argument, one would be sorry to see it taken for more than a polemical device.

Regrettably, orthodox wisdom in psychology, too, has a propensity to reflect a rather extreme viewpoint on the subject of sex differences, tending to treat them as of minor significance and, in any case, as being almost entirely artefacts of social and cultural moulding. The reason for this is that one's conclusions are likely to be very dependent upon the range of evidence considered and psychologists have, on the whole, been rather conservative in making their selection from the multi-disciplinary scientific literature relevant to this issue. As a result, a good deal of psychological opinion is to be found reflected in compendia, like Eleanor Maccoby and Carol Jacklin's *The Psychology of Sex Differences*, which tend to assign to genetic influences a fairly minor role.

The conclusions drawn from such encyclopedic collations of psychological studies are, in the main, perfectly reasonable in the light of the material included. However, conventional psychological investigations are usually subject to at least two influences which make them less than ideal when considering biological variation: in the first place, they are generally conceived within the philosophically empiricist tradition which presupposes the primacy, if not the

universality, of learning and environmental pressures. Second, and as both a corollary and a tradition, the testing and study situations beloved of psychologists tend to emphasize those cognitive processes and segmental aspects of behaviour which are so particularly subject to cultural determination. In other words, they are still for the most part looking at a different range of data from that being considered by the biologist, ethologist or anthropologist. And, just as the clearness and certainty of any perception varies with one's point of view, we can surely hope to achieve greater intelligibility by using the vantage of several disciplines, rather than depending upon the narrow perspective of any one subject.

At the present time, however, it is really only in relation to the areas of aggression and sexuality that a substantial degree of interdisciplinary agreement is beginning to emerge, although, to a lesser extent, psychological accounts of genetic influences on visuo-spatial, numerical and verbal capacities are now becoming more generally accepted.

The evidence concerning genetically mediated sex differences is, then, somewhat scattered and patchy, but some of it is very persuasive and of a fundamental nature. For example, we have seen that the evidence for an early structural and functional differentiation of the foetal brain is compatible both with our behavioural and biological observations and with a whole range of experimental work undertaken on other animals, including primates. In my view, there is no convincing evidence for the opposite notion – that the nervous system, and other physiological systems connected with behavioural and psychological functioning, is essentially sex-neutral at birth. In fact, the phylogenetic comparisons make it seem a most improbable hypothesis unless one has other powerful evidence to the contrary.

Human sexual dimorphism is a fact; and physical differences influence the sort of adjustments we may realistically make, whilst our reproductive roles are also powerful determinants of a wide range of behaviours and inter-personal relationships. *Psychological dimorphism* is, though, largely a matter of inference – a belief that the typical roles of males and females are not the arbitrary inventions of society, rather that they are manifestations of a pattern which has been selected in the evolution of most mammals. Different species and different societies have commonly modified their expression, but it would be both biologically and psychologically unfounded to accept the fashionable notion that male and female roles are simply the outcome of some sort of archaic social contract or due to the physical domination and moulding of women by men.

There was no Garden of Eden; instead, our species differentiated gradually from others and, in so doing, was subject to exactly the same physical and behavioural shaping processes which evolution controls and genetics transmits. The selective pressures which originally led to sexual differentiations may have ceased to be of great importance in peaceful and settled agricultural and technological societies but, whether this is the case or not, the genetically established differences themselves may prove very difficult to change, even given a rational decision to attempt it. However, though I believe that a clear empirical case has been made for the existence of certain genetically controlled psychological differences between men and women, it is still not obvious how important these are in a psychological sense; or how desirable or undesirable they are in the widest social, political and military sense.

As scientists, we are principally concerned with the reason for these differences and the mechanisms through which they arise and operate. In this role we are quite justified in stressing the exploration of differences, although, as members of a society, we also have some responsibility for trying to make sure that our findings are reasonably *interpreted* in the wider social context. For example, though the evidence strongly indicates that males have relatively greater aptitudes for visuo-spatial, mechanical and numerical tasks, this does not mean that any given woman would be ill-advised to enter science or engineering but should rather choose teaching, social work, literature or fine dextrous work – where the special skills of her own sex lie.

The degree of overlap between individuals composing the two sexes is such that advice or direction based only upon one's sex is nonsense. Yet the representation of sexes in almost all occupations is already, by an order of many magnitudes, far different from what could be expected on the basis of established differences alone. Quite obviously, bio-genetic influences are only part of the process which leads to such universal differentiation of the sexes.

For practical reasons only, we have followed a fairly limited line of evidence so that, before coming to any final conclusions, we must needs look to the other social sciences for further material and an analysis of many of the complex developmental processes taking place within the socio-cultural matrix. It is a difficult area of research, and one made unattractive by the bigotry which is met on all sides. Yet it is an area of absolutely fundamental importance, not only for psychogenetics but for psychology itself, as the sex we inherit is almost certainly one of the greatest single factors in shaping our personality, and thus our outlook and experience of the world.

But above all, the issue is *not* about the superiority of one sex over another: our differences are the two sides of a single coin. It would seem that the male differentiations from the basic female pattern are the result of evolutionary solutions to the problems of biological survival and that it is our *inter-dependence* of qualities which is finally important, not our differences. However, if we are now to assume responsibility for some of these survival functions previously controlled by evolutionary and genetic processes, we shall require a greater knowledge and wisdom than is generally evident in this area.

Chapter seven

THE INHERITANCE OF PSYCHOSES

In the last chapter we found ourselves at something of a methodological disadvantage in handling our subject matter since the most usual and direct approach, that of family and twin studies, is not very helpful in elucidating the genetic components in psycho-sexual differentiation. By contrast, questions concerning the inheritance of mental disorders seem to lend themselves admirably to such methods, though it turns out that they too are beset with their own particular difficulties when it comes to identifying pathological manifestations and establishing the connections between the degree of genetic relationship and the mental ill-health criterion.

In fact there are several more or less different problems involved, though most of them turn on the skill with which psychiatrists can devise their classificatory systems and then correctly assign patients to particular categories. But, in addition to this, there are also marked national differences in heritability estimates due to there being no internationally agreed psychiatric categories comparable with those in general medicine. For example, Russian psychiatrists use classifications such as 'shift-like schizophrenia' or 'sluggish schizophrenia' which have no equivalent counterparts in formal European or American diagnostic systems. And, in fact, the official taxonomies in use not only differ from place to place, but they tend also to be modified and applied somewhat differently by practitioners and research workers alike.

In consequence, local and idiosyncratic variations in diagnostic classification make it very difficult to take a consensus of the research proceedings world-wide. Reports from both Russia and America show that in these countries people are more likely to be diagnosed as suffering from some sub-class of schizophrenia than they would be in other parts of the world. Political misuses aside, many of the social deviants who, in Britain, would be diagnosed as neurotic or subject to

personality disorders of other types, would quite confidently be placed in the more severe category of 'schizophrenic' in the USA or USSR. Other countries tend to be more comparable with the British pattern, though there are still very substantial variations.

Because the criteria of mental pathology are so heavily dependent upon different cultural standards, the results of genetic studies may be confounded by the fact that researchers may accept a wider or narrower range of behavioural abnormality when determining whether the behaviour of family members may be considered concordant or not. For example, if one is applying strict criteria for the use of the term 'schizophrenia' it may well be that the reported levels of concordance will be relatively low as even quite marked abnormalities in the families of index cases may not conform to the precise criteria being applied, and thus may not justify using that diagnostic category. On the other hand, where the stated criteria applied are very much less rigorous, and where inference, interpretation and opinion are more prominent, higher concordances are likely. The former case perhaps errs on the side of underestimation, whereas the latter may tend to overestimate the genetic effect by including cases which were actually not concordant.

Regretfully, it must be admitted that psychiatry, even more than psychology, is not a very exact science. Aside from fairly trivial stimulus-response sequences, it goes without saying that prediction in human affairs can only ever be a rough probability estimate as the potential range of internal and external influences affecting a single individual can never be fully known. However, though it may be just as impossible for the human scientist to predict particular behavioural events as it is for the biologist, say, to forecast the exact time at which a particular leaf will fall in the autumn, it remains perfectly feasible to derive general rules and causal relationships from the study of large numbers of cases – where idiosyncratic conditions tend to cancel one another out.

So we persevere at the more modest task of attempting to order our observations into conceptual categories so as to provide the generalizations necessary for the creation of theories which will allow us to predict hitherto unobserved events and relationships. But here's the rub: our theories will be no better than the observational generalizations upon which they are based and, because psychiatry is so very dependent upon opinion and the interpretation of limited behavioural samples, it is not surprising that empirical studies have all too often indicated disturbingly low levels of reliability or reproducibility – the result either of variable skills among the

diagnosticians, or of the intrinsic difficulty of applying imprecise categories. But, for whatever reason, if we cannot rely upon accurate diagnosis, genetic studies in the sphere of mental disorder are made very much more problematic.

However, it would be wrong to allow the acknowledged sources of imprecision in our data to result in despair or undue pessimism: rather it calls for a cautious approach to existing work, coupled with a determination to record criterion behaviour more fully and objectively in future studies. For the present, however, some degree of unreliability must be accepted in these relatively early stages of our investigations. And, as we shall see later, it may even prove that psychogenetic studies of the sort described may actually result in a revision of existing psychiatric taxonomies by uncovering relationships between types of mental aberration which have until now been conceptualized as more independent of one another than they really are. This, however, remains somewhat speculative and, at the moment, we have no realistic alternative to making our appraisal on the basis of studies which have utilized more or less controversial psychiatric diagnostic procedures.

Clinical usage may vary in the ways we have discussed, but fortunately there still remains a substantial degree of agreement among psychiatrists as to the use of very broad categories. The differences may be expected to attenuate our correlational studies but, unless the relationships being sought are rather marginal, such disparities should not entirely obscure whatever connections are there to be observed. In fact, it might be argued that if the significant correlations between mental disorders and closeness of blood relationship persist, and are regularly observed in spite of such limitations, then there is every reason to feel even more confident that a powerful causal principle is at work.

The problems referred to apply equally to most psychiatric syndromes but in this present chapter we shall, as far as this is possible, be restricting our attention to the psychoses – particularly schizophrenia and the affective types of reaction, such as manic-depressive disorder – whilst deferring our treatment of the neuroses and personality or character disorders to the next chapter.

To begin with, it is appropriate that we should give pride of place in our discussion of the psychoses to schizophrenia for, over a period of very many years now, this particular form of illness must have attracted more research than any other topic in the whole of human behavioural genetics. It is, however, an emphasis which has been completely justifiable not only in terms of the clinical and social

importance of this relatively common major illness, but also in terms of the very promising research findings and their implications for treatment.

Like any other mental phenomenon, it is exceedingly hard to be definitive as to the specific characteristics of schizophrenia. In the present case this difficulty is compounded by the fact that it is probably more correct to refer to '*the schizophrenias*' as it is quite usual to identify four sub-types, and sometimes more. Some workers, though, prefer to make a single distinction between paranoid and non-paranoid types, whilst others prefer to think of a more unified pathological process which simply manifests itself through different emphases in the presentation of psychotic symptoms and behaviour. As one can soon see, this particular area of psychopathology is an unusually complex one, and is currently fluctuating greatly, so our pursuit of a brief overview will probably best be served by a generic treatment, while at the same time advising the interested reader to consult detailed sources for more precise information relating to the diagnostic and pathological niceties.

There are no invariable criteria which may be said to occur in every case of schizophrenia; rather, it is diagnosed in terms of the syndrome pattern ascertained by clinical judgement. Broadly speaking, we are thinking of a range of disorders typified by a very severe impairment of psychological functioning, in which the patient may be expected to suffer from hallucinations of one sort or another – commonly 'voices' or 'visions'. Mental life tends to become fragmented by failures of attention, alogical thinking, lack of realistic judgement, apathy about the challenges of independent living, and a withdrawal from social interaction into a profound state of self-absorption.

In addition to the considerable differences which occur *between* schizophrenics and the manifestations of their disorder, considerable changes in the presenting symptoms may occur in the *same* patient over a period of time, as well as perhaps being interspersed with periods of more or less complete remission. Indeed, a very wide range of combinations and permutations is entirely to be expected with this disorder.

Whatever the problems of defining it, it nevertheless affects approximately one per cent of people at some time during their life, though it most commonly first appears in the period from the late teens to the mid-thirties, with a steep drop in risk thereafter. Both males and females are equally likely to develop schizophrenia, though the period of onset is earlier in males than it is in females. Accordingly, though any transmissible genetic factors should be limited by the fact that the

illness reduces the reproductive rate overall due to many patients being hospitalized or otherwise socially impaired at the peak time for marriage and producing children, nevertheless the age-range for onset is still sufficiently wide to allow for a moderate return to the gene pool.

As we have seen, the reported prevalence rates vary from country to country and though this may be due to actual gene pool differences, perhaps affected by the age of marriage, it may equally be due to other causes or contributory influences. For example, environmental stresses may differ from place to place or, as has been mentioned, countries may simply differ in diagnostic practices or standards of medical care. For whatever reason, disparities in the prevalence rates reported from western countries are great enough to raise a whole range of interesting and unsolved questions which, alas, cannot be pursued further at present. However, this is a point to bear in mind when it comes to considering and comparing the results of family studies carried out in a variety of countries.

It is not only the national morbidity estimates which vary; so too do the familial studies even within a country, though shared diagnostic practices do generally help to reduce the differences, and in fact, despite all the problems, some sort of broad average estimate is now beginning to emerge from the wide range of studies which have accumulated in the western scientific literature. But, as is usual with all topics connected with psychogenetics, the more extremist adherents of both the biological and environmental theoretical persuasions have been exceedingly active in promoting the significance of those studies which most strongly support their own views. In this context, the work of F. Kallman and of P. Tienari must surely have been the most widely used.

Kallman's work has generally suggested by far the highest estimates for the heritability of schizophrenia – though it should be noted that, by contrast with most of those investigators who have produced very different results, Kallman* tends to work with impressively large numbers of subjects. For example, his 1946 series, undertaken in New York on consecutive psychiatric admissions and an already existing in-patient source, included 174 pairs of monozygotic twins and 517 pairs of dizygotic. The results were equally impressive, indicating concordance ratios of 86 per cent for MZ twins to 14.5 per cent for DZ pairs. Kallman's studies have regularly produced this sort of apparently clear-cut genetic result, and have also shown the broader effect of decline in concordance rates which parallel the decline in genetic

* *Amer. J. Psychiat.* (1946) **103**, 309–22.

similarity as marked by increasingly distant family relationships.

On the face of it, Kallman had achieved what he claimed – that there is little, if any, room for psychological causality. But his results and his interpretations did not pass unchallenged. In the first place, it was objected that the observed concordance rates, though conforming in a broad sense to the expected pattern of a direct relationship between susceptibility to schizophrenic illness and genetic closeness, nevertheless failed to yield an acceptable match to any pattern predictable on hereditarian grounds alone.

Classical Mendelian genetics can be used to generate tables of expected values for either dominant or recessive genes, but neither of these accords with Kallman's observations. For example, whether a hypothetical gene, coded for schizophrenia, was dominant or recessive, the result should be that *all* MZ twins would develop the illness if the genetic coding is to be regarded as a sufficient cause. In other words, the concordance rates for people of identical genotype should be one hundred per cent – a figure never yet recorded in any rigorous study. A similar pattern of inexplicably lowered concordances also occurs in the results of both Kallman's and others' studies, for all degrees of family relationships. Undeterred, Kallman nevertheless explained his results by reference to a single recessive gene whose operational effectiveness or *penetrance* was affected by the activity of multiple minor genes related to the development of an individual's physical constitution. We have already noted, in chapter five, that studies like those of Kretschmer and Sheldon had indicated a marked connection between the ectomorph's linear and slight body type and the presence of schizophrenia or schizoid tendencies, and the rounded endomorph's greater tendency towards manic-depressive types of psychotic disorder.

It was Kallman's contention that the recessive gene mainly responsible for schizophrenia actually modulates an enzyme which, when deficient, results in a metabolic failure affecting an individual's capacity to handle stress. The seriousness of this deficit was, though, held to be dependent upon the activity of an unknown number of minor genes and the degree to which they were phenotypically expressed. This, in the very barest terms, is the essence of Kallman's work: it unifies and complements a certain amount of research from other sources (like that relating to somatotype), but it presently still fails the acid test of a thoroughly acceptable scientific theory in that its plasticity allows for the explanation of widely different empirical results on a *post hoc* basis, whilst the theory itself is not susceptible to a crucial test of its validity.

This latter point has been taken up by Böök who has attempted to produce a testable model by the simple expedient of specifying the penetrance associated with the gene – which, in his opinion, must be a dominant form. In general terms, what Böök* has done has been to look at the empirical results and then calculate a figure which, when used as a corrective factor in the generation of predicted results in heterozygotes, has been fairly successful on a number of occasions. By giving a quantitative value to the penetrance of his presumed dominant gene, Böök has met one of the objections to this type of theorizing. But the results of many later statistical studies, some emphasizing a major locus of control whilst other presupposed some form of polygenic model do not, up to the time of Kinney and Matthysse's recent review†, seem to justify the acceptance of any particular theory regarding the mechanisms of transmission.

In fact, the empirical findings have passed through so many vicissitudes that there have been times when looking for unifying explanations of apparently contradictory data looked to some like an exercise in sublime futility.

In 1963 the work of Tienari‡ in Finland really set the cat among the pigeons for a while, for he reported a study in which the concordance rate for MZ twins was zero, instead of a figure around the seventy-eighty per cent mark which was commonly being reported at that time. The reaction to this new piece of research was remarkable for, though it referred to an unusually small sample of cases, it was just what the anti-genetic faction was looking for and, moreover, it called into question the methodological rigour of most of the more established studies. It could not therefore be overlooked, or set aside until further corroborative evidence was available.

Tienari's critique of the existing work, though of great impact, was not entirely of his own fashioning as his main points derive from the work of the American psychologist David Rosenthal whose contribution to the methodogy of psychogenetics has been considerable, and whose compact little book *Genetics of Psychopathology* is one of the best and most readable of all introductions to this whole general area. In the event, many of Rosenthal's experimental refinements had been built into Tienari's study and the results have come out very differently from those of the then conventional approach. The question of *why* these differences had occurred has been widely and vigorously debated and, the inevitable doctrinaire polemic aside, the resulting struggle

* *Acta Genetica.* (1953) **4**, 133–9.
† *Ann. Rev. Med.* (1978) **29**, 459–473.
‡ *Acta Psychiat. Scand.* (1963) **39**, Supp 171.

with methodological ideas has for once probably resulted in the creation of as much light as heat. Issues about the selection of samples, the reliability of diagnoses, the non-rigorous criteria by which decisions concerning the zygosity of twins had often been made in earlier studies, and the appropriateness of statistical treatments often applied to results, were all given an airing. It was a *tour de force* which has since done much to raise the standards of experimental design and control in this entire area.

However, it would be very far from the truth to conclude that Tienari's critique and negative results struck a lasting blow against the effects of genetic determinants in the development of schizophrenia, for his own study soon became the subject of critical attention by such able researchers as James Shields. In parenthesis, one might just note that Shields' co-operation with Gottesman in their book on the genetics of schizophrenia remains a model, and major source, of information concerning the development of research, theory and methodology in this field – as is their more recent review of twin, adoption and family studies*. And, as it happened, Tienari's study was bound to attract close scrutiny from such workers, not only because the results conflicted violently with most previous work, but also because they depended upon a sample which was small by any scientific standard – involving only sixteen pairs of MZ twins. Unfortunate as the small sample size might have been, it was soon apparent though that this was not the result of any lack of experimental vigour, but the consequence of a very unusual methodological approach: instead of following the typical research strategy of accumulating a sample by locating single members of twin pairs who were already in hospital suffering from schizophrenia and whose brother or sister could then be traced and assessed for concordance, Tienari combed parish records for twin births and then set about tracing them to see how many cases of schizophrenia were involved. As we have seen, of the sixteen genetically identical MZ pairs traced in which schizophrenia was present in one twin, there was not one case where it was held to be present in both.

Tienari himself felt that his results reflected a less biased approach to the data for, in the case of already hospitalized schizophrenics, one may arguably be dealing with a selectively sampled ultra-severe category of such patients, among whom concordances might be expected to be elevated above their realistic baseline. But, though the general proposition that observed concordance rates depend very

* *Schizophrenia Bull.* (1976) **2**, 360–400.

much upon the way in which index cases are selected is now widely accepted, Tienari himself was not able to avoid yet another of the artificial hazards of sampling. Out of the 2,888 pairs of twins listed in parish records, only 39 per cent were traceable and, of these only 32 per cent proved to be monozygotic pairs, though the figure predicted from theoretical principles would be about 45 per cent. In other words, Tienari's sample may itself have been substantially affected by the loss of great numbers of potentially concordant pairs – due, perhaps, to such individuals being more frequently among those with unsettled life-styles, or the like.

Also, Tienari had criticized other studies on the grounds that the diagnostic criteria employed suggested a greater degree of concordance than was actually justified. Scrutinizing Tienari's own usage, Slater reported that, of the sixteen cases in which one of the twins had been diagnosed as schizophrenic, four of these might equally have been diagnosed as organic psychoses elsewhere. Of the remaining twelve, two of the twins listed as discordant had actually been described as 'borderline', and nine out of the remaining ten had been ascribed 'schizoid traits' and may well have been diagnosed 'schizophrenic' in other hands or in other places. So, a result which has been widely quoted as being contrary to the genetic hypothesis may well, it now seems, just as easily have been presented as an argument for it: the more so since a later follow-up of the cases revealed concordances in excess of 30 per cent.

More positively, the outcome of Tienari's work, and the subsequent discussion which it fuelled, has been to alert other investigators to the need for carefully considering the actual psychological status of the relatives of index cases, it not being enough to report concordances, or lack of them, on some sort of arbitrary all-or-none rule. As a result, there are now a number of established ways in which concordance rates may be calculated – depending upon alternative gradations of stringency in applying the psychiatric criteria. Further, the strategy has also led to the formulation of tactics of heuristic value in exploring the range of associated but distinguishable conditions which may be used as evidence for the heritability of marked psychiatric disorder, as opposed to the heritability of a particular syndrome-type. Many researchers nowadays quote more than one concordance rate, together with the diagnostic criteria that they have applied, and this is beginning to provide an excellent source of information about the probable relationship between what have previously more often been treated as independent psychiatric conditions.

It is therefore quite difficult, and potentially misleading, to adduce

any general level of heritability from the many extant studies of twins. After all, a heritability index for a given condition is not a single objective fact awaiting discovery 'out there' somewhere – it is simply a function calculated from somewhat arbitrarily limited observations of a given population of unique people inhabiting unique environments. And unless genotypes are assumed to be inevitably, and more or less uniformly, expressed regardless of environmental effects, a heritability estimate can never be more than a situationally-based approximation. Having said this, the heritability estimates for schizophrenia *per se* are sadly wide-ranging, though they do tend to cluster somewhere between the somewhat conservative figure of 50 per cent and the equivalently radical estimates of 80 to 90 per cent.

There are still those who cherish methodological objections to twin studies which are dependent upon a comparison of morbidities observed in MZ and DZ twins raised together in the same environment. It is argued that the environmental circumstances of 'identical' monozygotic twins are only superficially similar to those of the simply fraternal dizygotic pairs. As a consequence, it could well be that the great disparity between concordance rates for MZ and DZ pairs (which, in the latter case, are only of the order of 10 to 15 per cent) are grounded in environmental rather than genetic differences. The thrust of this argument is naturally directed at discrediting heritability estimates which suggest a high genetic component, and putting social determinants in the place of biological ones; and, because heritability estimates are computed on the basis of observed differences in the concordances for the two types of twin; the assertion is obviously directed at a level fundamental to the whole discussion.

However, this is a weak line of argument in that it is unsupported by any evidence to indicate that there are significant directionally-operating differences in the environments of MZ and DZ twins. In fact, it is a theory-saving device rather than a properly scientifically-based conclusion and should not be accorded any status greater than that of a tentative hypothesis. But, in cavalier fashion, there is no shortage of psychopathologists who simply ignore the unpalatable genetic or biochemical data and, instead, proceed to construct their theories as though they were free to choose whether or not they will consider all of the available evidence.

The thoroughgoing proponents of environmentalist explanations usually have very little to say on the subject of differential MZ–DZ twin concordances which, though they are not necessarily inimical to the theories proposed, would require a considerable amount of further explanation. It is, of course, quite possible that genetic predisposition

is not a *necessary* condition in all cases – a caveat which probably does more than anything to allow for the survival of quite popular family-dynamic accounts, like those of Gregory Bateson* and Theodore Lidz.†

Bateson's model is usually sub-titled 'The Double-Bind Hypothesis' because it conceives of the developing schizophrenic as bound by the contradictions of paradox: a child inescapably caught within a web of contradictory communications and obliged to make sense out of non-sense. A typical theme might be of a parent communicating love at one level but rejection at another . . . 'I do love you darling but just leave me alone'. Alternatively, one parent may signal one response whilst another negates it by means of contradictory communication of the sort 'Get down from Daddy, he's tired and wants to rest'. In such cases, the child cannot respond to both levels or sources of communication; it cannot escape without responding somehow; and it cannot respond without experiencing the punishing consequences of denying one directive or the other.

The result, it is argued, is that the child is forced to develop an alogical mode of thinking which serves him poorly outside the home and indeed, through his resulting social failures, drives him back into it so that the cumulative effects of isolation, alogic and dream become substitutes for real adjustments and achievements. Ultimately, this may produce a psychological disorder which we identify as a schizophrenic syndrome.

Lidz's approach also highlights the nature of relationships and the problems of analyzing communication in the home, though with more emphasis upon analyzing the relative vigour, positive participation and the emotional qualities of each of the two parents. In the event though, both Lidz and Bateson have created causal models which make the illnesses of schizophrenic children directly attributable to the behaviour of their parents.

Of the major theorists, Bateson has placed the greater responsibility on the so-called 'schizophrenogenic mother' whose child-handling practices are held to be the prime cause of her child's distress and psychological failure. Certainly a number of studies have revealed very imperfect models of relationship in the homes of schizophrenics, and have typically described a situation in which the mother has achieved a prominence at the expense of her more ineffectual and shadowy spouse, whilst herself characteristically over-controlling her charges and being emotionally cold in her transactions.

* *Behavioural Science.* (1956) **1**, 251–64.
† *Arch. Neurol. & Psychiat.* (1958) **79**, 305–16.

Even so, it is still by no means clear what is cause and what effect: one can see that the presence of a psychotic child in a family is, in itself, likely to alter the structure of family life. A father may opt out of an unsatisfactory or stressful situation much more readily than can his wife – she carries more responsibilities and must remain at the epicentre, coping as best she can. Thus parental behaviour, which may at first look like the *cause* of a child's unusual functioning, may actually turn out to be a *response* to it: it cannot be easy to maintain normal relationships in a small group containing a psychotic. And this is particularly the case where the family is to be put under the microscope for analysis: even the fairly naive could hardly miss seeing the implication of such studies – that the misfortunes of the child are to be sought in the circumstances of its upbringing.

The situation is especially cruel for the mother who not only has to bear the sorrow of having an impaired child and difficult home circumstances, but also finds herself widely branded as 'schizophrenogenic' in the articles she reads about the subject. It is as though she was some sort of moral degenerate who had infected her offspring with something more disgraceful than venereal disease through contamination with her own crippled and child-torturing personality.

All of which would be bad enough if the evidence was strongly supportive; but actually the practical problems of resources and gaining intimate access to families which do not have seriously impaired children has meant that we really have no established baseline for comparisons – nor the much needed prospective studies of non-selected cases where one could make predictions about outcomes and thus validate or invalidate the theories. The homes of schizophrenics may be absolutely no different from the homes of many non-affected children, or from homes in which any other form of illness in a child leads the family to develop adjustive psychological strategies, so the additional burdens of suspicion and condemnation which have often been heaped upon the heads of the parents of schizophrenics is doubly inexcusable. There is no clear evidence of otherwise healthy children being made schizophrenic as a result of environmental pressure. And, even if we do establish that the parents of schizophrenics act in an unusual manner towards their growing children, we still have to consider that this might be expected in a genetically transmitted condition where both parents and children share not only the same living space but also many of the same genes, and their correlates.

The most conclusive evidence would come from adoption studies made of twins separated at a very early age – thus obviating any of the

proposed psychological effects of their interacting together. Having been separated from each other and their parents, they not only experience different rearing environments but avoid the possibility of any special treatment being accorded to members of MZ pairs only. Adoption may lead to a number of later adjustment problems for the children involved, but there is no way in which it should selectively affect twins drawn from genetically identical pairs. So if the concordance rates for separated MZ twins proved to be higher than those coming from DZ pairs, then there would be a very strong case for postulating the operation of genetic factors. In the scientific sense only, though, it is regrettably the case that the number of twin pairs put up for adoption, whose zygosity is known, whose parent(s) are schizophrenic, and whose adoptive circumstances can be closely matched, has made such studies both rare and small in scale. Consequently, though the data which have so far been reported is compatible with a genetically-based mode of theorizing, its scale and range is such that it can presently only occupy the minor role of supportive evidence.

In the event, adoption studies of non-twin children of schizophrenics present a much better picture as there is no shortage of this type of case: the work of Leonard Heston and his colleagues offers good examples of the sort of result which continues to be reported by other researchers. One of these comparisons* followed-up forty-seven children born to schizophrenic mothers within a psychiatric hospital and compared their mental health with that of a group of controls who had similarly been separated from their psychiatrically 'normal' mothers soon after birth and placed in the care of others unrelated to them. The results were quite clear-cut: not only were the children of schizophrenic mothers significantly more likely to become schizophrenic adults themselves, but they were also very much more likely to suffer from mental deficiency, sociopathic personality and neurosis, outcomes which perhaps also go some way to explaining the fact that this group was also found to be more likely to engage in criminal activities.

Additionally, there were other measures which also indicated a very significantly lower 'mental health index' for the target group as a whole. All the children involved had experienced normal births and were apparently healthy and normal as infants, yet the differences between the developmental sequences of the offspring of schizophrenic and psychiatrically normal mothers could not be traced to any

* *Brit. J. Psychiat.* (1966) **112**, 819–25.

differences in their environmental circumstances. It must therefore be concluded that the results reflect genetic differences.

Heston's work is not only valuable as a contribution to the general literature bearing on the inheritance of schizophrenia itself, it also highlights two additional points. In the first place, the fact that the progeny of schizophrenic parents are also more susceptible to a range of other adjustment dysfunctions strongly suggests that what is inherited is not a specific gene for schizophrenia but rather a more complex and variable polygenic combination affecting a range of endocrine and nervous system processes. Somatotype correlations with mental health are quite compatible with this view and we have already remarked on the clear polygenetic nature of physical development and intellectual capacity. Furthermore, beyond the question of content, manifestations of the schizophrenic syndrome itself are of such diversity as to suggest that a variety of mental processes may be affected in different degree and combination to yield the range of abnormalities which we describe generically as 'the schizophrenias' or, as is implied by Heston's results, mental balance more generally.

There is, though, a sense in which research like Heston's leaves the door ajar for additional, non-genetic, factors when accounting for greater susceptibility in the children of schizophrenics. However, some recent half-sibling adoption research reported by Seymour Kety and his colleagues*has shown that a great deal can be done to close this gap. Specifically, analysis of morbidity rates for children whose father only was a schizophrenic has proved particularly useful in eliminating factors like pre- and peri-natal conditions and the post-natal maternal rearing patterns which have so often been evoked as alternatives to the interpretation of genetic causality. The research design involved following-up 147 pairs of children who had a schizophrenic father in common yet had been born to different, non-schizophrenic, mothers. Subsequent adoptions led to these half-siblings being separated and brought up in different environments: a situation where obstetrical features or maternal pressures could not be used to account for any elevated concordance rates which might be found.

In the event, the very large number of comparisons which have now been made with other adopted pairs, matched in all obvious respects except parental schizophrenia, quite clearly show that the children of a schizophrenic father are substantially more likely to be afflicted by, and concordant for, schizophrenia than the children of non-

* *Psychiat. Annals.* (1976) **6**, 6–15.

schizophrenics. Speculations about the greater pathogenic potential of mothers must, therefore, continue to be treated with suspicion.

As might be expected, because of the unusually wide range of genetic and environmental interactions that can be teased-out from adoption studies involving half-siblings, either or both of whose parents may exhibit a given psychological characteristic, this technique is rapidly becoming one of the most prominent research strategies in the area of mental disorder. No doubt the present flurry of experimentation will continue until the causal worth of many variables proposed as whole or part explanations of familial inheritance has been assayed.

But to return to Heston's findings for a moment, there was also a most intriguing second conclusion to his research. This derives from the finding that the twenty-one children of schizophrenic mothers who did not evidence any psycho-social impairment were not just free of psychiatric disability, they were positively more creative and imaginative than the control group born to normal mothers. So it may be that different combinations of inherited qualities and environmental circumstances can lead either to mental disorder or to a more rich and flexible style of perception and thought. However, this conclusion is perhaps stretching the results beyond what is justifiable in terms of either their extent or their methodological rigour; but this sort of speculation is not without a rationale as we know that the same chemical substances which may create madness in one person may induce rich creative experiences in another.

In an earlier book on psychedelic drugs, I drew together the variety of such outcomes and also discussed the relationship between schizophrenia and the effects of a number of psychedelic substances. These drugs, although not manufactured in quite the same form by the human body, nevertheless are close cousins to several which are. Moreover, these naturally occurring biochemicals act mainly upon the nervous system to affect the transmission of nervous impulses by inhibiting, facilitating or spreading the neural flow activity. It seems likely that a range of mental disabilities from the most severe to the quite mild, as well as the shades of normal functioning and the capacity to cerebrate at a more creative and richly associative level, may all be affected by one or more sets of key biochemicals under the control of genetic mechanisms.

As to which chemical groups these may be, opinion is very much divided and, indeed, the mechanisms are not strictly central to our own line of thought anyway. Sufficient just to note that research which has aimed at comparing schizophrenics with normals has resulted in many

positive correlations, particularly those involving abnormalities in platelet monoamine oxidase (MAO) and certain antigens. There has also been a strong tradition of interest in the metabolism of noradrenaline and adrenaline. The biochemical products of these transformations are structurally very similar to the psychedelic *mescaline* and, as I indicated in my earlier book, it requires only minor flaws in the metabolic process to yield a number of chemical forms known to induce psychotic-like behaviour when administered to a normal person. There are alternative theories about where the biochemical failure might occur, but the conceptual net is tightening as a result of much back-tracking from the observations of metabolic abnormalities in the body fluids of schizophrenics.

So, given the known psychotomimetic action of the substances involved, and the several other different but corroborative lines of research, it seems as though the genetic basis of this illness may well turn out to centre upon the metabolic control of this (or a similar) group of neurochemicals. Not all research is compatible with these findings, but it seems to me that this is a case where we are justified in thinking of several causal possibilities leading to one or another of 'the schizophrenias' rather than remaining fixated upon the idea of a single syndrome.

But even though we are on the very threshold of identifying the actual biological processes involved, there still remains the problem of accounting for discrepancies between the *predicted* outcomes of a genetically induced condition and our *actual* observations. Not only is there an embarrassingly large proportion of genetically identical twins who do not both succumb to the illness, the concordances for all degrees of blood relationship are depressed.

One explanation may lie in the way we have lumped together so many heterogeneous behavioural abnormalities and given them what may be a false unity – including, perhaps, some forms which have no genetic basis. And, given the wide variation in diagnostic conceptualization and skill to which we referred earlier, this may well account for some lowering of expected concordances.

However, the main theories which seek to account for these discrepancies depend mainly upon the interaction of genetics with environment. This is obviously not the place to become too involved in the many variations of this theme, generically referred to as the 'diathesis-stress model', but some acquaintance is not only desirable but mandatory. The model's provenance is not important, and though it has a long history under a variety of names, it really boils down to a rather simple central premise: that we all have our own particular risk

level – the diathesis part – which is modified by the stress characteristics of our experiences. So, the diathesis term in the equation is conceived of as not only a genotypical predisposition, but as the sum of the interaction of this with our experiences and learning up to any given moment in time. Our resistance or vulnerability to schizophrenia is therefore invested in our phenotype, in our ability to handle stress. Consequently, two genetically identical twins will not only encounter different stress or conditions over time, but they may also be expected to differ in the success of the adjustive strategies with which their personal experiences have provided them.

The diathesis-stress model is therefore one which emphasizes the complex interaction of subtle qualities, both physical and psychological. By its nature, it introduces unspecifiable sources of variability which imply that psychiatric outcomes will not only be bound to differ between twins but will become multiplicatively more divergent within families – thus accommodating the old problem of observed concordance rates failing to match those which would be predicted on classical genetic principles. But though the model quite satisfactorily allows for the influences of both heredity and environment, it must be admitted that it does so in a manner which is completely non-testable in a rigorous sense.

Needless to say, hard-line proponents of the environmentalistic approach have found ways of discounting those implications which do not suit their preconceptions, just as have the more genetically minded. It is not my intention to become too deeply involved in the arguments which have been generated, and which cannot be resolved, regarding which are the salient terms in the diathesis-stress equation. One can see how fraught with difficulty are the steps from correlations to explanations, but the evidence which has now been amassed from carefully controlled studies leaves no serious doubt in my own mind that a predisposition to schizophrenia is inherited in most instances – though I fully accept that it does not follow that any particular case must necessarily involve hereditary causes.

Adoption studies from many parts of the world have now made it abundantly clear that schizophrenia in a biological parent will, compared with other adoptees, markedly increase the likelihood of a child becoming schizophrenic itself. The risk of becoming a schizophrenic is, from the child's point of view, much the same whether it is brought up by its biological parents or in an adoptive home and, in any case, is very much greater than the general morbidity rates for adoptees. Findings like these, and the studies of twins generally, have left little room for accounts which omit a genetic

component. The connection is no longer seriously in dispute: it is now really only the mechanisms which are unclear, and what magnitude of heritability estimates are appropriate.

The sort of high levels originally proposed by Kallman are quite certainly no longer compatible with the data being accumulated: a fact which may not be unconnected with the radical changes which have taken place in medical practice and psychopharmacology during recent years. Tranquillizers, and especially the phenothiazines, have transformed the psychiatric picture and have done much to reduce both the quantity and the quality of schizophrenic episodes. So, many who would previously have sunk into the full-blown syndrome may now avoid this altogether, or they may show only mild forms of psychiatric disturbance, or else recover quickly enough to avoid a final diagnosis of schizophrenia. Forewarned being forearmed, it may also be that when one member of a family has developed a psychiatric condition, an extra amount of preventive attention or prompt care is given to the others.

But, for whatever reasons, modern medical and psychiatric care has done a great deal to improve the effective environmental circumstances in which the predisposed genotype develops and so may well be responsible for significantly modifying the heritability estimates of schizophrenia – which are, after all, a function of the interaction of genetics with environmental circumstances. All we can say with any degree of conviction is that a genetic *predisposition* to schizophrenia has, in a general sense, been demonstrated and that our most promising hypothesis of the present suggests a form of polygenic transmission, or possibly a single dominant gene acted upon by a number of modulating genes, and having a penetrance which is further modified by the psychological stressor characteristics of the environment.

Diathesis-stress theories of this type, whilst being little more than statements of general principle, are nevertheless completely congruent with our existing data, and are widely acceptable amongst psychopathologists of many otherwise different persuasions. This is because the general principle involved does not insist upon the sort of complete determination, and therefore predictability of outcomes, which has no empirical support. Instead, it allows the much more popular and less doctrinaire premise that unique outcomes, healthy or pathological, may result from one's personal fortunes and the ways in which the interactions between environmental circumstances and personal dispositions have been structured into more or less effective psychological patterns. Areas of dispute therefore most often tend to

be about the *relative salience* of particular causal factors.

As we noted at the beginning of this present chapter, schizophrenia is not only a common and terrible malady of mankind, it also marks an exceedingly well-developed area of psychopathology – one in which many of the basic problems and research strategies in psychogenetics have been worked out for application to other disorders. Unfortunately, few of the other areas can even begin to compare in quality or quantity with schizophrenia research, though disorders like manic-depressive psychosis do share not only many of the same research methodologies but also many of the same principal research workers.

In fact, manic-depressive psychosis is second only to schizophrenia as an area of research, and is the major syndrome in the grouping generically termed the 'affective psychoses'.

The category of affective psychoses, referring as it does to psychoses in which emotional or mood disorder is prime, is fairly unexceptionable except that there is some reason to doubt whether its main subgroup – 'manic-depressive psychosis' – exists as a unified syndrome. Historically, manic-depression dates back to the activities of the great psychiatric classifier Emil Kraepelin who, towards the end of the last century, described cyclic swings in mood which do not seem explicable in terms of circumstantial causes, and which may oscillate from a lethally overactive state of excitability, through normality, to one of equally severe inertia with depression.

In fact, it transpires, about a quarter of the people classifiable as manic-depressives actually swing from mania to depression: most simply experience more or less distressing shades of mania or depression. However, the justification for grouping all such cases together is not arbitrary or unreasonable: it derives from a similarity in the symptoms observed in those who oscillate between the two opposite moods, and those who oscillate within one sector or the other. Additional superficial similarities have also suggested the psychiatric rationality of a conjoint grouping but, though modern taxonomists could hardly fail to harbour misgivings at the way in which this class has been formed, research findings seem to support its usage. Indeed, the psychogenetic results would appear to suggest a more homogeneous grouping than exists in the case of schizophrenia.

Establishing a baseline morbidity estimate for manic-depressive psychoses in the general population presents much the same difficulties as with schizophrenia: the range of estimates in this case too being susceptible to the source of sampling, diagnostic skill, conceptual usage, and so on. However, a figure of 0.7 per cent, or 7 cases per 1,000, is generally accepted as realistic. Unlike schizo-

phrenia, manic-depressive psychosis is not related to membership of the lower socio-economic classes, though it is rather more likely to occur in females than in males.

Rosenthal, reviewing the literature and looking for average values in the many extant studies, has selected a median measure as most representative and proposes figures of the following magnitude from family studies of first-degree relatives: having a manic-depressive parent will elevate one's own risk factor from the population baseline of 0.7 per cent up to 7.6 per cent and, where the illness occurs in one sibling, the risk rate for brothers and sisters moves up to 8.8 per cent. Not massive effects, but quite pronounced and, in cases where both parents have suffered from manic-depression, the risk factor moves up to somewhere in a range of 20 to 40 per cent.

Twin studies have also yielded marked positive results, though the range is a very wide one and, on a predominantly genetic line of causal argument, particularly puzzling in respect of DZ twins whose concordance rates have been reported as high as 37 per cent and as low as 0 per cent! However, there is no mistaking the powerful hereditary effect when comparisons are made with the genetically identical MZ twins – for whom concordances range upwards from 50 per cent to an astonishingly high 100 per cent reported by Kallman.

But, despite the fact that Kallman's survey was the largest in scope, his results were based on age-corrected projections and it is further argued that his results are elevated because of his sampling methods. That is to say, by drawing his index cases from those who are hospitalized, he is likely to focus only upon the more extreme examples of the syndrome: ones which might be atypical. The criticism is a reasonable one and experience shows that other ways of selecting the index case invariably result in lower levels of concordance. Nevertheless, results like Kallman's do unequivocably make the case for considerable genetic involvement in the more extreme forms, though they still leave us to speculate about the possible modulating effects of environmental circumstances in less marked manifestations of the syndrome.

So far as trying to clarify the mode of inheritance is concerned, the research results currently available are not very conclusive for, as with schizophrenia, the pattern of blood-relationship concordances does not sufficiently well match any of the several predictions which can be made in terms of either dominant or recessive gene action. In consequence, there is no widely accepted theory of causality, though most scientific opinion would rule out a recessive mode and evince little enthusiasm for a polygenic theory, despite the latter's compatibility with the gradations of observed severity. The most likely theory

is, then, that manic-depressive psychosis results from the action of a single dominant gene of lowered penetrance.

Naturally, psychological factors are not precluded in any way, and many theoretical formulations stress the experiential-environmental component in one or another of diathesis-stress types of explanation. Simple genetic-biochemical accounts seem to be ruled out because of the flattened heritability pattern in just the same way as do pure environmental accounts – which contrive to make a most unconvincing job of accounting for the observed concordance rates, particularly those highlighting the different morbidity risks for MZ and DZ twins.

Studies of the environmental circumstances surrounding the onset of manic-depressive psychosis have, on the whole, been disappointing in the range of developmental correlates identified. Although contemporaneous environmental circumstances such as loneliness often do have marked effects on the occurrence and intensity of the psychotic interludes, particularly depression, the pattern is so variable and the data so equivocal that it cannot be accepted that, as with the reactive types of depression, the prevailing environmental circumstances are a major cause of the severe psychological disturbances which occur. Indeed, the suicidally depressed patient not uncommonly comes from what are apparently most satisfactory social, economic and occupational conditions. Even where environmental circumstances prior to breakdown leave a good deal to be desired, the pre-clinical effects of the patient's own behaviour may well have had the effect of provoking dismissal or disrupted social relationships, rather than being their cause.

But this is not the place to become embroiled in the complexities of *post hoc* theorizing: sufficient just to note that no satisfactory case has yet been made to account for the development of manic-depressive psychosis on the exclusive, or even predominant, basis of environmental circumstances. Actually, it would be surprising if this was so as the taxonomic usage of all the endogenous classes of disorder, such as manic-depressive psychosis, are only used diagnostically in cases where current environmental pressures have been ruled out as being a sufficient cause of breakdown.

Studies of biological variables have, however, been more successful in locating the likely mechanisms of predisposition. Once again, as in the case of schizophrenia, it is the cerebral monoamines such as noradrenaline and also tyramine which look particularly promising as causal factors. Abnormal amounts of these substances are thought to accumulate in the brains of those susceptible to psychotic depression

thus severely impairing or short circuiting the activity of neural pathways and, as with schizophrenics, quite typically producing not only mood disorder but also abnormalities such as logical failures, severe perceptual distortions and hallucinations.

The empirical research completed along these lines continues to look promising, as do studies which focus upon mineral metabolism. It appears that psychotic depression correlates with abnormalities in body sodium level and its metabolism and, as we already know how important this chemical is in facilitating the transmission of nerve signals, it seems quite likely that disorders in this particular metabolic sequence might have serious psychiatric consequences.

The problem, as always, lies in deciding what is cause and what effect: whether the defective sodium metabolism is linked in some way with the cerebral monoamine abnormalities and, if so, how. And, from our own point of view, the salient question is how these various research findings relate to genetic variables generally, and the issue of heritability in particular. It might be, for example, that all the bodily disorders observed are the *result* and not the cause of severe psychological stress. However, the familial and twin studies of concordance presented so far argue a genetic mode of transmission and, if this is the case, physiological abnormalities of the type which have been reported would then be expected to occur as a result of anomalous genetic activity. But, as the concordance rates fall well below what might be anticipated on genetic grounds alone, we may still expect to find the cause of phenotypic expression in some combination or permutation of physical and psychological predisposition.

Up to the present time, however, our knowledge of what may constitute effective sources of psychological disposition has remained rudimentary, though a few interesting connections have emerged. For example, the early life of people who later suffer from manic-depressive psychosis is frequently severely disturbed and many, though not all, researchers have found evidence that the patient's mother is more likely to have died, or be otherwise absent, during infancy. Explanations as diverse as a dammed-up' anger at this deprivation and the presumed effects of a relatively impoverished emotional matrix in childhood have been proposed, but remain tentative theories. The pattern is simply not clear enough to justify any strong views at present. Better established are findings that severe mental disorder of kinds other than affective psychoses are also more likely within families of manic-depressives, and that alcoholism or recurrent drunkenness in the parents is more common.

Even more interestingly, the risk of schizophrenia is significantly elevated in the children of manic-depressive patients, one researcher (B. Schultz – see Rosenthal) having reported the risk to be raised from its general population baseline of 1 per cent to 12 per cent if both parents were manic-depressives; and Rosenthal's review of the literature suggests a median value of 2.5 per cent where only one parent was manic-depressive. So, as we noted when our focus was upon schizophrenia, manic-depression too is associated with not only a different form of psychosis, but also with the increased likelihood of members of the family manifesting personality disorders of a very wide range of types which are normally regarded as belonging to quite separate classes of disorder.

In fact, not only are the functional psychoses which we have been discussing subject to genetic determination, but the research done upon them indicates that it may be appropriate to adopt a more unified view of the causes of psychopathology than has generally been entertained. This is a point which we shall be developing throughout the next chapter, but it would be as well at this point in the discussion to say that, although the genetic relationships which emerge from the consanguinity studies of all types of mental disorder suggest a genetic component and may lead us to accept an emphatic genetic position in many instances, the causal circumstances of any given case may still be of a quite different type.

After all, every similar event need not have the same cause and it would be arrogant indeed to dismiss out of hand other, more specifically derived, causal analyses simply because we have hit upon some commonly prevailing relationships. Clinical studies of individual patients may well reveal no family histories of a sort which would suggest a hereditary explanation, whereas a behavioural analysis might give rise to a convincing theory of causality. In all such cases, one would hope that the issue might be considered in relation to all the evidence rather than in terms of just one set or another of doctrinaire beliefs.

Chapter eight

PERSONALITY DISORDER AND NEUROSES

Not very long ago, the distinctions made between psychoses, neuroses and other serious personality or characterological disorders were often such as to suggest that there was very little ground between them. When this happened, it tended to be because psychopathologists were anxious to identify discrete syndromes of the sort encountered in medicine, and because a taxonomy is more readily constructed with definitions than it can be by entertaining all the messy facts of real situations. Rigid and exclusive diagnostic systems have been under attack for some time now, for many reasons, but psychogenetics has also contributed usefully to the new more fluid approach.

Of course, the breaking-down of exclusive categories had not occurred at an equal rate in relation to all non-psychotic syndromes and may not, on *a priori* grounds alone, necessarily be expected to do so. But, as we can see in relation to the evidence emerging from psychogenetic research, some disorders – like alcoholism and psychopathic personality – have been shown to exhibit quite distinct familial connections with the psychoses, whilst evidence relating to the neuroses will also require us to consider a whole new range of possible relationships.

Perhaps though, purely as a device for organizing our discussion, it will be clearer if we deal first with disorders usually described as of personality or characterological origin, and from there proceed to the 'classical' manifestations of neurosis.

Given that it is not easy to accept an hereditary component for any human behavioural pattern, alcoholism must surely stand out as one which is even more difficult than most to contemplate from this point of view – except, that is, for those who are themselves alcoholics or who have had a wide experience of it. Generally it is dismissed as a simple expression of waywardness or fecklessness resulting from habits acquired in bad company, and perhaps reinforced by the feelings of

guilt and failure which are likely to arise from a life of regular drunkenness. However, we have seen already that other clues exist, and there are more than just a few other straws in the wind which seem to indicate another, genetic, possibility.

Whilst not in themselves always persuasive lines of evidence, animal studies have nevertheless produced some very thought-provoking results, some of which have been summarized in articles by Gilbert Omenn and Gerald McClearn in the 1972 Conference Report presented in the Annals of the New York Academy of Science. They refer to a great deal of research, particularly with rats and mice, and give very many examples of studies in which animals' relative preference for fluids containing alcohol could be shown to be related to their genetic background. Selective breeding can be used to produce rat or mouse strains which have either high or low preferences for alcohol. Furthermore, studies in which young animals from one strain have been cross-fostered with those of the other have shown that the alcohol-related tendencies are unaffected. So, in some forms of laboratory animals at least, we have evidence that alcohol preference, and also sensitivity to its effects, is not entirely a matter of learning and environment – even though it is possible to demonstrate the powerful modifying effects of conditioning.

Human beings are, of course, very different from the animals studied in laboratories, but our physiology is nevertheless of a very basic mammalian pattern and there is no very good reason to suppose that what is true for our phylogenetic second cousins is not applicable to ourselves. Indeed, the evidence from human research seems to indicate that it is. Many alcoholics and specialists in this area have long argued that the oft quoted recipe of hedonism, moral weakness and the strength of habit, were not sufficient to account for the addiction of many tragic cases: some instances perhaps but, it is argued, the difference between habitual social drinkers and alcoholics proper can only be explained on the assumption that the differences depend upon differing constitutional reactions to alcohol. Again, one must look elsewhere for the substance of these theories, ideas and arguments, as space will not allow us to pursue them here; suffice it to say that they enjoy a good deal of support and are quite compatible with extant psychogenetic studies. Moreover, the animal research is beginning to point the way to our understanding of the possible genetic mechanisms involved by uncovering abnormalities in certain metabolic processes.

Returning to the heritability of alcoholism in human beings, one must say at the outset that the evidence available is very much less clear-cut. One reason may be that 'alcoholism' is a diagnostic

category which groups together many different types of people having in common only the, perhaps superficial, fact that they are chronically intemperate or drunken.

Another reason may be that, unlike some other mental illnesses, not only are a predisposition and the appropriate psychological circumstances required, so too are alcohol and the opportunities for drinking. Not all upbringings are permissive of drink – or at least not in sufficient quantities and occasions to consolidate the amount of metabolic and social-psychological preparedness necessary to trip the individual over a threshold into the syndrome known as alcoholism. This heterogeneity in diagnosed cases is, of course, somewhat speculative and things may not be like this at all, but case-histories and family studies are suggestive in this respect. Either way, the effects of social learning are not to be under-estimated in constructing a genetically-based theory.

The weakest evidence for genetic involvement inevitably comes from investigations of prevalence within whole families for, although the literature reveals quite significantly elevated concordance rates, particularly with respect to the fathers and brothers of male alcoholics, the effects of imitative modelling and commonly shared attitudes and experiences cannot be ruled out. On the other hand, more restricted sources, like those deriving from twins contacted through registers, have gone some way towards making a more convincing case for an inherited predisposition. For example, Kaij in Sweden has shown highly significant differences between the concordance rates for MZ and DZ twins. The genetic component seems to be there, but the much higher than expected concordances among DZ twin pairs also argues that important environmental circumstances are at work.

The strongest evidence yet available for a psychogenetic theory of alcoholism comes from Denmark, from the work of Donald Goodwin and his colleagues who traced the fortunes of fifty-five male progeny of alcoholics (i.e. alcoholic fathers in 85 per cent of cases) and an otherwise matched control group, all of whom had been adopted by unrelated foster parents before they were six weeks old.

A wide range of behavioural data and indices of mental pathology was collected, most of which turned out not to discriminate between the groups in any way. What did stand out were the drinking problems of the fostered children of alcoholics: they had significantly more problems of a general kind relating to the use of alcohol – which might in turn relate to the fact that they evidenced three times the divorce rate of the control group – and the ratio of their being diagnosed or treated as alcoholics was as high as four to one. As the authors of the research

concluded, results such as these can leave little doubt as to there being a substantial genetic component involved in many, if not all, cases of alcoholism and failure to handle alcohol adequately. So it turns out that, on the basis of both animal selective breeding experiments and human twin surveys, alcoholism may, after all, fit fairly comfortably into a psychogenetic framework of explanation.

The work of Goodwin and his colleagues continues along ever-broadening lines, and now includes adoption studies of the daughters of alcoholics*. The women involved are still quite young and so have some way to go in the total risk period, but preliminary results indicate that they are not only more susceptible to alcoholism (though less markedly so than the male progeny) but are also more prone to adjustment problems and depression.

In fact, a broad spectrum of psycho-social maladjustment connected with genetic factors in alcoholism is beginning to emerge from several sources and this, rather naturally, leads us to questions concerning the specificity of the characteristics inherited. Animal research suggests that, in some cases at least, a pharmacological predisposition might be involved, but, after all, there can be many reasons for the immoderate use of alcohol – including attempts to manage anxiety or depression, or just very poor impulse control.

Of course, heavy drinking and delinquency are very frequently associated and both have in common the fact that many of our most respected theories tend to emphasize inadequacy and abnormal social learning as their cause. Nevertheless, in both cases, the accumulating evidence from psychogenetics is beginning to indicate that deprived socio-economic circumstances and inter- and intra-personal conflicts are only part of the story.

A good recent example of the sort of research findings which are upsetting traditional theories is not so easy to find, but one of the more provocative ones comes from America, where Raymond Crowe† followed up the adoptive offspring of women offenders in order to see whether criminal tendencies emerged more frequently than in otherwise comparable cases.

The mothers were all convicted inmates of a penal establishment and had been imprisoned for a range of offences, many of which, if culpable at all in other times and places, would simply be seen as minor sexual or social infractions. Nevertheless, 41 such women gave birth to the proband group of 52 babies – 27 boys and 25 girls – all of whom

* *Arch. Gen. Psychiat.* (1977) **34**, 751–5. and *Arch. Gen. Psychiat.* (1977) **34**, 1005–9.
† *Arch. Gen. Psychiat.* (1972) **27**, 600–3.

were adopted at an early age and followed up at a stage when their average age was about 25 years. A control group of adopted children born to non-delinquent mothers was drawn from the records for comparative purposes and the offspring themselves were similarly screened. The results obtained showed that the children of female delinquents were significantly more likely to be involved in criminal and anti-social behaviour than the children of non-delinquent parents.

Crowe's study is a real attempt to link criminal behaviour with genetics but, in my own opinion, it achieves something else instead. The criteria of delinquency applied in the cases of both the mothers and their children seem, even more than is usual in this area of research, to tap something more akin to social maladjustment than to criminal behaviour proper. But, whichever way we agree to view the problem behaviour reported, we are presented with statistically significant differences for which there is no apparent explanation in terms of environmental pressures or imitative learning. One may be inclined to accept the proposition that unknown genetic processes are at work but probably disinclined to view the connection as anything like inherited *badness*.

What these unknown processes may be is still an open question and, of course, they may differ with respect to different categories of criminality: sexual crimes may depend upon the operation of one set of physiological contingencies whereas crimes of violence may depend upon utterly different processes. The road to embezzlement, shoplifting or grand larceny may depend upon the effects of yet other heritable substrates of personality function though, in any particular case, each or any of these behaviours may be no more than rational adjustments in the face of circumstances and the effects of one's own social learning.

Indeed, a category like 'crime' can have no meaning in a biological sense – after all, criminal behaviour depends upon laws which reflect not only those things which are typically repugnant to our species, but also more ephemeral social tastes and morality. For example, whilst not being the main substance of the study, Crowe's tally of criminal behaviour included several irregularities in respect of traffic violations and sexual acts – some of which, like prostitution and adultery, are not only *not criminal* in contemporary Britain, but are not even morally condemned with any great vigour nowadays.

Nevertheless, that the women involved were sometimes imprisoned for apparently minor offences does seem to suggest that they might well have been habitual offenders. But without adequate court records, and indeed the reassurance of knowing that they were not

intellectually dull and seen as being more in need of care than punishment, one must feel very uneasy about such data. In fact, it is hardly too much to say that the circumstances which lay behind all cases so collected should merit at least as much attention as the subsequent experimental and statistical treatment of the study. But if this degree of caution is exercised, it is rarely obvious in the published reports.

Fortunately, however, there are some categories of 'criminal' behaviour, like homosexuality, which nowadays tend to be more carefully researched because they have been accorded psychiatric status. Even so, until quite recently male homosexuality would have been one of the major criminal categories in places where it is now freely tolerated.

In consequence, the results of studies which use conviction or any other legal criterion for inclusion in a quasi-psychological grouping of 'criminals' or 'delinquents' will be highly dependent upon the action of a local, not a biological, law. And, in fact, heritability estimates of criminality which fail to take account of this are likely to yield very misleading results. As we shall see, research findings make homosexuality an excellent case in point because of the degree of familial inheritance which seems to occur. So, as laws change, the composition of portmanteau groupings will change and, in so doing, it may very well greatly affect our conclusions about the heritability of 'criminal' tendencies.

Unlike some other sexual behaviours, homosexuality is actually more likely to be studied separately from criminality nowadays. However, much of the earlier work was based upon convicted homosexuals and so not only was supplementary information more limited than might otherwise have been the case, the patients were often exceedingly atypical and only available for study by virtue of the fact that they had acted flagrantly or recklessly – as when soliciting strangers or becoming involved with children. Earlier studies often reported remarkably high concordance rates when probands were selected on the basis of convicted or otherwise very extreme cases. For example, one of Kallman's series came up with a concordance rate for MZ twins of 100 per cent* – a figure which has been unrepeatable when cases have been drawn in other ways. The more modest figures reported by such workers as Heston and Shields† certainly suggest a genetic component but they too highlight the peculiar difficulties

* *J. Nerv. Ment. Dis.* (1952) **115**, 283–98.
† *Arch. Gen. Psychiat.* (1968) **18**, 149–60.

involved in partialling-out environmental influences and collecting representative samples.

Despite the fact that the extant research is neither overly abundant nor particularly clear-cut, Shields' recent review of the literature* indicates that there continues to be sufficient evidence from familial studies to suggest the reasonableness of a genetic interpretation in many instances – though what the intermediate mechanisms might be is still a matter for fierce conjecture given the many alternatives proposed on the basis of one physiological or anatomical correlation or another. Despite innumerable reports that male homosexuals significantly differ from heterosexuals on such measures as sperm-count, gonadal development and hormonal balance, replications have generally failed or experimental contaminants have later been identified. Consequently, no clear line of evidence has yet emerged, perhaps because homosexuality can result from an almost infinite range of antecedent events both physical and psychological.

Of course, few people would be likely to assume that genetic factors are *necessary* to the development of homosexual behaviour. Evidence from other times and societies – like ancient Greece, where homosexuality was not only rife but respected – make it abundantly plain that cultural standards and social learning are by far the best explanation for many of these activities. But under quite different social circumstances, where widespread cultural practices do not serve to obscure completely possible genetic predispositions, the behaviour of that minority which behaves contrary to social ethics and laws, and which is exclusively fixated upon members of its own sex, deserves some attention to establish why the normal cultural forces are inoperative.

Not that there is now any shortage of theories to account for the many abnormal developmental circumstances observed. It has been argued, for example, that chronic unavailability of members of the opposite sex may lead to an episode of homosexuality which may then become fixated as a result of conditioning through the positive reinforcement of sexual pleasure and relief: while guilt, anxiety, habit, and a continuing homosexual social milieu, may then make a successful counter-conditioning or rehabilitation very difficult to achieve. Case histories and observations have suggested many other alternative influences which may affect children otherwise not predisposed: these include mothers who for one reason or another overwhelm their boy children, making normal masculine identifi-

* *Eugenics Soc. Bull.* (1979) **11**, 9–13.

cations and adjustments difficult and sexual relations with females impossible because of generalizations carried over from the all too attractive, but taboo, mother. Abnormal developmental circumstances may well be salient in many cases, but particular themes are by no means as universal as the theories themselves tend to suggest.

Interestingly enough, though, a whole range of psychopathology-proper is much more common in the families of homosexuals and may mark either a source of abnormal developmental circumstances for the homosexual himself, or suggest the presence in the family of rather non-specific genetic abnormalities – or, more likely in the majority of cases, it might suggest some combination or permutation of genetic and environmental factors. We are, of course, now talking about typical situations rather than particular cases but, given the significantly elevated MZ to DZ concordance ratios which have been reported in research studies, it seems reasonable that a genetic contribution to the equation should not be underestimated.

Homosexuality is probably one of the best cautionary examples of the dangers inherent in conducting, or even considering, any piece of psychogenetic research which arbitrarily assembles categories of people – like 'criminals' on the basis of a legality criterion. Psychological categories must needs be based upon a much greater homogeneity of behaviour than is evidenced in research which groups people together simply because their very different behaviours have the common characteristic of being against one or other of the plethora of laws governing such matters as traffic behaviour, sexual activities or business transactions, and the more universally condemned (and biologically counter-productive) categories of unjustifiable and unnecessary killing, or causing serious injury, within the group.

Nevertheless, though caution is clearly advisable, indiscriminate hostility towards any assertion that there may be hereditary influences involved in serious anti-social behaviour is not. The empirical evidence available needs winnowing but it still contains a sufficient amount of good twin data which is not heavily contaminated by the criteria used to assemble the cases to make explanations in terms of social learning alone very difficult to sustain.

As studies of 'criminality' invoke so many semantic and categorical problems, a more specifically psychological grouping often proves more tractable – which in relation to anti-social behaviour, generally implies research into 'psychopathic personality' – a categorization which, though often applying to convicted criminals, is by no means a necessary condition of lawbreaking.

In a review of recent research findings, J. C. DeFries and Robert

Plomin* have summarized two separate studies, one of which – by B. Hutchings and S. A. Mednick† – involved the offspring of convicted criminals, the other – by F. Schlusinger‡ – the offspring of psychopaths. Both studies involved children who had been given up for adoption. In the former instance, some of the adoptive fathers were also criminals: a circumstance which had only a trifling effect upon the control group of adoptees deriving from non-criminal fathers, but which sharply increased the already much higher proportion of offenders in the biological offspring of criminal fathers.

Schlusinger's study of psychopaths also involved comparison with a matched control group and, here again, the results showed a markedly higher incidence of psychopathy in the children of diagnosed psychopaths than among the non-psychopathic adoptees. It would seem, therefore, that a case has been made for the inheritance of characteristics associated with anti-social behaviour, though exactly what these characteristics might be is still a matter for speculation.

In the last chapter, when discussing the inheritance of schizophrenia, we noted that the children of schizophrenics – whether brought up within their own families or by adoptive parents – showed a much higher degree of psychological disturbance than could be explained on any chance basis. Among the more prominent of these non-schizophrenic syndromes is that of psychopathic personality – typified by an indifference to the feelings of others coupled with the sort of defective impulse-control which allows the psychopath to be utterly ruthless and single-minded in the pursuit of any goals which attract him. In consequence, the relationships of the demanding and egocentric psychopath tend to range from poor to downright disastrous – which is not only a social and psychological misfortune, but is also likely to adversely affect working relationships and job opportunities.

Of course, such a combination of factors makes it more likely that psychopaths will also come to be classed as criminals when they quite rationally (in their terms) set out to satisfy their needs through rape, theft, deceit, violence, or whatever. So it seems that positive results often classed under the genetics of 'criminality' may well tap a good proportion of what might otherwise appear under the psychiatric rubric.

In fact, though, the identification of psychopaths is still too often a loosely conducted process of naming on the basis of persistent, and

* *Ann. Rev. Psychol.* (1978) **29**, 473–515.
† *In Fieve et al. Op. cit. Ch. 7.*
‡ *Int. J. Ment. Health.* (1972) **1**, 190–206.

unrepentant, anti-social behaviour. When handled in this way, the classification has very little more to commend it than has, say, 'recidivism' – which is also descriptive of superficialities, constituting no more than a sort of rudimentary botanizing without the benefit of a theoretical or explanatory substratum. Fortunately, this is by no means universally so, and the exploratory and classificatory efforts of Hans Eysenck are a good example of the kind of work which is concerned with refining categories themselves and supplying a theoretical coherence to the observations.

We have already, in chapter five, briefly discussed factor-analytic studies of the sort which have resulted in Eysenck's derivation of three main dimensions of personality – 'extraversion', 'psychoticism' and 'neuroticism' – each of which may be measured on a continuum to define uniquely any individual, whether 'normal' or markedly mentally disturbed. One would not wish to become involved in outlining details of his work here – Eysenck himself has done this with great economy at one point or another in almost everyone of the many books he has written – but his treatment of psychopathic personality and neuroses is so apposite to our immediate concerns that some further mention cannot easily be avoided.

In the case of psychopaths, scrutiny of personality measures, developmental circumstances, and family mental health records reveals the presence of two apparently different sub-groups – referred to as 'primary' and 'secondary' cases – which Eysenck urges should be considered separately. *Primary psychopaths* tend to emerge from the families of schizophrenics and are typified by high 'P' scores on Eysenck's psychoticism scales. Scores are typically well above those for the normal population, though lower than those found in groups of diagnosed psychotics. However, primary psychopaths are likely to evidence many typically psychotic tendencies – such as a gross splitting of intellectual and emotional responsivity – and tend to develop from a basically schizoid form of integration, which is presumed to be due to sharing some of the abnormal genetic materials prevalent in the family.

Thus, primary psychopaths are seen as inheriting something of their asocial and anti-social characteristics from psychotic family members, but *secondary psychopaths* are held to develop in a quite different way. In their case, there tends to be no family history of psychosis and they themselves do not evidence a high score on psychoticism scales. Instead, the genesis of their social disorder shares a common type of origin with that of clinically impaired neurotics, who also tend towards abnormally elevated scores on the

dimensions of introversion-extraversion and neuroticism as measured on the various Eysenck personality scales.*

These two dimensions, being independent of one another, are usually expressed in the form of Cartesian co-ordinates (see p. 173) with most people clustering around the point of intersect. However, those who score highly on either introversion or extraversion, if otherwise in the low range on neuroticism, are likely to remain as adjusted introverts or extraverts: just as high scorers on the neuroticism scale may yet remain socially well-compensated so long as they enjoy favourable low-stress environments. So, though there is room for manoeuvre when establishing the probabilities of breakdown versus adjustment, the theory allows a number of statistical predictions to be generated, and therefore tested, and these depend upon our understanding of the characteristics of the physiological processes which are believed to underlie the dimensions involved.

It may also be remembered from chapter five that one's degree of introversion-extraversion has been held to be very much dependent upon the properties of the central nervous system, and particularly those structures of the lower brain (the reticular formation) responsible for augmenting or supressing the level of incoming stimulation. Augmentation of the excitatory potential is typical of the reticular activity of introverts whereas inhibition, or 'damping' of input, is the typical action of an extravert's central nervous system. Behaviourally, there is a marked difference between such people in their preference for levels of sensory stimulation which, for the reasons already discussed, is thought to reflect differences in the operation of their reticular formation. If this is so, the external stimulus-seeking which so clearly distinguishes extraverts from introverts may prove to be largely physio-genetically determined – even though one's tastes and behaviour will undoubtedly be much affected by habituation or other forms of learning.

The main significance of these findings for the development of psychopathology lies, however, in the ways which the reticular 'booster' affects learning. Experimental studies in a variety of situations indicate that the effect of unusually high augmentation – as in marked introverts – leads to a more rapid form of conditioning, in which inhibition (akin to fatigue) occurs relatively slowly. By contrast, the typical extravert experiences a suppressor effect and his rate of learning is slower: the build-up of inhibition is greater and its dissipation occurs more slowly.

* For example, the MPI, EPI, PEN, or EPQ personality scales.

In consequence, it is argued, the expected behavioural outcome is that the extreme introvert will learn, and indeed over-learn, social rules with great facility whereas the extreme extravert will be very much more difficult to inculcate with (among other things) social conventions and rules.

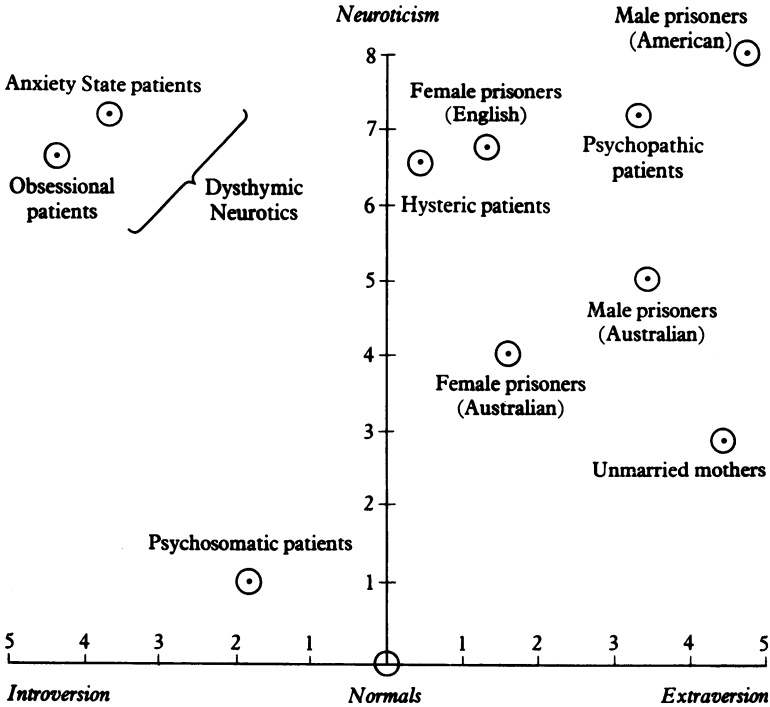
The neuroticism factor depends for its variable strength not so much upon modifying structures within the central nervous system, as upon the autonomic nervous system and the various endocrine and other bio-functions controlled by it. Once again, a considerable degree of heritability has been claimed for it on the basis of studies conducted in a variety of circumstances ranging from animal breeding experiments to the kind which we shall be considering in a moment – the inheritance of marked, clinical, levels of neuroticism.

The processes governed by the autonomic nervous system are, very broadly, those concerned with readiness for action, particularly the preparation for fight and flight. The sympathetic division of the system acts upon the heart, lungs, muscle-tonus, sweating, pupils and so on, so as to activate resources – whilst the other autonomic division, the parasympathetic, acts to restore functioning to the quietude of a vegetative level. It follows therefore that the neurotic is someone who is more than usually aroused for most of the time and who can much more readily than is normal be provoked into a state of fast heart-beat, tremor, sweating, and the psychological counterpart of this physiological tension – by actual, assumed or anticipated environmental events.

So there is the possibility of a vast range of interaction alternatives, depending upon the combinations and permutations of the characteristic strength of the two dimensions of extraversion and neuroticism – the outcome of which may lead to psychopathic behaviour or the more classical manifestations of the various neurotic reaction types. In the figure opposite, data derived from actual studies shows how these various outcomes are typical of different combinations.

The plot on p. 173 gives only a very general impression of findings which have been substantiated in many ways and by many research workers. However, more precise information is readily available if one cares to refer to the published norms for the various instruments developed by Eysenck and his wife Sybil. The collected data quite clearly and consistently shows a connection between the relative scores on extraversion and neuroticism and the type of symptoms formed. These aside, there is also a vast literature reporting predictions which have been made (and generally verified) as to the relationship between

certain criteria of behaviour and the anticipated strength of the measured introversion-neuroticism factors.



From *Crime and Personality* (2nd ed.) by H. J. Eysenck (1970).

In fact, on a number of occasions I have myself conducted studies of behaviourally interesting groups such as venereal disease patients, the sexually promiscuous of both hetero-* and homosexual† types, and those who misuse drugs‡ to manipulate their states of consciousness. The result of such studies has been to leave me in no doubt about the importance of differing interactions and the directional strengths of introversion-extraversion and neuroticism in determining type of adjustment disorder.

We have already, in chapter five, referred to twin studies which dealt with the heritability of introversion and neuroticism in psychologically normal people and so, taking all the evidence together,

* *Brit. J. Soc. Clin. Psychol.* (1969) **8**, 246–52.
† *Brit. J. Vener. Dis.* (1972) **48**, 75–8.
‡ *Brit. J. Addict.* (1976) **71**, 33–41.

it begins to look as though these apparently rather simple dimensions may provide an excellent starting-point for our understanding of a whole range of psychological adjustment and maladjustment. Moreover, as we have seen, and will see, the nature of these psychological constructs, and the correlates of variation in their measured strengths, are very helpful in suggesting which might be the actual physiological mechanisms modified by hereditary differences.

In consequence, there emerges a rationale which, on the best empirical grounds yet available, amounts to one of the most complete formulations of how hereditary factors may operate in psychological adjustment. But, whether or not this particular formulation manages to survive in its present form, some such account will continue to be necessary so long as the data point to the heritability of neurotic disorder.

Conceptualizing 'neurosis' is not as easy as it may seem, for not only is the literature on the matter voluminous but also it reveals all too clearly how far the various definitions proposed are a function of particular theoretical points of view. At a superficial level one can, of course, pick out many of the psychological reactions which are typical of neurosis – for example anxiety, tension, sleeplessness, compulsions, phobias, feelings of depression or futility, and so on. In fact, so many of the manifestations of neurosis are mere exaggerations of normal feelings, and are so commonly experienced in their more full-blown aspect at some stage or another in our lives, as to make theories which emphasize the essential continuity of neurosis and normality very attractive.

Unfortunately, however, the decision to categorize one person as 'neurotic' and another as 'normal' must always be one of judgement and opinion in the grey areas between those obviously distressed people who seek and need psychological help, and those whose lives are presently well adjusted and satisfying. Of course, this intermediate area is very large indeed and so a greater degree of imprecision in diagnosis must be accepted: there are no clear-cut criteria for determining neurosis which are comparable with those used in medicine to help diagnose fractured bones, tuberculosis, chicken-pox or whatever, so a more or less subjectively based decision is usual. One consequence of this is that concordance studies of the neuroses are particularly susceptible to the effects of mis-diagnosis – a factor which is likely to cause underestimates whenever the diagnoses to be compared derive from different sources.

Moreover, basal prevalence figures are very difficult to establish because only a minority of the population ever passes through the

hands of psychiatrists, and so the number of cases overlooked or simply treated for, say, insomnia or lethargy by a general practitioner is difficult to estimate. But given that the morbidity estimates will differ from area to area depending upon the quality of local care, particular research interests, and the criteria adopted to compile statistics, the final figure is universally high.

Even if we take a restrictive view and consider only that minority which has been deemed classifiable as clinically neurotic, surveys indicate we are probably dealing with a proportion of the population which may be as high as twenty per cent in the case of females, and ten per cent in males – depending upon where the research is conducted.

The relatively greater number of women becoming neurotic seems to be a general finding, though one which some theorists feel is mainly due to the particular difficulties experienced by women in trying to establish a satisfactory degree of individual autonomy and self-expression. Others may place a greater emphasis upon the very delicately balanced biochemical events which take place in a woman's body throughout each month and as a result of pregnancy – and indeed throughout her lifespan generally. Most, however, would tend towards some composite theory, stressing the interaction of socio-psychological and biochemical factors – an account which would be perfectly compatible with some sort of diathesis-stress formulation of the kind previously discussed. Of course an acceptable theory – though having to account for male-female difference in prevalence – will be expected to do so in terms of relative, not absolute, differences in the causal circumstances. After all, men too are the subject of hormonal-biochemical variation and psychological stress.

The mechanics of the diathesis part of the equation – the actual physiological structures involved – have already been mentioned, but the potency of *stress factors* in causing neurotic-type behaviours is such that it is still regarded as not only a necessary, but as a sufficient, cause by a surprisingly large number of psychopathologists other than those of a Freudian or other 'analytic' persuasion. For example, many behaviourists would argue that much, and perhaps all, of the 'neurotic' behaviour which is treated as an 'illness' by their more middle-of-the-road medical colleagues may be far more convincingly accounted for in terms of conditioning alone.

Inevitably, theories based upon conditioning evoke associations and comparisons with animal studies, and the literature contains an abundance of this type of research illustrating how anxiety-provoking conflicts can lead to behavioural disorganization or '*experimental neurosis*' in creatures as diverse as mice and monkeys. Yet the best

example of how neurotic behaviour may be conditioned in human beings was also one of the earliest – the case of 'Little Albert'.

Albert was a child of approaching one year of age: a youngster for whom the white rat was a pleasing companion, not at all a source of anxiety or threat. And so it might have continued had it not been his misfortune to become an experimental subject for J. B. Watson,* the 'father' (or, one might be more inclined to say nowadays, the 'godfather') of psychological behaviourism. Be that as it may, the experiment was of a remarkable and even elegant simplicity. Every time the child reached out for the pet rat, Watson (who was standing out of sight) would bang an iron bar with a hammer and so make a very frightening noise.

Just as Pavlov had conditioned his dogs to anticipate food by associating the sound of a bell with its presentation, so Watson conditioned a fear response in Albert by associating the rat with a frightening experience. The outcome was a child whose conditioned anxiety response applied not only to rats but, by a process of generalization, to a range of other harmless furry objects also. In other words, Watson demonstrated that something very like a neurotic phobia could be created in a perfectly normal child through the application of simple conditioning techniques.

Certainly Watson and his followers have managed to demonstrate that *some* psychological disorders of a type usually classed as 'neurotic' can be artificially created by means of classical conditioning techniques, and that one really has no need to evoke mysteriously operating unconscious conflicts. Subsequent studies have not only confirmed these opinions but have also expanded the range of abnormalities which may be created through these and other types of conditioning – while 'behaviour therapists' have shown how the identical processes of learning which lead to conditioned anxiety reactions may be used to reverse the process by more powerfully reinforcing other, incompatible associations.

What, however, behaviourists like Watson failed to do was explore individual differences in conditionability. They did not take sufficient cognizance of the possibility that people may have genetically different sensitivity to conditioning and that a fully stated account of how certain types of neurotic breakdown occur should also have something to say about which individuals are most susceptible to, say, developing a conditioned anxiety reaction, all else being equal.

Of course, applying the 'Little Albert' type of experiment to large

* *J. Exp. Psychol.* (1920) **3**, 1–14.

groups of twins and non-twins at different ages and stages of development is as impracticable as it is humanistically undesirable, yet it would be of considerable interest, and perhaps utility, to have more information as to the part played by genetic factors in such cases. But although we are never likely to obtain data in just this form, we have already seen – for example, in relation to Eysenck's study of that correlate of conditionability, extraversion – how probable it is that influential genetic predispositions exist even in cases where environmental circumstances seem fully to account for the behavioural outcomes.

Genetic influences on factors like conditionability, though they may at first sight seem very distant from the problems of mental health, may yet hold the balance between normality and disabling psychiatric disorder far more often than we suppose. This is something to bear in mind in relation not only to the behaviourist paradigm, but to all other theories – including the many forms of 'depth' psychology – which also lay considerable emphasis upon learning in its many forms.

But we return again to the question of the prevalence of neurosis and the problems involved in determining family concordance rates. We have already noted how very widespread are those manifestations of mental disturbance which are serious enough to merit treatment, and how seemingly similar symptoms which disable one person may be tolerated by another. The effect of this is, of course, to make wider family studies exceptionally difficult, not only because of the detailed clinical examination which would be required of all members in order to determine when criterion levels of neurotic behaviour had been reached, but also because of the great difficulty in persuading diagnosticians to adopt strictly comparable methods and criteria.

At least these appear to be major stumbling blocks in this area of research but, whatever other reasons may also account for the relative paucity of familial studies in neurosis, one can generally not expect much of wider scope than concordance rates for first degree relatives – parents, siblings and children. In these cases, though substantial concordances are quite usual, the results tend to be more equivocal than one would wish because the shared cultural, economic and social environment, as well as the mutual influences one upon another, are maximized. Then too, there are always doubts as to what, on the criteria of a particular study, would constitute a normal population morbidity. Without this piece of information it is always difficult to know how to judge particular results.

In addition to these difficulties, Rosenthal has pointed out that

other practical circumstances often mean that the experimental controls involved in family studies tend to be of a very dubious nature. For example, the investigator may be particularly susceptible to the effects of suggestion when conducting a study in which he knows the diagnosis of the proband – not at all an unusual circumstance and one which may be of critical significance where investigators are sorely taxing their personal ability to discriminate among the delicate shades of grey.

Some studies raise even more disturbing spectres of experimental contamination when one learns that the diagnostic categorization of family members may be based upon questionnaire material or upon descriptions and classifications provided by the proband himself. All in all, there are evidently considerable hazards here for anyone taking published concordance rates at face value and without reference to the way in which such studies have been made.

If this seems overly pessimistic, it must surely be a fault in the right direction though, hopefully, not one which will cause too much despondency. After all, the most powerful tool in the psychogeneticist's bag – the comparison of MZ twins with same-sex DZ pairs – still remains, and although this strategy is not free of all the problems we have referred to, nevertheless the necessary experimental controls are very well understood – and are increasingly more carefully handled. So, as is often the case, we must rely on twin studies to provide us with the most acceptable, or at least the less exceptionable, data.

But even twin studies are bedevilled by the fact that neurotic reactions are of a very heterogeneous nature. Even allowing for the presence of marked abnormalities in each twin, it is still by no means clear what should be counted a 'concordant' and what a 'discordant' result. The problem is that syndromes as apparently different as conversion hysteria (e.g. hysterical blindness or paralysis) and reactive depression tend often to be thought of as merely different ways in which neurosis can present itself. Freudians, for example, make assumptions that all of the many reaction types are more or less equivalent in that they are merely the surface aspects of a *deeper* process of intrapsychic conflict – and one, moreover, due to conflicts of a rather limited range.

By way of contrast, many behaviouristically inclined workers would regard the range of neurotic reactions as comprehensible only in terms of associational learning and the reinforcing conditions which applied in given cases. Behaviourists are not likely to feel the need, or even see the sense, of invoking any sort of shared 'core' pathological processes.

Consequently, investigators whose sympathies are somewhat akin to the former position will be more likely to claim concordances for different types of neurotic reaction than those more sympathetic to the latter.

In the event, a variety of policies prevail – studies which determine concordances on the basis of there being *any type* of neurotic reaction in the relatives of index cases perhaps overestimate the heritability of neurosis, but those which only allow positive concordances on the basis of similar-to-identical reaction type may well underestimate them. In an excellent summary by Gary Miner,* a good deal of the more recent work on the subject has been brought together in an attempt to establish a consensus which, it may be said, finally owes much of its substance to the work of Eliot Slater and James Shields. These last two researchers have, over a good many years now, been pursuing lines of enquiry ranging from the broadest familial-type studies of the relatives of neurotic military personnel through to tightly controlled classical twin studies in which concordances were calculated on several alternative criteria.

As in the case of Eysenck, Slater's own wartime work with neurotic soldiers has been exceedingly influential in shaping contemporary ways of thinking about the conditions necessary for the development of neurotic symptoms. But whereas Eysenck has continued to remain more concerned with the psycho-physiological processes which underlie the development of the phenotype, Slater has placed more emphasis upon the genetic aspects *per se*. Consequently, over the last thirty years or so, the complementary approach of these two has done much to promote a shift from the polarized behaviouristic and analytic ideas of earlier times. In their different ways, both have contributed substantially to the development of a more modern point of view – one in which the effects of several significant elements, both physiological and psychological, are acknowledged in the development of both neurosis and personality disorders.

But to return to the empirical evidence; and first that relating to the heritability of *undifferentiated* reaction types. Slater and Shields themselves tend to produce concordance results which are somewhat lower than those presented by most other investigators, being of the order of 15 per cent for the DZ twins of neurotic probands and 40 per cent for MZ co-twins – still a highly significant difference. However, when they combined their own results with those of other workers, an average for six studies suggested the higher concordance rates of 23 per

* *Arch. Gen. Psychiat.* (1973) **29**, 111–18.

cent for DZ and 52 per cent for MZ pairs respectively – an estimate which is currently widely regarded by people in the field as being quite acceptable.

In a study subsequent to those just referred to, Slater and Shields followed up a number of people who had several years earlier been treated in hospital, with a view to establishing concordance rates for their co-twin. Such a procedure has an advantage over studies in which the concordances are finally determined at the time when a proband case is identified in that it allows much greater latitude for the emergence of illness in the twin. In consequence, there is substantially less likelihood of underestimating the real degree of association. But, to add more than usually stringent conditions to the study, it was arranged that 'blind' diagnoses, without prior knowledge of the other twin's condition, should be adopted.

Of the 84 same-sex DZ pairs and 62 MZ pairs included, the diagnoses for those twins who had earlier been hospitalized ranged over the various types of neurotic reaction and even included cases of personality disorder. It transpired, however, that genetic features were quite clear-cut in relation to personality disorder, as indeed they were in relation to the single neurotic type of anxiety reaction, the crucial concordance figures for MZ and DZ twins being, in the case of personality disorder, 33 per cent and 6 per cent respectively and, in the case of anxiety states, 41 per cent and 4 per cent. Needless to say, the magnitude of these differences was such as to yield substantial heritability ratios, though this was not so for the remaining cases.

The residue, composed mainly of patients of the reactive depression variety, evidenced modest partial positive concordances only when the very much less stringent criterion of admitting co-twins on the basis of their falling into *any* of the other diagnostic categories was applied. However, the concordance rates did not differ between DZ and MZ pairs and so it was concluded that, though there may be a moderate degree of genetic involvement, they differ from anxiety state and personality disorder in that they are very much more positively determined by non-genetic causes.

Studies like these, as brought together in Slater and Cowie's excellent book on the inheritance of mental disorders generally, have done a good deal to illustrate the specificity of genetic transmission in relation to the various psychiatric syndromes. Perhaps, therefore, we could do worse than to follow their lead, and their findings, in considering the remaining categories of neurosis separately.

We have already mentioned *reactive depression* in relation to Slater and Shields' study – in which such patients were highly represented in

the category of 'other' neurotic types. In the event, the rather neutral results which emerged from this somewhat heterogeneous group are even more indistinct when the depressive patients are treated completely separately. In fact, the conclusion to be drawn from available research on both twin and first-degree relatives seems to be that this particular neurotic reaction is quite lacking in genetic specificity.

Perhaps the same conclusion needs to be stated, though perhaps slightly more cautiously, in the case of *hysteria* in both of its forms – *conversion* and *dissociative*. These two could hardly seem more dissimilar to a layman – the former mainly concerning somatic symptoms such as hysterically based (and therefore restorable) dysfunctions such as blindness or paralysis, whereas the latter generally involves things like discontinuities of memory which may, in its most acute form, present as double or even multiple personalities in the one person.

And this really is the weakness of the hysterical syndrome; its coverage is so extraordinarily comprehensive that it has become an unwieldy and imprecise catch-all. Furthermore, it all too often transpires that the diffuse manifestations of mental and physical disorder which lead to a diagnosis of hysteria later turn out to have signalled the onset of brain damage or even psychosis. Of course, many more cases do seem to be psychological stress-responses, but it is thought by many to be a classification of very dubious status insofar as it is used to define a unified syndrome.

Given the conceptual and diagnostic problems attaching to this particular traditional category of neurosis, it is hardly surprising to find that there is no good evidence for the working of any specific genetic process. Attempts have been made to improve things by re-defining and limiting the concept but, up to the present time, none of the many research strategies tried has yet presented a convincing case for significant genetic involvement in the development of hysterical reactions – at least not in a sense in which the term is meaningful to psychiatrists.

By contrast, research on *obsessive-compulsive* states – another portmanteau category which combines distress due either to obsessional ideas or else felt compulsions to perform certain acts (or both) – indicates a marked genetic component. Even in this case, though, it has proved difficult to establish a widely acceptable quantitative value for heritability as the literature contains many studies which are unsophisticated by modern standards. Nevertheless, the over-all trend would seem to be strongly suggestive of some form of polygenic mode of inheritance for this particular type of disorder.

Summing up the issue of whether neurosis is a heritable condition is, as we can see, a particularly difficult (if not impossible) task so long as one continues to think in terms of a homogeneous process which simply has numerous ways of manifesting itself. It is not only the concordance studies we have been discussing which strongly suggest the heterogeneous nature of the various conditions we traditionally lump together in the super-class 'neuroses'; causal studies too – like the work of F. N. Pitts and J. N. McClure*, summarized in Miner's review, suggest that specific causes and pathways are operative in certain cases only.

The Pitts and McClure work concerns the relationship which has been established between the neurotic anxiety syndrome and an excessive production of lactic acid. It was found, for example, that increases in the blood levels of lactic acid in anxiety patients, resulting either from physical effort or a direct infusion of the chemical, correlated positively with their symptoms of anxiety. Normal controls proved much less affected by infusions of lactic acid, though not entirely unsusceptible to its effects. Needless to say, the possible confounding factor of the observed anxiety resulting from the infusion itself was controlled for by also injecting both normal and neurotic patients with inert substances. In neither of these control cases was the level of anxiety affected.

It would seem, therefore, that neurotic anxiety may be connected with abnormalities in the metabolism of lactic acid and that, because of the concordances observed, one might reasonably think in terms of failure in a genetically controlled biochemical sequence. Other research has presented evidence for metabolic failures in other pathways though, from our own present perspective, it hardly matters very much which of the alternative biochemical transformations is operative: the important thing is that we have made a start in unravelling the physiological processes by which genotypes may operate in attaining their phenotypical expression.

Studies like these are making it plain that the genesis of neurotic reactions may in some (if not all) cases depend upon more than experiential factors, and that the unit of study may more profitably be *limited types* of neurosis rather than neurosis *per se*. In other words, much of the existing psychiatric taxonomy may need to be reviewed as a result of psychogenetic and associated biochemical studies and, perhaps, a number of the older conceptualizations and categorizations may need to be jettisoned.

* *New Eng. J. Med.* (1967) **277**, 1329–36.

Interestingly enough, the fact that causal theories of neurosis have differed as widely as they have is perhaps due in large part to an initial emphasis by researchers upon different examples and aspects of this large group of disorders. It is, after all, very typical of most of us that we construct theories on the basis of our initial studies and then find it all too easy to stretch the facts to coincide with our preconceptions, and all too difficult radically to modify systems of thinking once we have met with a gratifying degree of confirmation.

Freud's initial experiences were largely with hysterics, whereas those of Watson and the behaviourists were oriented towards conditioned phobias and, as we have seen, either type of reaction would be unlikely to suggest specifically genetic factors – even if they were sought. Pavlov's work on 'experimental neurosis' in dogs, though superficially analagous to anxiety neurosis in human beings, was treated as a straightforward effect of conditioning in stressful circumstances. In this case, and in addition to any theoretical presuppositions which might have been involved, the experimental procedures were so powerful that more or less uniform results could be created in all animals, thus suggesting that genetic variation was of minimal importance.

But, as a result of such exhaustive experimental studies as those of J. P. Scott and J. L. Fuller, we now know a good deal about the genetics of individual and breed differences in the temperament of dogs. And though, no doubt, these differences can be overlaid as a result of intensive conditioning procedures, they are, nevertheless, very significant under normal rearing conditions.

The same is true of our more typical laboratory animals and most research workers are now likely to consider the genetically determined characteristics of, say, their mice or rats before starting on any piece of behavioural research. The differences involved are so great that, generally speaking, animals are selected and results presented strictly in terms of specified and known genetic strains. This trend has been developing over a considerable time now, but it has always depended upon such people as Peter Broadhurst*. By constantly alerting their colleagues to the genetic basis of, for example, 'emotionality', 'exploratory behaviour', and so on, and through demonstrating by means of selective breeding experiments in rats just how far psychological characteristics may be transmitted, they have not only improved the quality of a wide range of experimental work but have also contributed substantially to psychogenetics. And, though there is

* *Annals New York Acad. Sci.* (1969) **159**, 806–24.

an enormous amount of difference between rodents and men, and even between the manifestations of what we choose to refer to as 'anxiety' or 'emotionality' in either case, such selective breeding experiments do help in that they demonstrate the feasibility of genetic coding for different degrees of emotional control. Together, they add to the *web* of evidence upon which all well-formed scientific theories must rest. The web which is presently being woven must necessarily involve people of very different backgrounds and interests, and herein lies the difficulty. It seems likely that the widely different views which are sometimes expressed on the nature of neurosis are not so much due to the presence of incompatible data as to over-generalizations being made on the basis of any one set.

Only very recently have psychogenetic studies evolved a sufficient substance, control and rationale to affect the thinking of those whose personal experiences or working hypotheses have led them in a different direction. Fortunately though, the present phase of psychiatric and psychological development is very much more favourable to eclectic thinking and we have begun to enter a period in which the older doctrinaire rigidities are crumbling. Even such bastions of conservatism as psychoanalysis and behaviourism are succumbing to the tendency to see that explanations, and causes, are generally more satisfyingly dealt with by considering interactions at many different levels of analysis.

As we have seen, the evidence for a genetic predisposition towards personality disorder and neurosis presents a very uneven picture, some syndromes exhibiting a quite marked degree of heritability, whereas others do not. However, despite exceptions, the over-all impression is that genetic factors are very much more pronounced than might generally have been expected. With regard to the neuroses, anxiety has stood out as a very good example of this, and one which we know to have an important place in the content of many other behavioural maladjustments and the theories by which we understand them. For example, excessive levels of anxiety, arising in response to intrapsychic conflict, is the very cornerstone of many 'dynamic' theories of neurosis, just as it is fundamental to many other theories which emphasize the swamping effects of anxiety provoked by the stresses of achieving an acceptable social adjustment in difficult or unpredictable circumstances.

Elevated levels of anxiety are typically found in alcoholics and delinquents, are prominent in neurotic types such as the obsessive-compulsive, and seldom absent in any of the others. And, in the case of people with such other adjustment problems as I have myself

researched over the years – for example, the sexually promiscuous, homosexuals, and drug abusers at all stages of involvement – its manifestations are usually quite markedly apparent. It seems that all manner of personal and inter-personal maladjustments tend to be found with heightened levels of anxiety – though which is the prior condition, if such may be said to occur, is the difficult problem we are left to answer.

This problem of what constitutes cause and what effect is the ultimately crucial question with regard to the development of all functional behavioural disorders; and, because of its very widespread association with syndromes which have been held to have a partial genetic basis, the case of abnormally high anxiety is a particularly central one. Is a chronically high level of anxiety one of the basic triggers for a wide range of disorders or maladjustment, or is it simply a *consequence* of being disordered or maladjusted?

I am inclined to think that the weight of evidence, particularly that relating to twin comparisons and animal selective breeding experiments, strongly favours the conclusion that inherited factors do play a part *in some cases*. But this does not necessarily exclude the possibility that neurotic anxiety states (or any other type of disorder involving chronically elevated anxiety feelings) may, in other cases, be entirely due to environmental circumstances operating upon a genotypically perfectly normal person. After all, both the physical and the psychological manifestations of anxiety are, in themselves, completely normal and appropriate anticipatory reactions towards what are perceived as possible sources of threat.

For these responses we are heavily indebted to the autonomic nervous system which works in association with certain endocrine organs and parts of the central nervous system to comprise a functional unit controlling the excitability and relative balance of the sympathetic and parasympathetic branches of the autonomic nervous system. Extra sympathetic activity keys us up; antagonistic parasympathetic activity acts as a damper. Together, their reciprocal relationship is of the utmost importance in regulating a whole host of psycho-physiological states, and it seems quite reasonable to assume that, like any other physical system, they are subject to genetically determined individual differences in their efficiency, and therefore in their relative balance.

If this is so, some people will be much more readily (and powerfully) responsive to stressful situations; some will be 'triggered' with unusual ease, whereas others may require considerable stimulation to upset their equilibrium; the majority of us will be somewhere

in between. So, it is not the unusualness of reactions that is an issue in determining what are 'normal' and what 'abnormal' anxiety reactions – rather, it is the strength and duration of their occurrence in relation to a given context.

A soldier in the trenches may have a very similar inchoate feeling of dread to the anxiety-type neurotic, as well as the same physiological effects – palpitations, 'butterflies', cold sweat, etc: such differences as there are depend, from a categorizing or diagnostic point of view, only upon the *reality* of a present danger. If the person concerned is in comfortable circumstances, with no reasonable explanation of what could cause him chronic fear, then we presume that the response is inappropriate and therefore pathological in some sense.

Dynamically orientated psychologists will seek explanations for this abnormal response in terms of fears which are just as 'real' as in the case of the soldier – though in this instance due to unconscious conflicts which may quite as realistically threaten to devastate the individual's very self. The behaviouristically inclined may try to account for the same phenomena in terms of equally inferential constructs, though this time using a very different technical vocabulary to deal with the individual's prior conditioning and the effects of its generalization. Then there is a final group of psychologists or psychiatrists who may be more inclined to emphasize a genetically, as opposed to a simply congenitally or psychologically, determined physiological hypersensitivity towards the quite ordinary pressures and circumstances of living.

Of course, there is no reason why each or all of these explanations might not be right in a given case. Unless the genetic or any other single type of evidence was a good deal stronger than it is, it would be rash to suppose anything different. As it is, we generally come back to some sort of diathesis-stress formulation as offering the best working hypothesis. The diathesis element is, as we have discussed, most commonly held to depend mainly upon variability occurring in processes controlled by the autonomic nervous system, whilst the stressor circumstances may result from any form of conflict – past or present, conscious or unconscious – which makes social adjustment or self-acceptance difficult. Stated in this way, the stressor circumstances envisaged are, of course, perfectly compatible with theories as diverse as psychoanalysis and behaviourism.

Being capable of including the findings of so many different types of biological and psychological research, the diathesis-stress model should prove useful in fostering mutual regard and, hopefully, even co-operative efforts from workers who might otherwise tend towards

the sort of polarization of views which is likely to occur when people feel threatened by ideological competition. Where the model is weak is that it allows for any outcome – that is, for interpretations based upon any combination or permutation of hereditary and environmental factors. Nevertheless, a new conceptual start has been made: it is, like the many alternative formulations, far from perfect but will be improved as people work out ways to develop, and then test, a range of more specific causal hypotheses generated from it.

I personally find it surprising that, given the research findings which now exist, there are still many who jib at the idea of a genetic component in the genesis of adjustment problems other than the most gross disorders. One might reasonably reject unpalatable models or theories on the basis of more closely reasoned alternative interpretations, but simply to dismiss them without then being able to integrate all the known evidence is surely very bad science indeed. One can always find faults, real and imagined, with opposing theories, yet the evidence relating to the various types of neuroses and personality disorders is such as to leave ample room for an equal working of quite different explanatory principles – though not necessarily contributing equally in each clinical type or individual case.

The evidence supporting all currently developed models in psychopathology is, to say the least, inconclusive and fragmentary. Furthermore, it is by no means logically necessary that the same principles are invariably applicable to all occurrences: effects may have many different causes. Anyone who doubts this might care to contemplate the wrecked cars towed into a breaker's yard and imagine the vast range of possible mechanical, environmental and behavioural events which may have led up to each crash and its aftermath. In psychiatric terms, the causal possibilities are likely to be very much more diverse, and this is rightly reflected by the many different types of explanation being offered – though, alas, they are all too often overgeneralized.

Some cases of psychological breakdown or malfunction would seem to be most comprehensively, and convincingly, explained in terms of behavioural conditioning; others through intrapsychic conflict of a largely unconscious type; whilst still others may seem to be sufficiently accounted for in terms of a psychological war of attrition – fought against insurmountable environmental odds. However, where only *some* of the total number of individuals who share similar circumstances of conflict, threat, deprivation or whatever actually respond with serious psychological maladjustment, the evidence that we have considered makes it doubly reasonable to continue looking for the cause

of these differences in constitutional variables. Research findings have made it clear that genetic involvement is not likely to be prominent in all, or even most, cases but they do suggest that this approach may contribute significantly towards the unravelling of many problems posed by mental dysfunction and disorder – perhaps even suggesting means by which they may be ameliorated.

But though the prospect of developing pharmacological remedies to ease genetically exacerbated disorders is not likely to disturb many people, we shall see in the next chapter that there are other possible forms of medical and social intervention which might be less welcome.

Chapter nine

THE POLITICS OF PSYCHOGENETICS

It is usually possible to discern at least two socially important consequences of discoveries made in new areas of knowledge. In the first place, ways are almost invariably found to apply them and, secondly, they change perceptions and belief systems relating to their subject matter. So when, as in the case of psychogenetics, the theme is mankind itself, the ensuing discussion should be of interest to all of us as we all have a stake in its outcome.

This concern with outcomes has, in fact, done much to shape this entire book: a more conventional treatment would have placed greater emphasis on technical issues and given more detailed information about the studies. But, important as a textbook approach undoubtedly is, it seemed to me that a more generalized account might also have its merits. After all, the wider context of psychogenetic research concerns our most basic understanding of our species and of human nature itself. In the end, one cannot divorce questions about the heritability of particular psychological processes from more general issues concerning the limits of self-determination and social modification, and even our place in the natural order.

Current theoretical implications raised for psychology and the other human sciences are, though very different in kind, assuredly no less challenging than the potential technological applications which face society. But, as we must begin somewhere, perhaps we should start by considering the potential social uses and abuses of our growing knowledge before turning to the scientific and philosophical implications.

Of all recent instances in psychogenetic research, one which has most directly raised controversy about public policies has been the identification of the 'XYY Syndrome' based upon correlations between the possession of an additional Y-chromosome, the enhancement of such attributes as height, physical aggressiveness, and unruly

sexual impulses, and the tendency toward criminal or sociopathic behaviour. Together with certain work in the sphere of ethnic difference in intellectual capacity, such research exemplifies better than almost any other instance we could have chosen the ways in which genetic research may lead to programmes of *action* which raise not only technical misgivings but ethical ones also.

The XYY, or 'super-male' Syndrome, as it is often ironically termed, refers to the effects of a fault which occurred in the process of sperm-cell formation in the proband's father. Instead of contributing only the normal single sex chromosome – either a male Y or a female X – two Y-chromosomes contrived to be packaged in a single sperm. The resulting genotype, though very rare, has proved to be very much more prevalent in the population of penal institutions and institutions for the criminally insane than its rarity suggests it should be if it was unconnected with sociopathic behaviour.

On the basis of this finding, there has been developed an explanatory theory to the effect that the additional Y-chromosome *causes* both the physical and the psychological outcomes. Evidence for the association, if not the causal nexus itself has, with some exceptions, continued to amass ever since the pioneering work of P. A. Jacobs and her colleagues at Carstairs*, the Scottish penal establishment for the mentally ill, drew attention to the phenomenon in the mid-sixties.

However, of late there have been a number of attempts to re-evaluate such findings and to examine alternative explanations of the associational data. As might be expected, the range of conclusions arrived at, even by similarly qualified workers, covers a very wide spectrum of opinion. For example, Johannes Nielsen's own work† in a forensic psychiatry clinic, together with his interpretation of other people's results, leads him to conclude that there is good evidence for accepting a causal relationship between the presence of an additional Y-chromosome and the immaturity and sort of defective impulse control which is likely to result in antisocial behaviour.

Moreover, he found himself in accord with other researchers in that *enlargement* of the single Y-chromosome in the otherwise constitutionally normal also seems to be associated with particular tendencies in personality deviation. It transpired that, amongst criminal patients, the relatively more enlarged Y-chromosomes were associated with individuals who exhibit a greater propensity for violence. Space does not permit us to go further, either into Nielsen's

* *Nature.* (1965) **208**, 1351–2.
† *Brit. J. Psychiat.* (1971) **119**, 503–12.

own studies or into his evaluation of the existing evidence, though this may be done easily enough by consulting his writings. Sufficient simply to note that he represents one pole of informed opinion in that he seems fully to acknowledge chromosomal abnormality as a predisposing cause in criminal behaviour.

In an excellent review article, Stanley Walzer and his colleagues* have recently analyzed current causal hypotheses in the light of the results from many studies conducted all over the world. The conclusions reached are guarded and the need for further research is stressed, but the *association* between the XYY genotype and presence in a penal institution appears to be a matter of fact which is not currently explicable in relation to the methods of study which have been applied in the original research, or by a completely non-genetic theory.

However, most opinion is somewhat more cautious than that of Nielsen for, whilst acknowledging the association, many workers are reluctant to endorse an interpretation which has such wide implications until they have a plausible rationale as to the causal mechanics. Inevitably, any mind-body nexus is likely to prove fairly intractable for all the reasons known to scientists and philosophers alike, but it is especially difficult in this instance as the behavioural criteria themselves are so tenuous. The upshot is that many people have simply suspended judgement and await further developments.

But, at the opposite pole from Nielsen, scientists like Jon Beckwith and Jonathan King claim that the so-called 'XYY Syndrome' is 'a dangerous myth'† based on inconclusive studies which actually constitutes a threat to the individual and to society itself. One should really read their paper carefully in relation to experimental reviews like Walzer's before coming to any personal conclusions, but I find it a very mixed offering. The thrust of their argument is concerned with making a case for social action (stopping this type of research) rather than with maintaining a disinterested scientific perspective.

One would not quarrel with their right to influence opinion, but they make their appeal in terms of minimal reference to the data which troubles the rest of us. Such research as they do mention has been selected because it raises some possibility of methodological contamination through, say, screening only very tall patients and thus perhaps artificially inflating the prevalence rates if the extra Y-chromosome is connected with tallness rather than criminality *per se*.

* *Ann. Rev. Med.* (1978) **29**, 563–70.

† *New Scientist*. (1974) **64**, 474–6.

But, it should be said, these possibilities are now well understood by most research workers and synthesizers of research and they are not, in any case, true of very well known studies like Nielsen's.

Furthermore, Beckwith and King, in their eagerness to neutralize the XYY research, resort to some very unconvincing alternative explanations of the stubbornly resistant associations. But what they do very well indeed is express the unease with which liberal-minded people view the proliferation of research findings which seem to threaten the philosophy of egalitarianism and lead to an invasion of our personal privacy. The case on which they specifically based their assault is a study conducted at a maternity hospital in Boston, Mass., where the research staff had been screening for XYY cases with a view to producing not only further information about prevalence rates in normal populations, but were also organizing counselling for parents in order to minimize the assumed risk to their genetically abnormal children.

The aim of identifying children at risk as soon as possible, and of alerting their parents to the need for thoughtful child-rearing practices would seem, at first sight, not only unexceptionable but downright admirable. However, according to Beckwith and King, there is a catch, or rather several. First, the XYY Syndrome stigmatizes the child: regardless of his actual personal characteristics, he is likely to be categorized as a potential social deviant. Even if it is only the parents who are party to the diagnosis and its present implications, this can hardly allow for a healthy, unself-conscious and spontaneous family atmosphere. In fact, it may become a self-fulfilling prophecy which, apart from the damage caused to the child itself, would invalidate any research which assumed that behavioural outcomes could be directly related to chromosomal abnormality.

Second, parents themselves, if given the choice, might well be unwilling to have thrust upon them what must necessarily be an upsetting view of their children. So that, in cases like the Boston maternity hospital, had the parents been made fully aware of the implications of the (supposedly) routine screening to which they consented, they might very well have refused to participate. In other words, it is argued, the unsuspecting parents were being exploited by scientists and doctors in the pursuit of their own interests.

However, it is the third and most sinister objection which has the more far-reaching implications – that such children may well become the vulnerable targets of society. If the potentially dangerous and anti-social nature of XYY individuals is acknowledged, this may open the flood-gates to ill-informed and malicious policies of eugenic control

or unreasonable sentencing practices when crime is also involved.

Beckwith and King refer to ominous developments of this sort taking place already and are rightly, in my own view, concerned by the possibility that public opinion and policy decisions may be radically influenced by data that may not only be incomplete but plain misleading. That there is an above-expectation representation of XYY cases in mental, and criminal mental, institutions may well be so – but they may also prove to be relatively more abundant in creative, sporting, and other socially worthwhile activities too. Until these possibilities have been properly explored it is too soon to be forging what may seem to be an exclusive association with deviancy. Furthermore, even if the elevated risk of deviancy stands, and is the only discernible association, it should not be supposed that all XYY cases are deviants: they certainly are not. So, to think in eugenic or special custodial terms where an XYY constitution has been determined constitutes a totally unwarranted threat to the individual – who would thus have come to be seen as a symbol rather than as a person.

In a telling comparison, the authors point out that membership of a group in which social deviancy is more than usually common is no justification for singling-out each individual for special control: if it was, then every black baby in America should be so treated as *its* statistical risk of ending up in prison is greater than that of a white child. One might even expand on this, and say that any child which has an XY complement (all normal boys) should be the subject of special psychological and medico-legal attention as they are of a group which contributes most of the killing, maiming and crime in society!

Beckwith and King's solution to the problems and confusions which have arisen as a result of research into chromosomal abnormality is to ban it, and to spend the money so saved on the social and economic structure which leads to, say, the relative preponderance of black American crime. But, whilst I sympathize with many of their arguments, and the need to tackle deviancy at its roots in society, I completely share E. O. Wilson's* grave misgivings about suppressing research which may throw light on other causes too. The Beckwith and King humanistic position is admirable up to the point where they denounce other 'ideologically influenced' lines of research and then propose ideological alternatives which, in their blinkered fashion and taste for censorship in knowledge, strike me as more dangerous than the things they are attacking.

* *J. General Educ.* (1978) **29**, 277–87.

It still remains to be seen whether the XYY Syndrome is a major causal factor in some people's social adjustment. If it is, then no amount of unctuous and self-righteous talk will take the place of a practical assessment of the situation and the development of treatment methods based upon a proper analysis of the causal mechanisms. At least this is true so long as genetically determined conditions can be modified or treated.

On the whole, it is true to say that we are currently able to do very little with abnormal chromosomal material as such, but there is every reason to believe that biochemical interventions in the processes they control may be very effective. As with the case of intellectual retardation due to phenylketonuria (PKU), the body's chemistry, and thus the effects of genetic activities, can be very favourably influenced even by such simple methods as rigorous attention to diet.

To suppose that genetic defect can only be treated by genetic means (if this is ever a possibility) is absolute nonsense: there is no reason why biochemical and psychological treatments should not be very valuable in treating genetic disorders; genes are not destiny, they are only like our bodies – sometimes good, sometimes bad, but something we have to learn to get along with anyway.

Now this is the point at which a discussion of *Eugenics* cannot be avoided. In fact, for quite a number of people, psychogenetics is still mainly construed as the theoretical substratum of those social and eugenic policies which are always waiting in the wings while the audience of public opinion is warmed-up by ingenuous or elitist scientists. And, if this seems a somewhat alarmist or one-sided interpretation of work in this area, it is not to be wondered at after our still quite recent experience of Adolf Hitler's horrendous attempts at breeding a German 'super race' capable of ruling the world.

The tactics used are hardly likely to be forgotten: genocide of groups like Jews, gypsies, and Slavs; homicide with mentally defective and certain socially deviant types; kidnapping of promising stock from other peoples; and the institution of *Lebensborn* – the 'patriotic' action of young women, married or single, giving themselves as breeding vehicles to other similarly 'pure-blooded patriots' dedicated to producing new members of a 'master race'.

The violence and degradation accumulating from these various attempts to create a genetic élite beggar description – but then so too does the quasi-science upon which it was based. What we actually witnessed in those days was not the implementation of science but the rationalization of naked prejudice and a morally repellent ideology. Ignorance and malevolence are the prior conditions of such behaviour,

its rationalization might equally have been political, economic, religious, or anything else. Nevertheless, mud sticks and it is still hardly possible to discuss genetics in so far as it touches upon social policy without reference to its perversion by the Nazis.

Joshua Lederberg,* a Nobel laureate in genetics, has devoted a good deal of attention to both the practical and the ethical aspects of genetic control and has done more than most in his attempt to restore the issue to one of social and scientific realities, rather than a constant re-hash of Naziizm and such literary spectres as *1984* and *Brave New World*. As he points out, effective genetic control of the individual is not possible until a totalitarian state already exists.

Taking an example from the (real) possibility of behavioural and thought-control by means of radio-controlled electrical stimulation, he makes the point, presumably true for the rest of us, 'I do not accept the implantation of the electrodes except at the point of a gun: the gun is the problem'. Clearly, ethical as well as political issues are involved.

In his closely reasoned paper on the ethical aspects of genetic control, Joseph Fletcher† has argued that such judgements are finally based upon moral systems and axioms of either an *a priori* or a *pragmatic* type. The former is predominantly of the religious variety and assumes *absolute values* which have been expressed in scripture, revelation, or dogma – the general principles always being more important than the individual case. So, for example, one could, in advance of knowing the human circumstances, always unequivocally decide to sacrifice a mother-to-be if it should come to a medical choice between saving her life or that of the unborn child. Nearer to our own present preoccupations, it could be, and frequently is, laid down on *a priori* grounds that *any* embryological or genetic experiment with human materials is always 'wrong', 'sinful', 'inhuman', or whatever – just as was the dissection of a human body a few centuries earlier. Dogmatists have no problems in foreseeing consequences; they can act according to a predetermined plan or logic.

By contrast, the pragmatic approach to ethical judgements tends to assume that no act can be wrong in itself, but depends upon the *consequences*. Of course, the problem then turns upon how far it is feasible to envisage all possible outcomes of scientific or technological innovation. Our recent experience is that it is all too easy to misjudge new situations badly, yet refusal to play in any risk-gain system seems to me to be an even more dangerous strategy in a world already at very

* *The Pharos of Alpha Omega Alpha.* (1971) **34**, 9–12.

† *New Eng. J. Med.* (1971) **285**, 776–83.

great risk due to vast population pressures and the associated tendency, which emerges regularly every few years, for us to get involved in the mass destruction of one another.

Alas, as it seems that prophetic vision is not given to mere mortals, we must judge situations as rationally as we can – whilst always monitoring outcomes and being ready to adapt to whatever corrective measures may seem appropriate. But upon what ethical principles are we to act, if not those of religion or other sources of unchallengeable *a priori* law?

Pragmatic ethics stress outcomes – but outcomes for whom? It goes without saying that there may well be conflict between what is good for society and what the individual considers good for himself. But there may also be a conflict between the short-term and long-term advantages of a particular policy and, perhaps, even far-reaching implications for the well-being of other peoples unconnected with the decision-taking.

These difficulties are not easy to resolve and one can say little more than that knowledge coupled with goodwill, and a genuine concern for the welfare of all our fellow human beings, is about all that can be hoped for: it may not guarantee success, but then neither does anything else. In the end, we must make our choice: either we can judge the potential applications of psychogenetics in terms of a religious doctrine or an unchallengeable political ideology, or else we must apply our own reason and wisdom to each new situation as it arises.

Assuming that we have not decided in advance that *all* developments in genetics which affect human psychological adjustment are unacceptable and therefore to be discouraged, we must now consider the sort of ethical problems which are beginning to arise. So far, our treatment of the subject has been far more concerned with theoretical understanding than with technological possibilities. But, in fact, these two are interdependent in the sense that a more accurate appraisal of genetically influenced mental defect, abnormality and excellence is also a pre-condition (should society will it) for the development of eugenic controls – both negative and positive.

In the latter category are some of the most spectacular and daunting possibilities – of which the most remarkable must surely be the potential uses of 'cloning' with human beings. The principles involved are natural, in the sense of commonly applying to certain insects and other lowly animals and to plants. Simply stated, it is a form of reproduction based on the development of tissues from a single parent. Amoebas are adepts, but a more familiar example may

be budding or taking cuttings in horticulture, where new plants may be created without resort to the usual form of sexual reproduction in which the genes carried by two donor parents are combined to yield a unique individual.

Cloning, which is still thought to be a technological possibility rather than an actuality in human beings, would involve the culturing of any number of somatic cells taken from the body of a person whom one wished to replicate and, using whatever other embryonic tissues might be necessary, developing them either in the metaphorical 'test tube' or else as implants in a woman's womb.

One might, for example, wish to reproduce large (or small) quantities of a scientific or artistic genius – or else, perhaps, great leaders, prize-fighters, or what ever else seemed desirable. If society decided what it wanted in its citizens – intelligence, beauty, courage, mental health, or whatever – it would, in principle, be possible to produce infants according to specification by the use of cloning. That is, so long as the premise that genotype strongly influences the phenotype is correct. It may be that the cloned physical replica of a genius could equally well turn out to be dull or undesirable in other ways but on a theoretical basis at least, the results of psychogenetics currently indicate that one's chances of creating psychologically similar individuals is far greater by a process like cloning than would be the case with ordinary reproduction.

This conclusion follows from the fact that cloned individuals are as genetically comparable to the donor as are MZ twins to each other. So, if much of the research which we have been considering so far is correct, and we accept that MZ twins are much more likely to resemble one another in certain psychologically important ways than they will their parents, other siblings, or their own children, it should be *possible* to breed for psychological as well as physical characteristics.

You may feel that we are now entering the world of fantasy, of science fiction, or of alarming titillation: to some extent this may be so, but it is the world of sober reality, too. In a recent best-selling book, David Rorvik presented a detailed account of what purported to be the first case of human cloning: the principal character was a multi-millionaire with a taste for immortality and a sufficient surplus of money to be able to pay the vast sums necessary to achieve the co-operation (and secrecy) of scientists, doctors, a woman to gestate her modified egg, and Rorvik himself – a specialist science correspondent who was the organizer and go-between. Given these ingredients of megalomania, intrigue and high finance, the book could hardly fail – not only to sell in amazing numbers, but also to stir up the entire

scientific world. Was it, as Rorvik claimed, a true account or was it, as many scientists feel, no more than a work of imagination?

The answer hardly matters: Rorvik's compilations of the technical background and the scientific literature, together with the public debate which resulted from the revelations, are fascinating enough in themselves. It goes without saying that the sensation created has done nothing to add lustre to psychogenetic or plain genetic research, but it has at least driven home the fact that the cloning of human beings must now be regarded as a feasible avenue of social and biological evolution. Moreover, the moral issues raised demand answers to questions of whether, and in what circumstances, cloning should be considered. Setting aside the notion of mass-producing 'desirable types' for a moment there are also some very important questions concerning the potential of these new developments for helping the infertile.

Already the key technologies are evolving rapidly, even though their present purpose has nothing to do with asexual reproduction. For example, the fusion of egg and sperm cells in a laboratory, followed by a re-implantation of the fertilized egg in the donor mother, is currently being pioneered for some cases of infertility. In other cases, where gamete cells are absent or defective in one or other of the pair, current technology cannot help, so one wonders what rational or humane objections there could be to cloning, where this was possible, through culturing and implanting somatic cells taken from the husband or wife. In principle, they could then have children of their own who are as genetically like themselves as they would have been to an identical twin, had they had one. In such cases the basis for a moral objection is difficult to imagine, unless one takes the *a priori* view that any human intervention relating to pregnancy or birth control is unacceptable.

No doubt some criticism of these developments will derive from hazy ideas that they are somehow 'against nature' – but then so is cooked food, democracy, flying, farming, contraception, an injection of penicillin, and just about everything else we do or use to maintain such high levels of health, comfort and culture for our perilously inflated numbers. Arguments about things being 'natural' or 'unnatural' are no longer very binding on our species: indeed a return to primitive nature would immediately result in the death by starvation and disease of the greater part of mankind.

Yet we are perhaps left with some sense of disquiet all the same, though why reproduction using cloning should be as repellent to people as it seems to be is not immediately obvious: after all, children born in this way would be no different from other children. Even supposing that many cells from the same donor were cloned, the

resulting progeny would not be any more standardized than are identical twins, quads, sextuplets, etc. – and, in the same way, they too would develop as unique, if more than usually similar, individuals.

One final, and even more extraordinary, outcome of cloning is possible: we might use it to achieve some sort of personal immortality by having our own genetic constitution repeated generation after generation. In principle, we could ensure that our genetic duplicate – down to our very fingerprints – would always succeed us. Husbands and wives could, for example, both be replicated in some of their sons and daughters whilst, if they wished, having other children by normal means. What heaven-challenging hubris! But, nevertheless, it would be a perfectly *rational* thing to clone our children and to perpetuate ourselves for, in so doing, we might feel more able to steer them in ways which our own experience suggests might be most advantageous. Not, perhaps, a probable outcome, but assuredly a tempting one.

Cloning may also seem a miraculous solution to other of mankind's oldest preoccupations and aspirations. For example, given the realization that psychological characteristics are indeed heritable in some marked degree and that cloning is a perfectly feasible procedure, the lure to use our new knowledge and technolgies to produce a planned society might just prove too tempting to those who had most to gain. Huxley's *Brave New World* realized in a modern setting the old dream of Plato and others of a perfect Republic in which individuals would not only be tailored for their role in society (as we are at present!), but also *bred for them* – so that youngsters would, from the very beginning of their lives, be shaped for use at their psychological and physically most appropriate levels as rulers, workers, soldiers, or whatever. There would, of course, be almost no practical upper limit to the number of times a given desirable type could be reproduced as the number of somatic cells which could quite harmlessly be removed from any of the various parts of the donor runs into billions. The limitations lie in the number of maternal hosts, the physiological and surgical man-power and, most of all, the co-operation of all concerned.

As we noted earlier, Lederberg was of the opinion that such genetic manipulation was not a serious threat because it would require the prior existence of a very efficient tyrannical regime in order to implement it – thus it would either be redundant or else more effectively achieved by other means. And, had Nazi Germany never existed, I might have agreed with this rational viewpoint. As it is, though, we have a recent example of armed force and genetic control being used simultaneously and I think it would be overly optimistic to believe that this could not happen again. What the developments,

inducements or sanctions might be which would encourage this type of conformist acquiescence is difficult to imagine, but some degree of vigilance with regard to this kind of genetic intervention is surely sensible.

In all probability the cloning of human beings for other than quite exceptional reasons will never take place, but the principles of *positive eugenics* – selective breeding for desirable characteristics – may yet find other, less dramatic, outlets in this overcrowded technological world of ours. But, in the short term at least, it is the possibility of *negative eugenic* measures which figures more prominently as a foreseeable ethical and political issue.

Alas, negative eugenic procedures are actually very far from the passive image suggested by the term. Instead, they concern the removal of 'undesirable' genes from the gene pool and the interception of births which might introduce inexpedient elements. Removal from the gene pool has many meanings – including sterilization and elimination – most of which have been practiced by states in our own century. And though some of these cases were simply manifestations of barbarousness, others have been motivated by an apparently humane concern for those directly affected, and for the lives of those whom they may in turn affect. Conventionally, the severely mentally retarded or deranged are the subject of these policies but, no doubt, prominent in the thinking of anyone who was currently compiling a 'list', would be our old friends the XYY cases.

Although judicial homicide may not seem likely, the monitoring of pregnancies for chromosomal abnormalities in the developing foetus is now commonplace, as are the terminations which are usually advised and performed on the basis of observed aberrations. Generally, these refer to conditions like mongolism or serious congenital conditions affecting the nervous system – such as spina-bifida and anencephaly – which may be detected during chromosomal screening. However, many such pregnancies are *not* terminated because of the parents' moral views – thus raising the very touchy issue, central to our own deliberations, of whether people have an absolute *right* to produce children which are likely to become a drain on society.

The more obvious chromosomal or congenital features aside, some families do produce much more than their share of mentally retarded or otherwise mentally unstable individuals, and it is a rational (if chilling) question to ask whether limits should not be set to the number of their progeny, and whether their choice of sexual partner should be subject to legal control. Some people would answer that having children with whomsoever they choose is the inviolable right of the

individual; others that no such right exists when the parents demand that others should bear the consequences in terms of financing and otherwise attending to the problems they create.

But, even in cases where defect is not an issue, the present threats posed by population pressures, coupled with our present forms of social and economic interdependence, mean that we can no longer take it as self-evident that people may do as they please and exercise their 'natural rights' in having children. Natural rights, like any others, depend upon one's fulfilling certain responsibilities too – if this is practicably possible. Few of us nowadays can fail to see the logic of this mainly economic argument: the question is, however, are we equally open to the political and personal implications of its application?

Society could, in principle, and sometimes does in practice, intervene in the reproductive rate of its members. Up to the present, though, this has been generally limited to such moderate measures as the slant given to family planning advice or propaganda, to manipulating a family's income by means of taxation or income policies, or by providing housing or other financial incentives by way of encouragement. Typically such strategems are not explicit in their intention, and perhaps not even consciously thought of by the people formulating them as being about differential rates of breeding, but the effect is just the same.

We could all think of examples from other parts of the world where more definite policies of population control are being attempted, and where the justifications for applying them selectively are the quite reasonable ones of poverty and general economic difficulties. The very real survival pressures which are now occurring in these places, and may soon be much more general, require a rationale which – in the absence of a disinterested scientific analysis – may be provided by ignorance and prejudice dressed-up as 'common sense'. Economics will provide one such justification, but we may be sure that quasi-psychogenetic ideas, however crudely framed, will be another, so there is a very real sense in which it is politically desirable that good research is available to moderate arguments which may ultimately bear upon people's freedoms.

A particular section of society which is frequently singled out for special attention is the intellectually below average, who are mainly to be found in unskilled occupations. For, despite the frequent precariousness of their means of employment, and their poorer standards of living, they do tend to produce an above average number of children. Economic and social pressures aside, this higher rate of reproduction by the intellectually less gifted is feared to pose a threat

to our national intelligence by inflating the relative preponderance of genes from a 'poorer stock'. The argument is an old and ubiquitous one but it is not currently being taken too seriously in scientific circles because such long-term empirical studies as are available have failed to confirm the prediction of a declining national level of intelligence.

On the other hand, it has been argued that the comparison made – intelligence test results for successive groups of schoolchildren made over lengthy periods of time – is inconclusive because any genetically determined effect which might be operative is quite likely to be masked by any of a number of intervening variables. It is argued, for example, that there is an increasing test sophistication among schoolchildren, who are being progressively exposed to ever more test material and that practice itself may well be sustaining scores which would otherwise show a drop.

Another line of argument is that the baseline may be sustained only as a result of more effective intellectual stimulation deriving from environmental and educational sources, whilst the basic genetic potential decreases generation by generation. Arguments such as these are, of course, insubstantial in the sense that they simply reflect opinions and theory-saving hypotheses: they are not grounded in critical empirical research and therefore deserve to treated with suspicion when affirmed rather than offered as a basis for enquiry. However, there is some more solid evidence which may explain why empirical studies reveal no decline in the general level of intelligence – but its rationale depends upon positive genetic workings, not upon the masking of genetic effects. The phenomenon which may account for the failure of the gloomy forecasts is known as the principle of *regression to the mean.*

Regression may be observed in many cases of polygenic inheritance, and an example such as height is helpful because it is not an emotionally charged issue, and because the units of measurement are unequivocal and the results readily observed. Now, tall parents tend to have tall children, but their *average* height is less than that of their parents. That is, progeny manifest a shift, or regression, towards the population mean. Similarly, short parents tend to have short children but, again, their average falls somewhere between that of their parents and that of the mean. In other words, a rather inexplicable convergence seems to occur in biologically determined characteristics including, as it turns out, intelligence. This type of regression has been observed in relation to many different attributes and is a process which should result in a population stabilization of the characteristics, just because of the familial fluctuations.

One cannot absolutely rule out the possibility of serious directional drifts occurring in the gene pool, but the lack of clear empirical support for this view suggests that research rather than rhetoric is called for. Not that the issue lacks a basis for much thought-provoking discussion: indeed many investigators have commented that, taking the long-term view, this particular issue may yet prove to be one of the most important facing civilized man.

Even the shorter-term perspective raises a number of points which are of considerable interest for, despite the effects of regression towards the mean, studies have repeatedly shown that the children of very intelligent parents themselves tend to be well above average, just as the children of very dull parents will tend to be well below the average. This is, of course, a *statistical phenomenon* and not a fact which may be extrapolated to any given individual. But, aside from any practical implications which eugenic enthusiasts might see in such findings, they are vitally important observations in unravelling the secrets of psychological inheritance – though their meanings and implications are a good deal less self-evident than the relatively clear-cut findings might at first suggest. Moreover, protagonists of both predominantly genetic and environmental persuasions tend to find their interpretations a two-edged sword.

For example, regression to the mean is a very mixed blessing for those who would underplay psychogenetic arguments: on the one hand, it is welcomed because it largely defuses alarming arguments that national intelligence may decline as a result of the higher reproductive ratio of the less intelligent. But, if one accepts this line of evidence and its interpretations, it also requires acceptance of what may be the even more unpalatable proposition that intelligence is modulated by genetic processes.

An emphatically environmentalist point of view – that intelligence is very little, if at all, determined by genetic factors – *should* imply that children from homes in which the parents are of low intelligence, and where cultural resources are meagre, would show the effects of their underprivileged environments by sliding even further down the intellectual scale. Similarly, it might be expected that the children of intelligent parents who had been brought up in culturally enriching and intellectually stimulating conditions would evidence IQ levels of at least the same order as their parents. However, this is not so: instead there are shifts towards the mean in both cases, a finding which runs contrary to any view of intellectual growth which is based predominantly upon environmental factors.

This is, by the way, a finding which also runs contrary to the élitist

view of inheritance, that there is a sort of natural order such that some family lines are inherently superior to others. Nature, it seems, see-saws about a point of equilibrium, propelled upwards or downwards by genetic flux, while each individual child's potential is maximized or minimized by the forces of circumstance.

At the present time, most research into the heritability of intelligence still relates to theoretical and pedagogical concerns, despite the propaganda uses to which it is sometimes put. But prevailing social attitudes ensure that there is still very little impetus to apply eugenic controls in cases other than those where the most gross forms of retardation are involved. By contrast, such an impetus is beginning to develop in connection with mental disorders of the type discussed in chapters seven and eight – though it currently excites little attention as it involves advice rather than control, and because the psychological cases are embedded in a wider concern with physical pathologies.

Nevertheless, genetic counselling in family planning and obstetrics is now common practice, and empirical research is presently the basis for guidance on the whole range of decisions from whether a couple would be well-advised to marry, through to whether pregnancies would best be terminated. And, as our knowledge of risk factors deepens, and extends to an ever-increasing range of conditions, such eugenic considerations may be expected to play an equally extensive part at both personal and social levels of decision making.

For the present, though, we still tend to cling to the idea that the individual has an unquestionable right to marry whom he pleases, even if the case-histories of the two families involved strongly suggest a considerable degree of risk for their children: apparently the unborn do not have the same right to expect the exercise of prudence by others as have the rest of us. Yet the alternative to absolute reproductive freedom is surely not as outlandish as we may sometimes think, nor is the limitation of sexual choice without contemporary precedence in most countries – though we may not always recognize the purpose served by the restrictions imposed. Religious and taboo prohibitions of marriage between close relatives are so universal and so time-honoured as to make them seem unconnected with such modern concerns as genetic risk. Yet, though the underlying reasons for these tribal or theological rules may be obscure, and will certainly be connected with other things too, the increased dangers of inbreeding have been widely understood for much longer than have the genetic theories which now explain them.

After all, there can be no other practical reason why a mother and

son, or a brother and sister, may not marry: any repellent feelings this idea may give rise to are of a social and cultural origin only. But where recessive mutations have taken place, as in the case of haemophilia, inbreeding would prove disastrous to the family or clan – and all animal breeders who use inbreeding to select more rapidly and precisely for desirable characteristics in their stock will be equally familiar with the great advantages and the great hazards of this strategy in respect of both physical and behavioural characteristics.

Ancient customs in every known culture regulate who, among relatives, may marry or have sexual intercourse with whom. We are so used to these regulations, perhaps having associated them with religious truths, that they have become powerful taboos almost beyond questioning. And indeed, if we did critically examine them, modern science would certainly endorse the sound judgement found in many of these archaic codes of behaviour. Some proscribed unions, particularly those relating to in-laws, may not survive the light of strictly biological reasoning, but many even of these may still serve a socially useful purpose by subtly regulating behaviour in family groups.

Modern genetic and psychogenetic rationales about who should, and who should not, mate do not enjoy the same venerable justifications which cloak those of a cultural or apparently religious origin. For a contemporary figure to advocate limitations upon individual choice, however scientifically argued, is to court considerable hostility, since it runs absolutely contrary to the current belief that all individuals have an absolute right to mate as they please – excepting only the limitations imposed by the unchallengeable taboos! But all branches of human genetics, not excluding psychogenetics, are now revealing the mechanisms by which undesirable outcomes may occur in given matings. This is additional information which is not catered for in the ancient codes, but which nevertheless illuminates both similar and newly recognized hazards to the unborn – thus raising moral questions which may well transcend assertions of personal rights.

Haemophilia is a case where *advice* about marital choice may be freely given, though there is no actual restriction upon high-risk couples opting to have children, despite the very serious hazards which are often involved for the unborn. But one could imagine that if social controls ever do come to be implemented, they are most likely to begin with such severe physical conditions. And, if this should happen, the implications would be daunting for, if we are to take Lederberg's

figure that somewhere between a quarter and a half of all common diseases are of genetic origin, genetic counselling and reproductive regulation could, quite reasonably, come to play an ever more important part in our social system – perhaps even comparable with the almost unbelievable legal ramifications regulating the use of motor vehicles!

The simile may be strained, and the picture presented of widespread genetic control may be far too unfamiliar for it to carry any conviction but, by the same token, I am sure that people living in the time of horse-drawn traffic would have been astonished at the incredible system of controls which was so rapidly brought about by a combination of the introduction of the internal combustion engine and the state bureaucratization of governments. Our freedoms depend upon anticipating the possible course of future developments: even the most fundamental changes seldom occur overnight, generally they begin quietly enough and just spread.

Moral questions have been raised by our new understanding of the mechanisms of inheritance and they will ultimately require political decisions, whether these result in action or inaction. For my own part, I can quite see the great practical advantages of applying much of our newly emerging knowledge, and can also quite understand how tempting it may be for policy-makers to save us from ourselves. Yet, even in the case of severe physical conditions, I feel that naked control is not the answer for, individual and practical considerations aside, so much more than the marginal social benefits attained could easily be lost if one extended what might look like a good precedent into the area of psychological adjustment.

Our knowledge of the heritability of psychological factors is simply too tenuous to justify any prescriptive solutions and, in any case, the sort of negative eugenic controls which we have been considering are likely to be counter-productive in other ways – particularly in that they would discourage investigations concerned with genetic-metabolic defect. When these occur, there still always remains the possibility of biochemical intervention in the individual's metabolic function – so long, that is, as we are unflinching about psychological inheritance and prepared to persevere in our attempts to understand the processes involved.

Moreover, there is no reason that I know of to believe that most heritable psychological tendencies cannot be handled in psychological terms: genetic causality in no way precludes other forms of treatment or behavioural modification. Psychiatry gives one example of this, biology another. In the former case, psychotherapy, behaviour

erapy and chemotherapy have all shown how they may counter the ects of disorders such as alcoholism and schizophrenia, and the dy of animals shows how even the most powerfully coded dencies may be affected by training.

As we all know, Elsa the lioness became, as a result of sensitive and m handling, a member of the human tribe – loving and gentle in this ntext – though still physically and potentially one of the most ngerous of all land carnivores. So, if animals with much more ongly developed instincts, and much less capacity for learning, can erride their biological coding, then we need hardly fear that our own plies any necessary predetermination – even though it may well sult in certain general predispositions.

We have already aired at some length the question of how far and in hat ways we may be susceptible to the workings of unconsciously erating biological propensities, so further delving into the subject ust be left to the reader. What is immediately striking though, is the eat diversity of the relevant sources of data, ranging over olutionary thinking and phylogenetic comparisons; studies which pear to indicate the hereditary element in neurological and neuro-emical systems which act upon the central and autonomic nervous stems to affect cognitive processes such as memory and intelligence, temperamental ones like excitability or extraversion; and the oneness of offspring to develop the same psychiatric disorders as eir parents. Clearly, this new source of input into psychological eorizing is of basic significance and, because it permeates such a de spectrum of psychological functioning, is bound to influence our tire conception of psychology – and consequently its subject matter.

Our view of mankind's place in the nature of things is always in ansition, profoundly affected by theories which accompany the pression of our scientific knowledge and conception of the universe. eligious interpretations of man's nature have, when not totally jected, been greatly modified by discoveries such as Copernicus', that e earth is not the centre of the universe; Darwin's, that we are not a ecial creation and separate from nature; Freud's, that our very tionality is illusory; and, most disturbingly of all, Pavlov and kinner's demonstrations that even our unique personalities, con-ctions and values are in large part the product of quite simply nderstood conditioning procedures. Each of these steps forward in ience seems to have led to a more mechanistic view of man, and to an creasing emphasis upon his mechanical aspects at the expense of his eculiarly human qualities. Truth to tell, psychogenetics itself is not kely to add much to our understanding of each other in a personal

sense. In fact, it has often been said that it is likely to achieve t opposite: to do only what so many other psychological theories seem do, deflect our attention away from individual people and towards a stereotypes.

English & English, in their well-known dictionary of psychologi terms, have described a stereotype as 'a relatively rigid and ov simplified or biased perception or conception of an aspect of real especially of persons or social groups'. And one has to admit that mu of what is revealed by psychogenetic research is quite compatible w this definition. Yet, despite the limitations inherent in su approaches, I believe that they may still serve vital purposes so long the conceptions produced are treated as hypotheses, guidelines, a first approximations in an approach to the individual. Wh generalizations are taken, or mistaken, for propositions abo particular individuals, and are propagated *in lieu* of a more round approach to psychological understanding, then they are liable become not only misleading but potentially very pernicious.

In fact, however, psychology, or any other science for that matt could not exist without various levels of generalization which beco ever more specific until one approaches the individual case. We nee level of generality to develop our conceptions about what is speci typical; a level which treats typical sub-group differences – whet predominantly biologically or culturally defined – such as tho relating to nervous typologies or to membership of sexual or soc groups; down through ever more refined familial groupings to tho which help in understanding particular people.

Taken together, these form a background against which t individual may be seen, allowing us to fit our more speci observations into conceptual categories and, reciprocally, evolving t conceptual categories from our observations. Broad generalizatio narrow correlations, and particular observations, are not by the selves to be trusted, but a rationally worked-out combination of the should inspire much more confidence.

Unfortunately, however, a good deal of work yet remains to be do in fashioning psychological theories which are clearly articulated fro the species to the individual level. Genetics has recently added a ne dimension to this very difficult task and it will take some time reconcile the various interfaces of different levels of behaviou analysis – even after the significance of psychogenetic findings h been more fully explored and more generally accepted. Difficu though this task may be, no satisfactorily integrated human science conceivable until this particular nettle has been grasped.

There will inevitably be additions to, and subtractions from, the sort of evidence presented in this book but, whatever revelations may come about as a result of further research, it seems beyond question that a range of psychologically important features depends upon genetic transmission and it only remains to establish *which* psychogenetic theories and findings require assimilation.

Any new perspective on man in general – whether it be religious, philosophical, behaviouristic, political or whatever – always has an unfortunate tendency to transcend and dominate our view of other people. At least, this is likely to be true of those whom we do not know personally. Our enthusiasms all too often cause us to forget the integrity, complexity and dignity of our brother man and, instead, to see him as a stereotyped representative of the class or classes to which we have assigned him. But, theoretical understanding of general processes aside, in so far as we can treat every other individual as a unique person like ourselves, we should not err too seriously. If, however, we should fail in this, the fault must be judged to lie in our own lack of humanity, not in our new sources of knowledge.

Finally, intriguing and perilous as the conceptual implications undoubtedly are, it seems likely that psychogenetics might equally well serve mankind as a powerful investigative and practical tool in tackling the problems of people at psychological risk and disadvantage. After all, genes are not destiny: if we can understand their workings we can modify them, and if we can learn to do that successfully and wisely, we might also be able to see the end of many of our ancient sources of sorrow and inequality.

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