Chapter 25

Psychology as science

H. Nyborg

1. INTRODUCTION

There is considerable confusion about how to define psychology as a science. Some see it as a social, others as a biological science. Some universities place psychology under the humanistic faculty, others under medical, social, or political science. At still other universities it is part of information technology, sociology, anthropology, philosophy, or computer sciences. Opinions also differ about what the proper subject for psychology is. Some define it basically from a philosophical or anthropocentric point of view, others see it as basically a material subject. It is a truism to say that methods must be adapted to the phenomenon under investigation, but as there is no general agreement about the subject for psychology, the methods applied differ widely. Some use correlational techniques, others experimental, some a nomothetic, others an idiographic or clinical approach. Many social scientists, including cultural anthropologists and psychologists, even doubt if there is such a 'thing' as a human nature to be studied. Genes, biology, and brain structure and function mean little to such scientists; they simply constitute the basically neutral substrate on which societal forces forge the social construction of sexual identity, intelligence, personality, human nature in general, or even the reconstruction of society.

The main title of the book—*The Scientific Study of Human Nature*—was chosen with this boggling confusion in mind. So was the book cover. Here, the fifteenth-century Dutch master Pieter Brueghel reflects on an old legend: People who dare to construct a building that reaches into the sky approaching God deserve punishment. What could be more devastating than forcing the builders to speak in different tongues? They would not understand a word of each other, and their construction would amount to nothing. Perhaps this metaphor illustrates quite well what happened to psychology from the very beginning. Not that God punished the builders, of course, but rather that a self-induced punishment was enforced. These early scientists listened too much to armchair philosophers, who with unbelievable ease produced flashing metaphors that, as a rule, never materialized into empirically testable ideas. Their scientific construction, therefore, soon began to stumble and the design that was necessary to support a proper science of human nature disappeared in heavy verbal smoke. As an extra punishment, almost everybody followed rather uncritically the advice of the early philosophers, and used them in a verbal crusade lasting more than two-thousand years. All this developed uncontrolled head-banging on intractable linguistic body–mind problems for those involved. The result was the establishment of a large number of very different pseudo-empirical schools. It was not even possible to find absolution in eclecticism, because the schools were based on fundamentally different and often opposing ideas about human development and nature. Fragments taken from the various schools were bound to lose the internal construct validity of the original movement.

This Babylonian confusion created more heat than light, and the philosophical impact on the study of human nature turned into the greatest intellectual disaster of all time. The fault was not so much that, in the absence of proper empirical tools, the first faltering steps toward a scientific psychology were turned into clever and rational exercises, but rather that few saw any need to confront the clever word games with an external empirical censor to harness them and evoke self-correcting procedures. Most of the followers accepted for centuries that rhetoric was the measure of progress. The few stubborn scientists that constantly resisted the temptation of easy linguistic solutions and worked experimentally, were largely neglected. This tendency can be seen even in contemporary psychology.

However, changes to the philosophically inspired psychological constructions seem now to be induced by the recent progress in brain and molecular sciences. In fact, psychology based on the idea of an abstract psyche begins more and more to look like a house of cards on the brink of collapsing in the slipstream of the very successful empirical brain sciences.

Such a bleak view on the apparently successful psychology is bound to lift an eyebrow here and there, and the indictment is admittedly strong. The following sections strive to justify the position, by trying to find answers to three questions: (1) Precisely when, where and what went wrong with psychology? (2) What is Eysenck's view of psychology as science? and, (3) What is the most likely next step in the development of an appropriate twenty-first-century scientific study of human nature?

Here I must ask fellow contributors to this book for permission to have a free hand. Some undoubtedly disagree in part or *in toto* with my dreary view of psychology as science. Worse, I intend to draw heavily on their more pessimistic conclusions and indications of obvious lacuna in contemporary psychology to illustrate the fundamental fault in psychology in general.

2. WHEN, WHERE, AND WHAT WENT WRONG WITH PSYCHOLOGY AS SCIENCE?

2.1 The fatal decision

Plato, Aristotle, and Democrit are key actors in the following simplified account of the early formation of psychology as science. One interest they had in common was to identify the nature of things. One of Democrit's ideas was that there are eternally moving atoms and a vast void. Things materialize when atoms collect. They take form in the process, and cease to exist when atoms separate again. This materialistic position heralds a much later atom theory and a natural-science view on the world. Actually, Democrit held additional views that are rather unpalatable to natural science, but this need not concern us here.

Plato and Aristotle saw the world quite differently, as can be illustrated by the 'cave' example. Imagine a man standing in front of a fire. The shadow he projects on the cave wall better expresses the general idea of humans and their nature than the individual projecting it. Form is accordingly more important than content, and the abstract is more important than the concrete. This is an early forerunner for the later philosophical–psychological–humanistic view of the world, where abstraction becomes the basic element in an explanation. The metaphysics of Aristotle presumed that everything is predestined, but this and other aspects are disregarded here.

Although they were contemporaries, Plato (427–347 BC) and Aristotle (384–322 BC) never met personally with Democrit (460–370 BC), as far as we know. Democrit was about 70 when Plato's pupil, Aristotle, was a boy of 14, but they probably knew each others' positions quite well, as their time was one of fierce discussion of these matters in several ancient places.

Unfortunately, Democrit and his materialistic view by and large lost the battle for the scientific study of human nature. Gradually, it became generally accepted that concept formation, logic, and rationality were the proper tools for getting on with meaning, abstraction, theory, and philosophy in order to define (human) nature.

2.2 The dire consequences

The early defeat of the Democritean view may, to a large extent, be seen as the basic problem in contemporary psychology. It was all very well that, after a while, confused ideas of an animated nature gave way to a God-given soul that was later renamed psyche (or ego, self, or me), to rid it from religious connotations. It was also taken as progress that psychologists then renamed the psyche cognition or metacognition, and that other scientists invented highly abstract superorganismic concepts like social norms, cultural stereotypes, and even collective consciousness. In reified form these concepts were gradually acknowledged to either represent or exert an indirect causal impact on mind and human nature.

The common theme behind these apparently very different phenomena is that they are based on notions of abstract Platonic qualities, which elevate them to a status above the material brain or world. The defeat of the materialistic view of Democrit thus paved the way for the extensive use of high-level abstractions as a substitute for proper operationalizing and explanation in the scientific process. Inevitably, this easy solution led to excessive verbosity in the form of uncensored fabrication of hypothetical constructs and intervening variables, reification of these variables, the postulation of causal relations among them, and to the various forms of mentalism-based philosophies that characterize most forms of social sciences, including psychology.

This briefly characterizes (some may perhaps say, makes a caricature of) the actors and sets the stage for the creation of the numerous insurmountable philosophical–psychological dilemmas associated with a dualist view. The results of this conceptual dance macabre with human nature as the victim, is illustrated graphically in Figure 25.1.

According to Figure 25.1 we have in principle four different types of analyses at our disposal after the great intellectual disaster: Surface, top-down, bottom-up, and all-bottom approaches. The three in the middle are the children of the body–mind schism and demand hierarchical solutions. Surface analyses are basically of a purely descriptive value, usually at a very high level of abstraction. They may, nevertheless, serve as a useful starting point for the formulation of genuine causal questions. All-bottom analyses circumvent the dualist traps and allow for proper causal analysis.

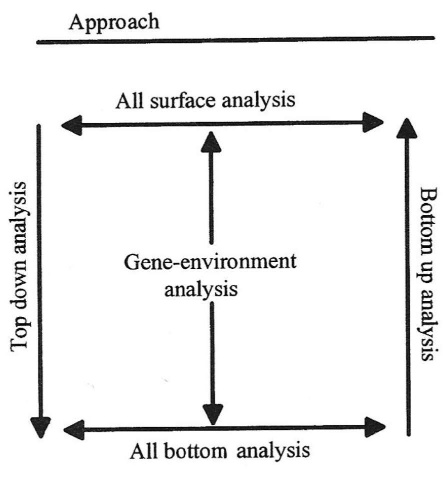

Figure 25.1. Types of analytic approaches to abilities and personality (from Nyborg, 1995).

Examples of surface approaches are behaviorism, information theory, social learning theory, phenomenology, cultural anthropology and cultural relativism, descriptive, lexical, and factorial trait psychology, and classical psychometrics. Some of these traditions strive to escape body–mind interaction traps by sticking to one or a few levels of abstraction close to the top and by concentrating more on the end product than on the processes leading up to it. They pay a high price for keeping biological factors out of the abstract analysis, however. As they remain satisfied with pure description or correlation between surface factors, 'causation' accordingly becomes a possibility rather than a certainty in the empirical sense. This critique applies to behaviorism as well, despite its explicit intent to turn into an objective natural science. Correlation is the major analytic tool here, and a build-up of hypothetic associations the explanatory devices. However, only by opening the "black box" of the organism, and then having to acknowledge that the tremendous individual differences observed in brain structure and function mean something to phenotypic behavior, could behaviorism ever transform into an experimental natural science, but then it would no longer be behaviorism and its characteristic surface analysis would turn into a top-down or bottom-up approach.

Top-down analyses typically take point of departure in some phenotypic surface observation, and then strive to identify the most likely biological candidates for explaining the observation. Many good examples of studies of the biological underpinnings of intelligence and personality are provided in this volume. Bottom-up analyses typically take point of departure in some kind of manipulation of one or more biological parameters and then use phenotypic behavior as a dependent measure. Of course, top-down and bottom-up analyses can be combined or explored serially. They do not necessarily run into serious body–mind problems if they stick to only a few neighboring levels of explanation close to the top or bottom, that is, keep within compatible levels of explanation. All too often, however, psychological analyses operate with a wide spectrum of variables of very different degrees of abstraction, ranging from genes and biochemistry over metabolism to brain structure and function, to self, motives, desires, will-power, intentions, goals, theories and attitudes, to individual phenotypic measurements, over social norms to collective behavior and society in general. It all looks very convincing when presented in textbooks, but what it really boils down to in the end, is a futile exercise in trying to camouflage unavoidable Rylean category errors with truly catastrophic effects for the causal analysis. Modern psychology, based as it is to a large extent on such clever exercises of rhetoric, may actually harm more than it benefits the scientific study of human nature by deflecting the focus away from the essentials in the causal chain of events (Nyborg, 1994a).

Nature–nurture models may be seen as much more precise tools, but they represent just another case of dualist rhetoric. They are typically built around Fisher's analysis of variance model, and presume that independent proportions of genetic and environmental variances can be identified and added up linearly to explain 100% phenotypic variance. While quite useful at the descriptive level, such models are causally as empty as classical psychometrics, because their focus is on individual differences around population averages. They thereby statistically average across person-specific within-group differences in actual causal agents and mechanisms (Nyborg, 1987, 1990, 1994a). Unidentified genes are presumed to interact through unknown mechanisms with highly abstract and usually intuitively defined environmental components such as rearing, norms, and social interaction without any consideration for the biological locus of action except in terms of an unspecified mind or brain. The extended use of reified superorganismic concepts makes nature–nurture models basically dualistic. Molecular genetics might provide a much needed differential cause–effect perspective on the nature–nurture interaction, but then we are no longer talking about the traditional nature–nurture models in behavior genetics, but an all-bottom type of analysis. The all-bottom approach is described at the end of the epilog.

2.3 Hans Eysenck's position

Eysenck spotted many of the dangers long ago (e.g., Eysenck, 1952, 1960, 1967, 1970, 1979, 1983, 1985, 1996), and proposed a detailed program for successfully proceeding beyond surface analyses based on psychometrics, correlations, and factor analysis. These analyses have performed well in the past, according to Eysenck, but they do not bring us below the descriptive level. Eysenck therefore wants to combine the correlational with the experimental tradition, in order for psychology to finally attain proper unitary scientific stature. In this he follows the advice of Cronbach, addressing the American Psychological Association in 1957. But Eysenck stresses again and again the critical importance of establishing a good (cognitive?) theory before starting an experiment. This intent is seen most clearly in the introduction to his book *Genius: The Natural History of Creativity* (Eysenck, 1995, p. 1) where first of all he quotes W. L. Bragg: "The important thing in science is not so much to obtain new facts as to discover new ways of thinking about them." Too often psychologists search for "facts" without stating a prior theory. But then again, Eysenck agrees that: "One's knowledge of science begins when he can measure what he is speaking about, and express it in numbers" (Lord Kelvin, cited in Eysenck, 1995, p. 4).

Eysenck wants to re-construct psychology on the basis of several different traditions. To Danzinger's (1990) three models of research: "The experimental (Wundt), the psychometric (Galton) and the clinical (Kraepelin)," Eysenck would like to add a fourth: "The psychophysiological–genetic approach (Helmholtz)."

With the correlations approach, the new Eysenckian psychology has an explicit focus on the individual differences tradition coming from differential psychology. It certainly is no coincidence that Eysenck was one of the founding fathers of the International Society for the Study of Individual Differences, and also the founder and co-editor-in-chief of the prestigious journal *Personality and Individual Differences* since it went to print. With his differential perspective, it is worth noting that Eysenck is not a particularly great admirer of the idiographic approach:

> If a person is unique we cannot study him scientifically, because we cannot measure his unique aspects, or compare him with others. We cannot even prove that personality is unique, because that would involve measurement, which is explicitly condemned as disregarding uniqueness! Indeed, we would have to abandon all psychological terms and concepts which allow us to compare individuals; ... This whole approach ... leads to a completely non-scientific approach to the study of creativity and personality. (Eysenck, 1995, p 7)

Eysenck here criticizes Allport's (e.g., 1965) preference for studying the unique personality of the single individual. Allport, for his argument, objected strongly when Eysenck (1952) stated that: "To the scientist, the unique individual is simply the point of intersection of a number of quantitative variables." Allport found that the fully legitimate interest in analyzing the mutual interdependence of part-systems within the whole system of personality, was threatened by the analysis of separate dimensions whereby many persons may be compared.

A fourth step in a fully developed Eysenckian psychology is its transformation into the ranks of the natural sciences (H. J. Eysenck and M. W. Eysenck, 1989); here he sides with many other notabilities, including Watson and Skinner (and William James, see later). The "fuzziness" of psychological concepts such as intelligence, creativity, and personality is no hindrance for such a move, according to Eysenck. Psychological concepts are no more fuzzy than physical concepts like gravitation, electricity, or metal. Gravitation and temperature can, for example, be defined in various ways as can intelligence, but that renders none of these concepts useless, even if it awakens caution. Though the notion of "action at a distance" appeared absurd even to Newton himself, he used it well and so do we today (but now sided by at least two other very different definitions). Neither is psychology, methodologically speaking, much worse off than physics. Temperature measured by resistance and by expansion of metal give scales that differ somewhat in various ranges. So do different measures of intelligence and personality. The point is that concepts in any science should be judged in terms of their usefulness or uselessness, says Eysenck. Then he stresses once again, that we first have to elaborate a useful theory, or no functional laws are likely to be found.

In a recent special review of *Hormones, Sex and Society: The Science of Physicology* (Nyborg, 1994a), Eysenck outlined in a very broad sense his view on the theoretical underpinnings of psychology (Eysenck, 1996). He sees the field of psychology as suspended within a triangle, cornered by Titchenerian psychology, behaviorism, and reductionism/materialism. Titchener (1909, 1912), it will be remembered, was inspired by J. S. Mill's (1865) notion that sensations are fugitive and temporary. However, there are "permanent possibilities of sensations" that last. Titchener developed this idea into a two-stage context theory to account for meaning. Seeing a face for the first time provides little meaning. Seeing it again, and adding the context to the visual core, such as previous contextual visual or auditory images, and the face provides meaning. With repeated perception and habituation, the context may drop off and the meaning takes on a purely physiological form. It is now embedded and unconscious. Titchener was not alone with this view. Also George Berkeley (1709) thought that to create meaning, at least two sensations (images) are needed, but with later use, one sensation might suffice. Eysenck is actually making much practical use of Titchener's context theory of meaning, which can be seen quite clearly in his recent *Genius* book. Eysenck was also inspired by Titchener's dimensional view of mind. It is thus not too difficult to see why Eysenck finds that Titchenerian psychology ought to be one of the three cornerstones in psychology.

Commenting on (what he sees as) my philosophical materialist–reductionist stance (I am neither a reductionist nor a materialist, and I most certainly do not afford the luxury of having a philosophy, as will be apparent later!), Eysenck notes that it is entirely a matter of personal philosophy whether a particular researcher feels more at home in Titchener's corner, chooses the behaviorist angle, or experiences a reductionist itch towards materialism. There simply is not yet enough hard evidence in store today, he adds, to make a qualified choice among these alternatives.

The above description may account in broad outlines for Eysenck's view of psychology at the brink of the twenty-first century. At least, it hopefully provides the reader with an idea of how far Eysenck is prepared to go in denouncing classical psychological views in future examinations of human nature.

2.4 Unended quests in Eysenckian psychology

Eysenck has without question had a colossal impact on the development of twentieth-century psychology and psychiatry. In the following I will first ultra-briefly comment on selected highlights of his impact, and then focus in more detail on unended quests.

Many chapters in this volume testify to Eysenck's genius in identifying likely distal evolutionary and proximal biological underpinnings of consequential phenotypic behavior, even in early times when little empirical evidence could be mustered in defense of the view. This legendary foresight may, in part, explain why Eysenck is the most cited psychologist, and next to Marx and Freud (what strange bedfellows!) is the most cited person ever. If citation means importance, then Eysenck is extremely important. He has set a heavy mark on many directions in psychology. He has exposed the fallacies of psychoanalysis, and opened many people's eyes to the importance of a proper scientific approach to personality and intelligence. He has formed the course of behavior therapy, and changed the way we look at psychopathology. He has emphasized the use of Pavlovian principles, so that concepts of conditioning, arousal, and cognitive inhibition found a safe place in Western psychology, and he has done so with admirable clarity and at times also with surprising force. When I once commented on the latter aspect, he just laughed and said "I don't mind blowing fresh winds through the dusty halls of academia." The contributors to the present volume have done their best to document Eysenck's influence in their areas, by illustrating how it guided research, clarified or provoked counterattack.

On balance it is only fair to mention that several of the chapters also air concern about unended quests in psychology. Although Zuckerman readily admits his profound debt to Eysenckian personality theory, which he finds provides a much-needed bridge between genotype and behavior via biochemistry and neurologic intermediaries, he nevertheless finds it appropriate to present an "alternative-5" model, and to suggest that his own sensation-seeking scale taps deeply into Eysenck's psychoticism dimension. Eysenck and he continue to discuss this matter. Chris Brand finds it problematic to settle for just three Eysenckian personality dimensions. Perhaps very intelligent people have a more differentiated personality than the less intelligent. For this and other reasons, Brand therefore settles for an alternative "Big 6" solution. Eysenck is not particularly happy with this, and so the discussion about the dimensioning of personality goes on. Brand further wonders whether Eysenck's search for psychological underpinnings will bring anything useful that previous mechanistic approaches couldn't. He suggests that dimensional personality variation may be better accounted for in terms of individual dynamics, purpose, and biological function. For reasons given in chapter 20 and below I fear that to explain anything in terms of "purpose" is another dead end.

Gray and the very active group around him are not too happy with Eysenck's interpretation of his own E and N axes, so they rotated them and provided a different causal interpretation. Eysenck in turn, is not too happy with the re-interpretation. A related problem here is the fact, that the intensive research series by Gray and others demonstrated inconsistencies in their own approach and, still worse, the group sees no easy solution to these problems. Strelau and Zawadski ask whether Eysenck is correct in assuming that his personality dimensions equal temperament. They found that E and N correlate with almost all temperamental scores, but also that P relates to sensation seeking only, a finding that interests Zuckerman. They suggest that P differs fundamentally from E and N, and perhaps reflects a temperamental disposition to inhibit impulses. Nias carefully inspects the controversial evidence on cancer–personality and nutrition–intelligence relationships. Overall, he finds that the observations made by Eysenck and others in these areas are promising, but he also stresses the need for good replication studies.

In light of the ongoing discussion of the status of the P dimension in Eysenck's personality theory it is interesting to note Sybil Eysenck's warning in chapter 6, that leaving P (and L!) out of consideration will seriously damage the true picture of personality. Reviewing the evidence for Eysenck's biosocial theory of crime, Raine finds that large parts of the theory are intact and well after all these years. He wonders about its future, however, and ask whether the problem of growing crime and violence would require a modification of the theory. Methodologically, Eysenck's biosocial theory could benefit from molecular genetics and discordant twin studies. New imaging techniques could further increase our knowledge of brain dysfunction in arousal, conditioning, and emotional regulation, and permit us to go beyond skin conductance and heart-rate measures. It concerns Gudjonsson that a high P score does not always correctly identify persistent criminals, and that high P scorers actually form only a small part of the criminal population. He further finds that Eysenck's theory of crime has had more success with research into the causal basis of crime than with attempts to prevent it. Wilson notes that, whereas Eysenck's personality theory seems to account reasonably well for essential aspects of sexual behavior, it encounters problems in explaining sexual conditioning. Revelle concludes part I by reviewing the evidence on how impulsivity relates to extraversion. He finds this task difficult, in part because Eysenck changed his view over time, in part because Gray (and others) have suggested alternative interpretations that Eysenck does not endorse. There are also problems with how to best measure impulsivity. Revelle finds that impulsivity is very important for the understanding of individual differences, but it has not yet found its proper place in multidimensional personality theory.

As in part I, the authors of part II fully acknowledge Eysenck's tremendous influence on the development of their area of specialization. As in part I they report on remaining problems. Jensen underlines an important distinction in research on intelligence by asking: Is reaction time a function of higher-level cognitive processes?; or Can the speed and efficiency of neural processes explain the observed covariance between reaction time and Spearman *g*? We simply do not know, and Jensen finds that only further brain research will provide the answer. Vernon finds that genes explain much of the phenotypic covariance between intelligence and personality, but also that is time now to move in new directions in behavioral genetics. Lynn notes, in the light of many later studies, that Eysenck surely was right when he changed his early environmentalist view and suggested that race differences in intelligence probably reflect a genetic component. We are now concerned with the important question of how the genetic component came into being in the first place. Lynn proposes a geo-climatic theory. In dealing with the question of intelligence and information processing, Deary notes that attempts to identify the basic cognitive processes, primitive or raw brain processes, have so far been largely in vain. Neither does he find much success for Eysenck's idea of a unification of the differential/correlational with the experimental/cognitive approach. Brody notes, in his attempt to determine the degree to which environmental intervention affects intelligence, that Eysenck's authoritative view that our biology predestines to a large extent our future seems basically correct. That being empirically established by now, our next task is, according to Brody, to examine in intimate detail the biological components behind intelligence.

In part III, Irene Martin follows up on the vast importance of Eysenckian personality theory, but she also notes that research on human eyelid conditioning has not benefited much from the theory, and that there might be a problem with whether high P/high E–low P/low E differences can be explained in terms of impulsivity. Theoretical terms such as inhibition and excitation are today used mainly as explanatory concepts, arousal has resisted attempts at definition as has its links to attention, and there are not many bridges between contemporary conditioning theories and therapy. Martin would like to see further clarification of the physiological underpinnings of E/I and N, as she finds Eysenck's notion of a factor of conditionability that transcends specific response systems untenable.

The concepts of inhibition and excitation are now used as descriptive concepts in cognitive research as a basic mechanism for fine-tuning cognitive processes, but their precise role remains uncertain. She finally regrets that conditioning studies often fail to take individual differences into account. Claridge inquires into the rather open question of how to visualize the biological elements common to neurotic anxiety-based high arousal states (N) and the opposite "extraverted" (E) disorders. Like others, Claridge expresses some concern about the metrics of the P scale and about how psychoticism has been conceptualized—not so much from the personality theory side but from a clinical point of view. He thus wonders whether the assumed close association between aggression and schizophrenia in a dimensional scheme might be looser than postulated. Perhaps part of a more modest relationship can be explained by other factors? However, Claridge does not doubt for a moment that P remains highly relevant for our understanding of serious mental illness, though perhaps not with the status of a primary etiological factor in psychosis.

It could actually be crucial in determining actual psychotic breakdown. In his detailed review of the psychophysics and psychophysiology of extraversion and arousal Stelmack finds some support for the view that introverts are more sensitive to physical stimulation than are extraverts, and that differences in extraversion involve individual differences in fundamental motor mechanisms, quite as suggested by Eysenck. However, he finds little evidence for the notion that individual differences in cortical arousal actually determine these effects. Perhaps differences in extraversion involve peripheral brain stem and spinal motoneuronal processes rather than central cortical arousal mechanisms, Stelmack asks. But even if dopaminergic activity modulates sensory input and response output, Eysenck's original conception is really not far from the mark.

Rushton, in his discussion of (im)pure genius and its relationship to psychoticism and intelligence, expresses skepticism about the repeated reassurances that creativity only correlates with IQ up to 120, and argues that many of the most creative scientific disciplines today most likely require a much higher IQ for success. Other evidence suggests that IQ correlates significantly not only with the complexity of a task but also with achievement within an occupation. Rushton finds that the ordinary term "motivation" may be too self-willed to explain the almost obsessive–compulsive behavior of many geniuses. Perhaps they "get high" more easily than others in a fashion that looks very much like being under the influence of stimulant drugs. Perhaps the urge of a genius is better described in terms of acting out a unique value system, "super-ego" or a "concern for excellence" than in terms of the personality traits at the high end of the psychoticism scale, Rushton suggests. Nyborg notes, while discussing details of Eysenck's creativity theory that the particular genes made partly responsible for shaping the components behind creativity are not yet identified, and that dopamine more likely is an important covariate than *the* key physiological variable in creativity and psychoses. Moreover, a strong dopamine hypothesis runs into difficulties in explaining the marked sex-related differences in creativity. Nyborg notes that the role of P in creativity and psychoses might be more complex than anticipated. Finally, he finds that multiplication of inferred psychological with measured biological concentration factors leads to serious problems with the interpretation of the end product. What does it mean in causal terms to multiply high dopamine with ego-strength?

Even though Eysenck has not himself published much on occupational and organizational psychology, his personality theory had affected many working in these areas, according to Adrian Furnham, and led to the construction of the Eysenck Personality Profiler measuring 21 primary factors in addition to the super factors PEN. Furnham finds that, in general, the extant research from classical personality theory on occupational/organizational variables has been largely disappointing, and that the few interesting results are rarely followed up and the theoretical implications exploited. It is to be hoped that Eysenck's theories would in the future make the occupational psychology and organizations behavior literature benefit from each other.

Suitbert Ertel explains Eysenck's interests in sunspot-related bursts of creativity, and notes that anomalies in solar activity do covary with discontinuities in human cultural history. At the same time Ertel cautions that the explanation for this is presently quite speculative. He hopes that our present ignorance about helio-dependent effects on creativity, perhaps mediated via an underlying neuropsychological mechanism, will soon diminish, but refrains wisely from expecting surprising advances in the field. Dean, Nias, and French review Eysenck's involvement with graphology, astrology, and parapsychology, and notes that each area has a solid core of testable ideas. Actual tests suggest, however, that effect sizes for graphology are too small to be useful, astrology shows effect sizes that probably can be explained by nonastrological effects, and nobody has so far claimed the million dollars in prizes for the conclusive proof of psi.

Summarizing this brief exposure of unended quests, there seems to be fairly widespread disagreement about the number of personality dimensions as well as their causal interpretation; some find that the suggested psychological underpinnings of both personality and intelligence meet problems with interpretation and empirical foundation; almost all agree that the biological underpinnings of both need further elaboration and clarification. Another way to phrase this is that there is a growing consensus—including Eysenck himself—that essential areas in contemporary psychology face problems with explanation in terms of psychological concept, and that future research needs to focus more precisely on the physiological basis of human nature. Translated into the Eysenckian triangular view of psychology, this means that many leading scientists in the areas of intelligence and personality are now on the move towards the reductionist/materialist corner in the triangular space, demarcated by Titchenerian psychology and behaviorism in the other two corners. At the same time, few seem ready to entirely skip psychological or cognitive explanations in one form or another.

This ambivalence creates an interesting situation. On the one side, there is no doubt that further work on the psychological side will lead to increased theoretical sophistication and, perhaps, also to methodological elaboration of top-down and bottom-up approaches. At the same time, there is a real danger that further elaboration of psychological theory will intensify the Babylonian body–mind confusion, if only at an ever higher level.

Given that this characterization of an increasingly difficult situation is not totally overwrought, it is perhaps time to stop, and try to resist further temptations to explain human nature in terms of mentalism and anthropocentrism. The next section briefly discusses the outline for a program designed specifically to study (human) nature without mentalist connotations, body–mind confusion, and anthropocentrism. Space limitations leave room for only a simplified account, but even this probably suffices to sicken some of the happy psychologists and philosophers who faithfully subscribe to mentalism and love to chase reductionists.

3. THE TWENTY-FIRST-CENTURY SCIENCE OF HUMAN NATURE

3.1 Introduction

A new "mindless" program would see the body–mind schism as the principal stumbling stone for developing a proper science of human nature. It accordingly dispenses entirely with all high-level Platonic abstractions and the dualism they encompass. However, multilevel analysis is not per se seen as the problem. As long as the levels do not refer to fundamentally incompatible categories, intractable category errors can be avoided (Ryle, 1980). With abstraction reduced to an absolute minimum, the new research program can be found near the bottom of Figure 25.1.

Eysenck has, as usual, long since spotted the new writing on the wall, as have most of the contributors to this book. It is equally obvious that neither Eysenck nor most of the fellow contributors are ready yet to jump on the exclusively materialist/reductionist bandwagon. Perhaps this is a wise decision. I, nevertheless, found the following three pieces of information sufficiently important to prepare me for the jump. I landed not in the philosophically defined materialist wagon, but in a type of molecular analysis of (human) nature that philosophers probably would call an expansionist position, but what philosophers say is in general rather immaterial!

> First, more than two-thousand years of almost desperate search by the best brains for mental stuff has brought us nothing but postulates. This is, of course, no proof that it does not exist! However, an extensively researched phenomena stops serving as a useful heuristics when century after century turns without the slightest sign of proper operationalization or verification, and it becomes an increasingly less likely candidate for twenty-first-century empirical research. This alone was sufficient reason for me to jump off the intuitively defined mentalistic–cognitive–philosophical bandwagon.
>
> Second, Watson and Crick's success with their mid-century attempt to break the genetic code for life was a powerful demonstration that even highly complex phenomena such as "life" are fully amenable to exclusively natural-science analysis.
>
> Third, the recent breathtaking advances in biochemical and brain sciences make it obvious that new technologies already now provide fairly precise answers to questions which were previously addressed with intuitively based speculations and the use of philosophical and psychological tools. The new techniques address the brain directly and the results do not depend on philosophical abstraction.

In other words, any scientific twenty-first-century program for the study of human nature should start by drawing the inevitable conclusion that mentalism has failed, and then give it back to the armchair philosophers without gratitude! Second, any new program should try and combine the lesson from the successful demonstration that all different life forms reflect different combinations of just four bases, with the lessons emanating from the recent explosive methodological progress in physics, chemistry, and brain imaging techniques. Together these lessons will provide a fertile soil for a new generation of cross-disciplinary scientists by allowing them to harvest genuine empirical fruits in formerly so-enchanted gardens. No longer burdened by the futile task of circumventing or hiding anachronistic body–mind traps, these scientists would be free to examine how matter interacts with matter in humans as everywhere else in the universe, in accordance with a few simple natural-science principles. Of course, such a move has to be closely monitored experimentally by the uncompromising application of step-by-step testable models operating at a coherent level of analysis. The big bonus is, that all the lofty talk about complementary effects between a qualitatively different subjective observer and the objective world would boil down to empirically addressable questions about what interacts with what at the material level, and only that. Obviously, in this approach the term "model" would refer to particular physico-chemical brain states.

One research program designed to accomplish this is called physicology. This molecular all-bottom approach was named physicology, because all explanations based on abstractions like psyche, ego, or cognition and high-level superorganismic concepts are substituted with references to their physico-chemical underpinnings. The physicological research program is presented in detail in (Nyborg, 1994a, b, 1997a, b). Here only a few aspects will be highlighted, and then only as they relate to relevant aspects of Eysenckian psychology.

3.2 The use of theory in psychology and physicology

Eysenck insists, as mentioned before, that without a good (cognitive?) theory, it will be impossible to find functional laws. A physicologist would question this, by demonstrating that such a task basically needs only two a priori assumptions (read: molecular states): (1) That molecules have differential stereotaxic affinity; and (2) that the affinity-related flow of energy in space–time coordinates defines the developmental characteristics and behavior of any system, irrespective of molecular complexity and whether it is carbon-based (i.e., organic) or not (i.e., inorganic).

A simple case illustrates the point. Let three molecules contact each other, randomly or forced. Given that two of the molecules have high stereotaxic affinity for each other, they might form a new species of chemical behaving in ways that differ from the previous behavior of the single molecules, while all the time leaving the third molecule with a different affinity untouched. The differentiation in this very simple system, including the birth of new functionality can (1) be described entirely in physical and chemical terms, (2) be analyzed by ordinary natural-science methods, (3) be extended ad libitum to systemic behavior of any complexity, including humans or stars, and (4) it does not require any a priori theory to observe how meeting molecules with different affinities bring different types of order into an otherwise homogenous or chaotic world. In fact, physicology dispenses entirely with abstract concepts such as theory, understanding, meaning, and purpose. Such anthropocentric terms do not apply to molecules, whereas affinity, space–time position, concentration, action, reaction, conformation, transcription, and metabolism do. We only have an untrustworthy nonempirical philosopher's pessimistic prediction, that this all-bottom analysis will never suffice to describe the nuts and bolts of human nature and society.

3.3 The three analytic windows in physicology

To start assessing whether the two a priori mentioned above are not only necessary but also sufficient for the study of human nature, the physicological analysis begins with the opening of one or more of three windows: The intrasystemic, the intersystemic, and the extrasystemic (for details, see text and Figure 20.3 in chapter 20, or Nyborg, 1994a). The material basis for various mentalist and superorganismic reference concepts can then be examined through these windows by examining their most likely systemic physico-chemical addresses, the interaction of these with activities on other physico-chemical addresses in the vicinity, and the consequences of all this for, say, phenotypic behavior. Behavior is, in this framework, seen as a special case of the global molecular transport of complex (carbon-based) systems (like humans), or parts thereof, in space–time coordinates.

The physicological analysis through the intrasystemic window presumes that consciousness refers to physico-chemical rather than cognitive processes; intelligence, personality, and symbolism to intrasystemic metabolism rather than a flow of ideas; meaning to molecular changes instead of the use of logic or attribution of emotional importance; cognition to synaptic activity; premonitions to the state of physical parameters, and philosophy corresponds to molecular flow-patterns.

The intersystemic window is reserved for a special case of extrasystemic analysis, namely, the molecular correlates of so-called social interaction. Social interaction is defined as systematic exchanges, not of signs, attitudes, or meanings, but of physical stimulus patterns. Pedagogy is the more or less systematic environmental realization of molecular options within systemic physico-chemical constraints, not limitless accumulation of abstract instructions, norms, or culture conveyed by significant others. Love refers to a special set of coordinated adjustments in gonadal hormone parameters in two or more individuals, ultimately in the evolutionary service of another entirely physico-chemical phenomenon: Reproduction. This is not exactly the way romantics see it, but they may have to admit that this arrangement fits perfectly in a natural-science view of our evolutionary past as a series of entirely physico-chemically coordinated arrangements. At least the physicological analyses need not be pestered by unclear anthropocentric ideas of mysterious mental qualities emerging during evolution to raise humans to a status close to God.

The extrasystemic window focuses on all nonsocial external physico-chemical stimulants, some of which may have an impact on intrasystemic physico-chemical parameters. An example is the hormonal exchanges between the mother and her child in the womb. This prenatal arrangement can dramatically influence the body or brain development of children. Moreover, a multitude of variations in the mother's entirely physico-chemical environment may alter her body and brain chemistry, which in turn may affect her unborn child. This illustrates that the physicological analysis does not operate on the previous widespread assumption of a decisive inner–outer distinction with the skin as the border between subject and object, but is rather concerned with dynamic molecular interactions, the foci of which may be examined through different analytic windows.

The analysis of cultural differences also belongs to the extrasystemic window, and this is not as surprising at it may first seem. Culture-related similarities in behavior (themselves molecular phenomena) reflect, according to physicology, a certain degree of molecular commonality in geographically defined groups of people. These commonalities came about as a function of selection among many different molecular constellations. Only those constellations that were compatible with economic adaptation to their particular ecological niche during evolution survived, had a reproductive advantage, and were able to raise competent children capable of surviving in the harsh competition in their generation. There is no place in a causal physicological analysis for unclear notions of reified local prescriptions, cultural stereotypes, or other superorganismic concepts with postulated effects on behavior.

3.4 Uniqueness, causality, and the science of human nature

Eysenck holds, as mentioned, the opinion that a unique person cannot be studied scientifically, because there would be no standard with which to compare this uniqueness. We would have to abandon all psychological terms and concepts which allow us to compare individuals, and no functional laws could be derived.

In contrast, uniqueness presents no particular problem in the physicological analysis. The meaningfulness and explanatory value of psychological terms is questioned anyway, and all functional laws can, in physicology, be derived from one simple basic molecular characteristic, the functionality of which does not depend on theory. Psychological terms, concepts, theory, and philosophy serve in this system only as convenient descriptive shorthand references in need of physico-chemical addresses in space–time–phase coordinates.

The important a priori for the physicological analysis is, as said before, stereotaxic affinity that primarily harnesses what molecules can and will do when they meet. It is affinity that reduces entropy by self-organization of molecules into body and brain structures. People differ substantially in which molecules meet where, when, and under what circumstances, during development and adulthood. It is this molecular variability that determines individual differences in internal structures and functioning and, accordingly, in phenotypic behavior at large. We seem to be talking about molecular continua here. It has to be remembered, however, that nonlinearity is the rule rather than the exception in molecular cause–effect relationships (e.g., Nyborg, 1994a, 1997a, and see later), and this dissolves the problem in traditional (linear?) psychological theory with understanding how to analyze individuals getting "unique" scores. A "unique" phenotype is a natural causal consequence of a unique molecular constitution but it, nevertheless, conforms fully to the same law that defines "modal" development, including individual differences in body, brain, intelligence, personality, and death.

The physicological view is, to press the point slightly, that there is no scientific detour around uniqueness; this is the only direct way to reveal the true causal basis of human nature. In this, physicology actually agrees with Allport's (e.g., 1965) strong advocacy for the study of uniqueness, and disagrees with Eysenck's strong condemnation. However, with respect to causal analysis, physicology is more in line with Eysenck's emphasis on physiological and neural factors than with Allport's somewhat imprecise stress on "dynamic" psychological factors.

Methodologically, the task of generalization in physicology goes through a series of comparisons of individual molecular constellations to see to what extent they represent common characteristics (Nyborg, 1977), so we have to start with individuals (unique or not). This is so, because the major weakness of the usual group averaging approach and the study of individual variations around the mean is that they do not allow for closer examination of the causal agents and mechanisms underpinning phenotypic characteristics. The averaging process does not allow for control of whether different causal agents or mechanisms result in similar phenotypic scores, neither does it reveal whether different phenotypic expressions were caused by similar agents or mechanism. As everything is averaged, the individual with all its internal characteristics disappears in the process. What we are talking about here is the question of using the individual differences versus the different individuals approach. Unfortunately, the detrimental consequences for the exact causal analysis of choosing the individual differences approach are often overlooked in nature–nurture studies (Nyborg, 1987, 1990a,b) as well as in psychometrics (Nyborg & Sommerlund, 1992).

To summarize, Eysenck finds that the unique person cannot be studied scientifically in psychology, whereas physicology finds that there can be no proper causal study without focusing on the individual, unique or not. We must insure that we have correctly identified the relevant causal agents and mechanisms in each individual before we begin to generalize across individuals to human nature. I called this the "idiothetic" approach (Nyborg, 1994a, p. 164), because it combines the idiographic with the nomothetic approach. The idiothetic approach may in fact be the only scientifically acceptable way to causally connect DNA, over body, brain, intellectual and personality development, to society, without having to average across different causal events, mechanisms, and effects and thereby obtaining an anonymous average that may neither fit any particular individual in the group nor say anything in particular about human nature.

3.5 Methodology

On the methodological front, Eysenck recommends a unification of the correlational/descriptive with the experimental/cognitive method. As the physicological view on what determines human nature differs fundamentally from the psychological view, it is only natural that physicology also calls for other methods. A few examples may illustrate this point.

Nerve cell membranes can be defined either at the cellular level, or as conglomerates of associated molecules "frozen" in space–time coordinates in accordance with their specific affinities (e.g., Nyborg, 1997b). Organs can be defined in terms of their overall function, or as mass-assemblies of molecules. Neurotransmitters can be defined as chemical species, or seen as molecules associated in a fluid state. As will be known by now, in physicology it is differences in molecular cohesion that is the common principle behind all these seemingly very different bodily and brain structural and functional manifestations. The molecular focus allows the physicological analysis to dissolve the usual sharp distinction between structure and function at the bottom level. Another consequence is that body and brain structures and functions are defined not so much in terms of permanent traits as by more or less permanent molecular states with all sorts of gradual temporal transitions and changes as the neighboring molecular circumstances dictate. Plasticity becomes an option rather than an enemy in structural–organizational terms. The dynamics pave the way in physicology for defining the brain as a fairly unstable molecular system constantly at the brink of sudden change. Instability or changes in, say, the permeability of a nerve cell membrane can be mediated by sudden endogenous coordination of otherwise chaotic firings in channel proteins building up energy, or by stimulus-phase-locked or stimulus-related changes caused by exogenous import of energy. In fact, the major technical difference between an educable brain and a stone is the difference in molecular stability. As the method must suit the analytic task at hand, a physicologist would obviously prefer natural-science methods to handle such molecular mass-interactions. These methods make us able to check the causal chain of molecular events behind observations in psychology, anthropology, or sociology.

Biological actions and reactions are often, as said, of a nonlinear nature. This complicates the analysis tremendously, but a fully developed physicological analysis must be able to handle this (Nyborg, 1997b). Massive number-crushing computers are needed for the task, as are numerical and graphical simulations. Descriptive sequential analysis of massive molecular cascades of interlocking events must be dealt with in appropriate ways. Pattern-recognition algorithms have to be adapted and perfected. Existing parallel computation algorithms of fluid dynamics will need adaptation to fit the task at hand, and we need to attend to problems with shared and distributed memory parallel computation of dynamic load balancing and parallel fluid-flow visualization, preferable in the form of small "movies" to illustrate covariant changes over time. Of course, none of the simulation techniques can substitute actual wet experiments or real-time brain monitoring, but they may act as a valuable supplement as we progress. No doubt, these complex computations and simulations will greedily ask for hitherto unseen raw computer power, that will make contemporary simulated nuclear explosions look simple.

The encouraging perspective is that it should, at least in principle, and perhaps one fine day also in practice, be possible to simulate the molecular machinery of a human being in dynamic interaction with its environment in real time. No longer bothered with mysterious intervening variables and effects of hypothetical constructs at a high level of abstraction, it should be possible to go beyond "simple" mappings of the genome, and reconstruct the molecular cascades of events that unfold in the space–time coordinates between DNA and the physico-chemical environment. This would amount to nothing less than finally coming to terms with human nature by demonstrating that an individual is a material-interacting organizational part of nature rather than a philosophical construction isolated from and elevated above a material world.

In that case the ancient body–mind problem would finally have withered away after 2400 years of obsessive search for still more sophisticated abstract constructs and references to a mind that might not be there. What remains after the body–mind dust has settled are thus a number of practical problems. They are complex, indeed, but nevertheless much easier to tackle than the quicksand of dualism, from which there is no escape. The bad news is, however, that there is not the slightest doubt that methodological imperfection will for quite a while constitute a major bottleneck and be the worst enemy of progress along these lines.

3.6 Reductionism, materialism, or expansionism?

Reductionism is clearly held in favor by Eysenck but he is, nevertheless, not ready for the wholesale reduction of psychology. He supports a Helmholtzian psychophysiological–genetic approach but not full-scale materialism. When he discussed my position as materialistic/reductionistic, he added that I might demur with the designation.

This caution is well placed, because physicology is not a philosophical position in any practical sense of that term. It is a simple research program for the study of what happens when molecules meet and some of them stick together. Odd things happen when this occurs, indeed. People and stars are examples of such events, but their coming into existence might seem odd only because of our ignorance! Their ontogenesis has nothing to do with philosophy and everything to do with stereotaxic affinity and changes in energy distributions. This hardly counts as a philosophical position in the usual sense. Critics sometimes say: As a physicologist you say that you have no philosophy? That is then your philosophy! Such philosophizing friends do not seem to realize that such word games mean nothing to a causal analysis, where the proof is in the effect. Neither is materialism a suitable label for physicology. There are many different materialistic positions, and none of them looks even slightly like the ultra-minimalist position of physicology.

Eysenck sees physicology as representing a reductionist position, and this requires a comment. The reductionism critique boils down to an ingenious linguistic gadget, invented by philosophers to protect their precious abstract conception of human nature. The logic of the reductionist critique allows philosophers to accuse physicology of being a grotesque misrepresentation of the truly elevated status of human nature. What they forget to say is that the reductionist critique presumes an a priori existence of something above the material dimensions to be reduced. This would be true if Plato's shadow on the cave wall represented more than what was in the physical optics of the situation, more than what met the eye, and more than what altered the molecular state of the observer. Only in such cases would the reductionist criticism apply. However, nobody has ever documented the existence of these extra and abstract representations (environmental or mental). The proof for the abstract mind is in the postulate, intuitively understood and never documented. The reductionist critique is therefore shooting itself in the foot, and cannot be taken seriously except in circles where arguments precede evidence. This becomes all the more obvious in an era where the brain sciences come closer and closer to answering pertinent questions previously far beyond empirical reach.

3.7 Natural science and the study of human nature

In the physicological analysis, the role of theory and concept are much less important than are answers to empirical questions about affinity and molecular mass interaction. It, for example, does not matter greatly whether we associate one concept or another with the molecular processes of defeminization and masculinization of a female fetus by androgens, or with the inverse cycling of female verbal and visuo-spatial abilities as a function of monthly changes in plasma estrogen. What's in a name? The important task would rather be to monitor where sex hormones go in the organism; which receptors they induce where in the organism; what happens when the hormone–receptor complex enters the cell and is activated, and which genes in the nucleus change expression when high-affinity parts of the genes "steal" the hormone from the lower-affinity receptor and begin or stop producing proteins as a result; where the newly transcribed protein products go in body and brain tissues, what their biological effect is there, and what all this means for the process we call sexual differentiation. This is not just another clever name game of inventing hypothetical constructs and intervening variables to explain what happens, but a question of carefully monitoring in a step-by-step fashion the molecular cascades of events in empirically verifiable ways with the use of natural science tools.

As mentioned in section 2.3, Eysenck, and many before him, have suggested that psychology should ideally transform into a natural science, but nobody found an easy way. Köhler (1960) remarked that most attempts to deal with the body–mind problem tacitly accepted the existence of emergence—referring to the assumption that when systems become very complicated, entirely new forms of action would arise that were not valid on lower levels. Such a discontinuity would preclude the transformation of psychology into a natural science like physics, as would the assumption that values guide behavior. Köhler indeed found it likely that an organism simply consists of special configurations of cells, by which events are given particular directions, distributions, localizations and so forth (ibid., p. 17). The system is open to absorb energy from the outside, and all actions in the brain must, as a particular kind of process, be known to natural science. Pepper (1960, p. 39) assumed that the physiological body is a function of cells occupying a limited volume of the space–time field, which would place an analysis of it solidly within the reach of chemistry and physics. Putnam (1960, p. 175) found that the body–mind problem is nothing but a different realization of the same set of logical and linguistic issues, so it must be just as empty and just as verbal. Jacob (1982) found that the ever-recurrent problem in the natural sciences is to get rid of anthropomorphism, such as the purposive activity of man. He found that "endowing the elementary particles that constitute matter with some kind of a psyche does not help much, and the conclusion is inescapable that mind is a product of brain organization in the same way that life is a product of molecular organization" (p. 59). Jacob nevertheless found that the study of man can neither be reduced to biology nor do without it (p. 62).

These eminent scientists saw, in other words, the many problems with explanations above the brain, and recommended physical explanations instead. Common to them was also, that they were unable to formulate the formidable problems at hand in terms of stringent natural science methodology. William James, while fiercely advocating psychology as a natural science, was undoubtedly also the most pessimistic of them all. In 1890 he delivered the manuscript for his two-volume *Principles of Psychology* to a publisher with the following characteristics. It is "a loathsome, distended, tumefied, bloated, dropsical mass, testifying ... that there is no such thing as a *science of psychology* ... ." He indeed wanted psychology to be a natural science, but found that "the waters of metaphysics leak at every joint ... ." He found it strange to " ... hear people talk triumphantly of *'the New Psychology,'* and write *'Histories of Psychology,'* when into the real elements and forces which the word covers not the first glimpse of clear insight exists. A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that we *have* states of mind, and that our brain conditions them: but not a single law in the sense in which physics shows us laws, not a single proposition from which any consequence can causally be deduced. We don't even know the terms between which the elementary laws would obtain if we had them ... This is not science, it is only the hope for a science."

To this I would like to add that now, more than a hundred years later, we cannot even justifiably keep up the hope of turning psychology into a proper natural science. Psychology is a hopeless science, and it will remain so until the time when it has been totally liberated from all forms of abstract psyche, cognitions and unconsciousness, symbolisms, mentalism, and anthropocentrism. But then it will no longer be psychology as we know it today, and its name would be an oxymoron.

It is actually quite likely that molecular affinity represents James's dream of a "single law in the sense of which physics show us laws" and from which "consequences can causally be deduced." In that case, affinity will finally allow the study of human nature to turn into an experimental science that can address the complex phenomena observed in psychology, anthropology, sociology, and philosophy in terms of stringent natural science methodology. Hopefully, some economical brains will soon develop the tools desperately needed for the proper handling of nonlinear dynamics of complex carbon-based systems such as us. The implementation of the physicological research program depends on this.

3.8 Concluding remarks

The transition of psychology to a molecular science like physicology will undoubtedly be slow. It may take at least a generation or so. It is thus not realistic to expect that the many happy entrepreneurs working on the psychological and philosophical tower of Babylon will readily admit that their whole project was a terrible mistake from the beginning. As their talent and creativity lies basically in the verbal–hypothetical–philosophical domains, they cannot be expected to just turn around and take up the study of molecular dynamics. The social–humanistic and natural sciences seem to attract different levels of intelligence as well as different personalities (see chapter 20). Planck probably had a point when he said that scientific truth does not triumph because opponents become convinced but rather because inventors of the old truths die. So, a qualified guess is that for another long period of time we will see a parallel race among, on the one side, psychologists and philosophers ascending into new and higher realms of their home-made conceptusphere with armchair theorizing as the elevator and, on the other side, an army of brain-oriented natural scientists, bored with the many trivialities of modern high-tech physics and eager to simulate, and perhaps in a distant future reconstruct, human nature in all its fantastic physico-chemical variety in accordance with a few evolutionarily speaking very conservative natural-science principles for what molecules can do in terms of life, love, society, culture, and all other entirely material matters.

Eysenck's plastic brain organization and dynamic synaptic functionality has, in combination with stable and optimized personality parameters, allowed his overall molecular organization to function extremely efficiently and to express a prolonged tendency to go biological and experimental even in times where this was reacted to by most other brain organizations as being even more heretical than it is today. This activity has affected many contemporary brains in such a way that they now waste less time with repetitively running narrow (perhaps a predominantly left-hemisphere Wernicke and Brodmann area) linguistic dead-end loops, and accordingly can spare energy for the more demanding molecular activity that goes under the name of scientific activity. According to physicology, scientific activity refers to the ways adaptive synaptic activity in material brains comes systematically to terms with an entirely material environmental reality, including people and society.

4. A PERSONAL REFLECTION

This book concludes with a personal reflection on Hans as friend (if I dare say so to a man, who invited me to sleep in his daughter's bed; unfortunately, Conny was away at the time, but Sybil at least took care of the electric blanket) and as the institution that he also is. Hans is, in my opinion, to psychology what Bach was to music. Both are towering figures that combine the best of their time in elaborate compositions, marked by a developed sense for accuracy, clarity, tempo, harmony, beauty, elegance, and a variety that often takes an almost mathematical form. Both demonstrate stringent criteria for solid work, both conclude an important period in history, and both point towards the future. What more can one ask for?

Bach was acknowledged as a master by some in his period. However, as Hans writes in his recent *Genius* book: High capacity is certainly no guarantee for glory in general. The history of extreme creativity is full of appalling examples where exactly the opposite was the case (see chapters 19 and 20, or better, read the *Genius* book). When Bach one day quite unexpectedly appeared at the gate of the castle of Friederich the Great of Prussia, the music-loving monarch immediately terminated his exercise on the flute, his favorite instrument, and asked the old master to improvise a piece of music, an art form he practiced to perfection. The monarch very appropriately suggested variations over the theme B-A-C-H. Johann Sebastian sat down and composed wonderful variations over the theme—off his hands, just like that. The monarch was grateful, thanked Bach, and told him that he could eat in the kitchen. His Majesty then went off with the usual number of largely insignificant court puppets to enjoy the waiting gala dinner in the hall. Again, I find a remarkable similarity between Eysenck and Bach. Most acknowledge their genius, but the glory often goes to less significant people. Both are able to improvise over complex themes straight out of their brains (see chapter 24). Both have had to accept that the halls of glory were often occupied by lesser people. Only in recent years has a certain measure of glory come Eysenck's way (again, see chapter 24). It is perhaps no coincidence that Eysenck lets the first chapter in the *Genius* book (1995) begin with a citation of Jonathan Swift:

> When a true genius appears in the world,
> you may know him by this sign, that the
> dunces are all in confederacy against him.

It may warm your heart, Hans, to know that this book should be seen as a token, that not everybody is in confederacy against you. In fact, there are many who are grateful for your numerous brilliant improvisations over the years, and who would like to see you preside in the seat of honor at the high table.

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