Chapter 1

The Biological Basis of Intelligence

H. J. Eysenck

Professor Emeritus of Psychology
University of London

INTRODUCTION

The Concept of Intelligence

The word "intelligence"—like most scientific concepts—began life as a descriptive term used in everyday life to characterize certain aspects of behavior, or of personality. "Intelligentia," as understood by Cicero and other ancient writers, had two rather divergent meanings that can still be found in our dictionaries. On the one hand, the noun may refer to quickness of understanding, sagacity (the Concise Oxford Dictionary), or the capacity for understanding—ability to perceive and comprehend meaning (the *Collins Dictionary*). On the other hand, it may refer to acquired knowledge—"information, news," according to the *Concise Oxford Dictionary*, or the *Collins Dictionary*. Common speech also acknowledges this dual meaning of the term (Derr, 1989). Equally, science has embraced a similar distinction in Cattell's (1963) differentiation between "fluid" (g_f) and "crystallyzed" (g_c) intelligence. Clearly the two concepts are not unrelated; the first refers to a capacity or disposition that enables us to acquire knowledge, remember things, solve problems, and so on, but the second deals with the results of using that capacity under certain environmental conditions. As a scientific concept, clearly that of intelligence as *capacity* is more fundamental, while that of intelligence as *acquired knowledge* may be of greater practical importance.

From this point of view of measurement, of course, it is much easier to measure acquired knowledge than capacity. Fortunately, under certain circumstances (universal education, similar exposure to books, newspapers, and so forth, the presence of free libraries, etc.) the amount of knowledge acquired may be a good measure of capacity. In spite of the fairly high correlation between g_f and g_c in those populations mostly frequently investigated (North American, Canadian, Australian, British, European) the distinction is an important one that should never be forgotten. Many pointless arguments have been caused by failure to remember it.

It is possible, and may be useful, to extend this notion of different meanings of intelligence, taking into account scientific investigations of the concept. Figure 1.1 shows the three major concepts of intelligence that have been widely used in the past. At the one extreme we have biological intelligence, that is, a concept referring to the *biological* basis of all cognitive behavior. Biological intelligence is conceived of as being largely determined by genetics, which in turn influences the physiology and the biochemistry of the brain. It may be investigated through the use of the EEG, the averaged evoked potential, the galvanic skin response, the contingent negative variation, and possibly through the use of reaction time and inspection time measurements. It is not asserted that biological intelligence is wholly innate, and cannot be influenced by environmental factors; such biological factors as nutrition and sensory experience almost certainly influence the physiology and biochemistry of the brain. It is only in recent years that interest in biological intelligence has come to the fore, although Galton (1883, 1892) had already advocated views emphasizing the biological nature of intelligence.

Strongly determined by biological intelligence is psychometric intelligence or IQ; ever since the days of Binet psychologists have been much more concerned with IQ measurements and psychometric investigations than with biological intelligence and its determination. While IQ is clearly dominated by biological intelligence (as shown by the strong genetic component of IQ), there can be no doubt that environmental factors are also important. Education, socioeconomic status, family upbringing, and cultural factors have been shown to be significantly related to IQ, the degree depending to some extent on the nature of the tests used (Eysenck, 1979). Psychometric intelligence has had considerable practical applications, but has always lacked a solid scientific foundation.

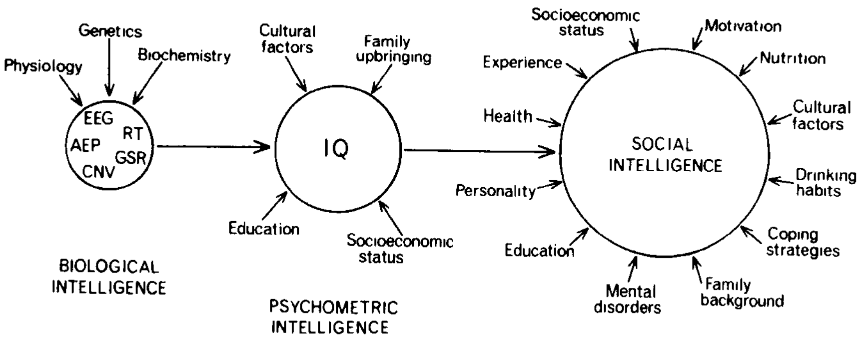

Figure 1.1. Three different meanings of "intelligence."

If psychometric intelligence is an uncertain mixture of capacity and acquired knowledge, then the third concept of intelligence, social or practical intelligence, while largely determined by IQ, is even less unitary. The term refers essentially to the more or less successful way in which people use their cognitive abilities in everyday life (Sternberg, 1985; Sternberg & Wagner, 1986).

We may suggest that IQ, because of its close relationship with biological intelligence, may be an acceptable definition of intelligence (provided its weaknesses are kept in mind), but this is not true of social or practical intelligence. The concept is far too inclusive to have any kind of scientific meaning. Sternberg (1985) acknowledges that this concept "is certainly highly inclusive in the sense that it includes within the realm of intelligence characteristics that typically might be placed in the realms of personality or motivation . . . for example, motivational phenomena relevant to the purpose of adaptive behaviour—such as motivation to perform well in one's career—would be considered part of one's intelligence broadly defined" (p. 55).

It is difficult to assign scientific meaning to such a very broad concept. Scientific advances are based on analysis, and analysis means that artificial compounds should be shunned, and that we should insist on reducing them to their unitary constituents. To bring together dispositional ability factors, personality, motivation, health, experience, and nutrition into one concept simply means that this concept is scientifically meaningless and cannot be measured. Even personality is obviously too vague a concept in this context; you may be able to measure certain aspects of personality, such as extraversion or neuroticism (Eysenck & Eysenck, 1985), but no measurement of personality as such is conceivable. The same applies to motivation. To bring together all these and many other constituents in one concept of practical intelligence is to move it out of the field of scientific investigation and theory altogether. What we must do is to measure each of the variables in question separately and then, if we wish, define social or practical intelligence by means of a formula including each of the variables as a term. Whether this is or is not a meaningful process is questionable, but it is not an issue of interest for the moment.

It is of course true that intelligence and personality can *combine* to produce behavior that is socially acceptable or not, and may prove advantageous to the individual. Eysenck (1979) has summarized some of the literature which shows that both in the high IQ group of Terman's follow-up study, and in low IQ groups of retardades a high degree of neuroticism is disadvantageous, regardless of IQ, and produces social failure. It is specific studies of this kind that are needed to give any meaning that it may have to the concept of social or practical intelligence. Even then, of course, the use of the term "intelligence" is misleading and confusing. In this chapter we will not be concerned with it any further.

What we are concerned with in this chapter is an attempt to place the measurement of intelligence, and the theory of intelligence, on a more scientific basis. Such an attempt at objective analysis has in recent years been frequently declared impossible by writers who have advocated what is sometimes called the "sociology of knowledge." This is based on the belief that it is the relations of production in a society that constitute the basis for the superstructure of ideas in a particular cultural group. Social, political, and intellectual processes within a given society were determined by the *mode of production* in the material sphere, and the attendant social relations. Marx suggested that in relating ideas to a sociological basis, it was the *class structure* that was paramount. The ideas of the ruling class became dominant in a society, and these dominant ideas were nothing more than the mental expression of the dominant material relationships. Thus ideologies emerge which serve the purpose of legitimizing the existing class structure. The measurement of intelligence, and particularly theories concerning the genetic determination of intelligence, are frequently used to exemplify these Marxist notions which, if accepted, would make any scientific study of intelligence impossible.

Recently, Buss (1975) has attempted to apply some such scheme to what he calls "the sociology of psychological knowledge." Following the writings of Berger and Luckman (1966) and Stark (1958), he conceived of his task as being broadly concerned with the social basis of the psychological academicians' ideas. His thesis is based on the belief that "there are no absolute truths in the social sciences, where the 'facts' are embedded in a particular theoretical framework which in turn rests upon certain epistemic and metaphysical presuppositions" (p. 991). In his view there is an intimate relationship between statements of value and statements of fact; "normative statements do have implications for existential statements and vice versa" (Buss, 1975). And he goes on to say that "one of the practical aims of a sociology of psychological knowledge would be to emphasize the relationship between fact and value within psychology and thereby help to make psychologists more self-conscious of the implications their research has with respect to creating a specific image of man in society'' (Buss, 1875).

One of the examples of a sociology of psychological knowledge chosen by Buss is differential psychology. He argues that the growth of capitalism depended on a growing division of labor, and specialization of human talent therefore came to replace the universal man. ''The rise of the scientific study of individual differences may be seen as a new development spurred on by a climate of quantification, where the manifest individual differences promoted by a capitalistic class society became amenable (like its material products) to strict measurement'' (p. 993). He goes on to say that the prevailing political ideology of liberalism *demanded* a strictly genetic interpretation of individual differences in mental abilities. ''Because there were individual differences in abilities as reflected by the existent class structure, such differences must reflect innate differences given the belief that each individual theoretically has the freedom and opportunity for full development'' (Buss, 1975).

Kamin (1974) applied a similar kind of argument to the American continent, and Buss comments: ''Of particular importance in the present context is the idea that a genetic interpretation of individual differences in mental ability served well to legitimise political decisions concerning the restriction of immigration from certain European countries during the 1920s and 1930s'' (p. 993). These ideas coincide with the attempted demonstration by Pastore (1949) that belief in genetic causes went with right-wing political attitudes, and belief in environmentalism with left-wing political attitudes. This whole approach was criticized by Eysenck (1976) on general philosophical grounds, but recent events behind the Iron Curtain suggest a new look at the specific example chosen by Buss (1975).

Let us note, first of all, that the widespread notion that the belief in the (partial) determination of individual differences in intelligence by genetic causes is ''un-Marxian'' and right-wing, is completely false. Mehlhorn and Mehlhorn (1981), speaking as representatives of the communist government of East Germany, explicitly condemn any such interpretations as ''unmarxistisch,'' because they contradict the clearly different positions of Marx, Engels, and Lenin (p. 7). They quote other East German and Russian psychologists in support of this view, and go on to quote Marx and Engels in some detail to the effect that genetic causes are very powerful with respect to differences in mental and artistic ability. These ideas are of course clearly explicit in the Communist Manifesto, as the Soviet psychologist Krutezki (1974, p. 140) points out: ''When it is said, from each according to his abilities, then it is clearly stated that men in this respect are not equal. . . .'' (The best sources for an understanding of Marx's position are his *Kritik des Gothaer Programmes* and the *Deutsche Ideologie* by Marx and Engels.)

Even more explicit is the statement by Lenin (1965, p. 137) that ''when one says that experience and reason testify that men are *not* equal, then one understands under equality the equality of *abilities* or the equivalence of bodily strength and mental capacities of men. It is quite obvious that in this sense men are not equal. No single reasonable man and no single socialist ever forgets this.'' Lenin goes on to characterize as an ''absurdity'' the idea of extending equality into these spheres and concludes by saying: ''When socialists speak of equality, they understand thereby *social* equality, the equality of social position, but not at all the equality of physical and mental abilities of individual persons'' (1965, p. 140).

As Guthke (1978), writing from a communist country, points out: ''Marxist psychology does not by any means deny the importance of genetic factors in the causation of individual differences in intelligence . . . [F]rom the beginning Marx and Lenin have emphasized the biological and psychological inequality of man'' (p. 69). Few Westerners, unfortunately, are familiar with the large-scale work done in the USSR using the twin methods, along lines similar to those adopted in the West (e.g., V. B. Schwartz, K. Grebe, L. Dzhedda, Y. Mirenova, M. Ishidoia, M. Rubinov, B. Nikityuk, V. Yelkin, S. Khoruzheva, N. Annenkov, and many more).

It would seem that historically, communism and capitalism give rise to similar ideas, derived from Darwin, about the importance of genetic factors for differences in human abilities; it would be difficult for any kind of sociological interpretation of psychological knowledge to suggest that the very divergent industrial and social relations obtaining in these two kinds of cultures would necessitate the arbitrary invention of such concepts. It was the brief aberration of Stalinism, with its encouragement of the Lysenko heresy, that gave the erroneous impression to many people unversed in Marxism that environmentalism found some support in the works of Marx, Engels, and Lenin; it is clear from the quotations cited here that this is not so, and indeed these quotations could be multiplied at will.

What is more, recent work in Russia, Poland, and elsewhere have very powerfully supported the view that the influence of genetic factors in differences in IQ is overwhelmingly strong. Thus Lipovechaja, Kantonistowa, and Chamaganova (1978) have recently reported a study in Moscow of 144 pairs of MZ and DZ twins, who were given the various subtests of the WISC, and whose scores were analyzed using Falconer's formula. They found a heritability of these Russian schoolchildren of 0.78 (uncorrected for attenuation), that is, an heritability in excess of that reported by Eysenck (1979) from a reanalysis of all available Western data, excluding Burt's. Similarly the extensive work of Firkowska et al. (1978; Firkowska-Mankiewicz & Czarkowski, 1981) in Poland has shown that in spite of the attempts of the communist government to introduce complete egalitarianism into the school system, the health system, and every other aspect of the individual's life, variance of IQ and correlations between IQ and social-intellectual status of the parent were similar to those found in capitalist countries. The authors rightly argued for the prime importance of genetic factors in producing the observed differences. The important work of Weiss and Mehlhorn (1980) and Weiss (1982) on genetic factors in intelligence and mathematical ability, carried out in East Germany, is too well known to require discussion. Other important references to recent empirical studies and theories in socialist countries are: Mehlhorn and Mehlhorn (1985), Friedrich and Kabatvel Job (1986), Krylow, Kulakowa, Kantonistowa, and Chamaganova (1986) and Ravich-Shaherbo (1988).

We thus arrive at a position which seems to be in exact opposition to that taken by Buss. When he says that "unfortunately (or fortunately) there are no absolute truths in the social sciences," he seems to be arguing a case which cannot be supported by the facts. Both Russian communist and English and American capitalist psychologists arrive at a figure for the heritability of intelligence which is very similar indeed, and Polish, American, and English psychologists all arrive at relationships between the child's IQ and achievement in school, and the intellectual caliber of his parents, which are similar if not identical. Thus regardless of political regime, findings in capitalist and communist countries with respect to this prime example of alleged determination of ideas by the mode of production in the material sphere and the attendant social relations, give rise to identical conclusions which must be said to have a considerable degree of approximation to the "absolute truths" which Buss denies are to be found in the social sciences.

The arguments concerning the sociology of knowledge and the possibility that work on intelligence may be influenced by political ideas have been discussed in some detail because much of the hostility to modern views on intelligence, and many of the arguments against the theories, has arisen from these ideological concepts, rather than from scientific concerns, and it seems desirable to lay this particular ghost to rest once and for all. Our concern in this chapter will be entirely with scientific arguments, although of course the question of what is and what is not scientific is one not as easily settled as might appear at first (Cohen, 1985; Suppe, 1974). The next section will review some of the arguments concerning this problem insofar as it deals with the measurement of intelligence specifically.

Science and Intelligence: Some Misconceptions

Many critics of the concept of intelligence base their rejection on the grounds that this concept is not scientific; this notion is widespread among many scientists and academics who have little direct knowledge of the research that has been undertaken to make the concept meaningful in scientific terms. Inevitably such criticisms are based on philosophical grounds, and although we shall see that they have little substance, we need to discuss them in some detail, particularly as they are quite relevant to the main contention of this chapter—namely that research into the biological foundations of intelligence is a prerequisite for the scientific acceptance of the concept.

The first criticism to be discussed asserts that theorists in this field reify intelligence, and assert its existence, whereas the critic clearly disbelieves the existence of something called "intelligence." Thus, Keating (1984) has argued that those who believe in the usefulness of the concept of intelligence appear to assume "that *it* is a thing that exists in the head of a person" (p. 2). He and many others argue that intelligence does not exist and that, hence, all efforts to measure it must be useless. This is not a tenable argument. In the first place, none of the leading proponents of the concept of intelligence has postulated its *existence* in any physical shape or form; Galton, Spearman, Burt, Cattell, Wechsler, Horn, Thorndike, Thurstone, and this writer have always regarded it as a scientific *concept*, analogous to such concepts as gravitation, humidity, society, or atoms. Scientific concepts like these do not carry an implication of existence; neither does intelligence. They may be useful or useless as far as scientific description and investigation are concerned. Their main purpose is to bring together in a meaningful shape a large variety of individual events that constitute the blooming buzzing confusion that is reality. There obviously is no such thing as "society"; there are large numbers of individuals interacting in many different ways, and assuming many different roles. These individuals exist, and their interactions (educational, criminal, marital, political, social, etc.) might be considered to exist (although even there some philosophers might express doubts), but society as such is a concept that may or may not be useful in comprehending the totality of these interactions, and cannot be predicated to "exist."

Discussions on the nature of concepts, and the question of existence, will be found in Suppe's (1974) edited book on *The Structure of Scientific Theories*. It is interesting to look at concepts like "ether," "caloric," or "phlogiston," and so forth for which existential claims were made at one time, but which clearly were concepts which, while mistaken, did help to advance the discovery of more useful concepts. Philosophical problems of this kind are somewhat intangible, and a more detailed discussion would not be appropriate here. Let us merely note that criticisms along these lines would have to be much better documented in order to carry any weight. Certainly the claims to be made in this chapter are not that intelligence exists in the same sense as tables and chairs exist, or people, or buildings. It is a concept that unifies many empirical findings in a unique fashion, and has hence been found useful. It is perfectly possible that more useful concepts will be found to describe reality, and in that case intelligence will be displaced by some other concept. What does exist, of course, is the individual brain, with its network of cells, axons, dendrites, and synapses, as well as a multitude of activities governed and regulated by the brain. These "exist" in a very real sense; intelligence does not, and in that sense it shares this quality of "nonexistence" with all other scientific concepts. To argue that intelligence is useless, and cannot be measured, because it does not "exist" is to commit an elementary philosophical error.

A second criticism is often made of the concept of intelligence, namely that there is no agreed-upon definition of the term. Consult such books as *What is Intelligence?* (Sternberg & Detterman, 1986), or its forerunner, the classic symposium published in the *Journal of Educational Psychology* 65 years ago under the title of "Intelligence and its Measurement," and one can see indeed that there is some disagreement on definition. However, as Snyderman and Rothman (1987, 1988) have shown, there is a considerable unanimity among psychologists currently concerning what is *meant* by intelligence. It is easier to recognize an elephant than to describe it!

It is important, in this connection, to realize the difference between a scientific definition, and the identification of important elements or consequences of a given concept. Thus Snyderman and Rothman found that among the 600 plus experts they consulted, there was almost unanimity concerning the importance of abstract thinking or reasoning, problem-solving ability, and capacity to acquire knowledge as important elements of intelligence. But of course these are not definitions, and neither are the many putative definitions given in the Sternberg and Detterman book. To take as an example the concept of gravitation, what would we think of a physicist who attempted to *define* it in terms of the apple falling on Newton's head, planetary motion, the movement of the tides, the bulging of the earth's equator, the falling of the moon toward the earth, "black holes," the formation of the galaxies, the shape of the planets, the paths of comets or asteroids, and the numerous other consequences that follow from positing the concept of a force that acts according to the product of the masses of the bodies interacting, and the inverse square of their distance? Clearly, intelligence is *involved* in abstract thinking, reasoning, problem solving, the acquisition of knowledge, memory, mental agility, creativity, and so on, but these are the *consequences* of applying intelligence in certain directions; they cannot be used to *define* intelligence. The fact that psychologists, when asked to define intelligence, often choose different *examples* of intelligent activity does not mean that we cannot in due course achieve a proper definition of intelligence. Perhaps in the absence of a general theory all that can be done by way of definition would be by way of a descriptive formula, such as the inverse square law of distance in the case of gravitation. Thus one might define intelligence as that which is responsible for producing matrices of unit rank when a large number of dissimilar cognitive problems is administered to a random sample of a given population, and their intercorrelations calculated. The main point to note, however, is that disagreement, so often observed by critics discussing the definition of psychology, does not usually refer to *definitions* at all, but to *examples* of intelligent activity. Here we have a wide choice, and diversity is not really disagreement.

A third objection is often put, pointing out the complete lack of an agreed-upon theory concerning intelligence. In the absence of such a theory, it is argued, is it possible to regard intelligence as a useful scientific concept? Such a view would certainly run counter to anything that the history of science can teach us. Concepts develop for centuries before agreed-upon theories arise, and often the theories on which they are based are known from the beginning to have faults. Gravitation is a good example. Newton's Action at a Distance theory was already known by him to be absurd, but it served a very useful purpose. Even now, 300 years later, there is no agreed-upon theory of gravitation. What we have are two quite dissimilar theories between which it is impossible to make a rational choice. On the one hand, we have Einstein's view according to which gravitation is a distortion of the space-time continuum, and on the other, we have the quantum mechanics interpretation in terms of particle interaction (gravitons).

Much of the same may be said about the theory of heat, where we have the thermodynamic and the kinetic theories side by side. Thermodynamics deals with unimaginable concepts of a purely quantitative kind: *temperature*, measured on a thermometer; *pressure*, measured as a force exerted per unit area; and *volume*, measured by the size of the container. Nothing is said in the laws of thermodynamics about the nature of heat. This, on the other hand, is the foundation stone of the kinetic theory of heat, using Bernoulli's view that all elastic fluids, such as air, consist of small particles that are in constant irregular motion and that constantly collide with each other and with the walls of the container, their speed of motion creating the sensation of heat. Many formulae are quite intractable to kinetic interpretations even today but yield easily to a thermodynamic solution. The unified theory here, as elsewhere, eludes physics, after centuries of endeavor. Should we expect psychology to do better? The unified theory appears at the end, not at the beginning, of scientific search, and to demand such a theory before a concept is taken seriously is to make impossible all scientific research.

However that may be, there is in any case no final, correct theory in science; what we have is a constant improvement in theory that may show considerable differences from one stage to another. Consider the very important notion of an element in chemistry. Boyle gave the first precise definition: "No body is a true principle or element . . . which is not perfectly homogeneous but is further resolvable into any number of distinct substances how small so ever." This insight into the nature of elements unfortunately was unable to furnish him with techniques that could decide in any but a few cases whether a given substance was or was not an element; Boyle's criterion remained inapplicable for another 100 years. Finally, of course, Boyle's definition and the work of the next few centuries resulted in that great monument of classification, Mendeleev's periodic table of the elements, in 1869. This appeared to be a final step in classification for a time, but then came the discovery that the atom was not after all indivisible, and since then we have had a whole shower of long-lived elementary particles and antiparticles, as well as resonances, isobars, and excited states—so much so that few except professional physicists can find their way about among the fermions and bosons, the leptons, baryons and mesons, the nucleons and hyperons and the neutrinos, neutrettos, muons, lambdas, sigmas, pions, kaons, and so on and so forth—not forgetting the quarks! Obviously another classification was required, and now that we have the theory of *unitary symmetry* known as SU(3) we have gone some way toward achieving a more satisfactory state, particularly since the discovery of the omego-minus particle has seemed to verify the principles on which the theory of unitary symmetry was based. Modern as all these recent advances may seem, many of them had been foreseen already by Newton, who had evolved a theory of the atom composed of a shell within a shell of parts held together successively more firmly. All these anticipations of future developments by Boyle and Newton were of little use in the development of chemistry because, as Bernal (1969) points out: "In the seventeenth century chemistry was not yet in a state in which the corpuscular analysis could be applied. For that it needed the steady accumulation of new experimental facts that was to come in the next century. Chemistry, unlike physics, demands a multiplicity of experiences and does not contain self-evident principles. Without principles it must remain an 'occult' science depending on real but inexplicable mysteries."

This is an important limitation which applies to psychology just as much as it did to chemistry. The cry is often heard for a Newton to rescue us from the avalanche of facts, and to remedy the lack of self-evident principles in psychology. Yet even Newton, who worked at chemistry for much longer than he worked at physics, did not in fact succeed in advancing that science to any particular degree. Both in the matter of classification and in the matter of the creation of a genuine science of psychology we simply have to live within our means, and realize the bounds set by the nature of the material to the development of the laws we would all like to see develop.

A fourth point of criticism often relates to the accuracy of measurement, contrasting unfavorably the precision of measurement in the physical sciences with that achieved in psychology. It is true that certain measures in physics are extremely accurate. Thus the measurement of time is now accurate to a second in a million years, using the Caesium Time Base at Rugby. Even more recent advances, using "ion traps" to measure time, have improved accuracy from one part in 10^{13} to one part in 10^{15}; at the National Physical Laboratory, the element ytterbium is used as a standard for optical transition methods. But of course this accuracy was achieved only after 2,000 years of constant improvement, using originally devices like the sun dial, or the hourglass in which sand or water ran through a narrow opening at a more or less even rate. (The rate of course was not even because pressure varies with the amount of water or sand in the upper compartment.) Accuracy of measurement of IQ tests does not compare badly with the accuracy of measurement of time intervals prior to the invention of mechanical devices, and Galileo's demonstration of the laws governing the pendulum (Bernal, 1969). A similar lengthy period of development from very primitive types of measurement attended the use of scales to measure weight (Kisch, 1965), the measurement of temperature (Baker, Ryder, & Baker, 1975) and the measurement of mass and length (Feather, 1959). Accurate measurement is the outcome of a long period of evolution, in which practice, theory, measurement, and invention interact in a complex manner to improve accuracy.

It is in any case quite wrong to imagine that all measurements in physics approach the accuracy of the measurement of time. Measurement of the cosmological constant, for instance, has given constantly changing results over the past 50 years, and even now resists accurate determination. To take another quite fundamental measurement, we may look at the half-life of the neutron, which has proved notoriously difficult for physicists to measure. This is an important quantity for both particle-physicists and cosmologists. The former need to know this quantity accurately, because it allows them to determine the so-called "coupling constants" of the weak force of nuclear measure, while the latter need to know because accurate knowledge would allow them to determine the proportion of neutrons and protons that existed soon after the Big Bang. In 1951, the best estimate available of the half-life was 768 seconds, with an error margin of 150 seconds. Recent measures suggest a duration of 615 seconds, a difference from former estimates even beyond the error margin suggested originally!

To take another example, concerning errors in radio-carbon dating, recent studies have shown that errors with this technique may be two to three times as great as practitioners of the technique had claimed previously. Here, as in IQ measurement, there are many unaccounted-for sources of error that occur during the processing and analysis of samples. These are more realistic examples of the fact that all measurement involves error, and that the error, even in physics and astronomy, can be very large indeed. It is not the size of the error that determines whether a measurement is scientific; we could never undertake any scientific measurement if this measurement had to be accurate from the beginning within very narrow limits. What is important is to be able to have some estimate of the *size* of the error variance and some ideas about the *factors* that affect measurement to make it less accurate than it ought to be. On all these grounds measurement can be remarkably accurate under appropriate conditions—even in psychology.

It is important to emphasize the qualification contained in the last sentence, because a fifth objection often made relates to the practical application of IQ measurement and the errors that frequently occur. The use of IQ tests for more practical purposes should not be confused with its use as a scientific measure in experimental studies. The practical application is often constrained by financial considerations, administration is often by untrained personnel, and interpretation is often undertaken by nonpsychologists. Furthermore, tests are often chosen for reasons that have little to do with the accuracy of IQ measurement, but relate to the practical purposes of the investigator. Many so-called IQ tests are really measures for the prediction of scholastic achievement, and combine items of verbal and cultural knowledge with items more properly designed to measure g_f. This may be reasonable from the point of view of the administration, but such a test is not a proper IQ test, and the measurement of IQ should not be criticized because such tests fall short of ideal requirements.

But, and this is a sixth objection frequently raised, is it not true that there are many different types of IQ tests, and that these do not always give identical results? This is true, but equally there are many different types of measures of temperature, and these also do not give identical results. There is for instance a mercury-in-glass thermometer depending on the change in volume of the mercury with increase in heat; the constant-volume gas thermometer, depending on the reactance of the welded junction of two fine wires; resistance thermometers, depending on the relation between resistance and temperature; and thermocouples, depending on the setting up of currents by a pair of metals with their junctions at different temperatures. Nelkon and Parker (1965), in their Advanced Level Physics, point out that temperature scales differ from one another, "that no one of them is any more 'true' than any other, and that our choice of which to adopt is arbitrary, though it may be decided by convenience" (p. 186). Thus when a mercury-in-glass thermometer reads 300°C, a platinum resistance thermometer in the same place and at the same time will read 291°C! There is no meaning attached to the question of which of these two values is "correct" any more than to the question of whether an IQ of 120 on the Wechsler Scale is more "true" than an IQ of 125 on the Raven's Matrices!

One further objection may require a brief answer. It is often said that the ordinary measurement of IQ disregards important aspects of human life, such as creativity. That is true, in one sense, but it makes the assumption that creativity is essentially a cognitive variable. The empirical evidence seems to suggest, however, that creativity is a function of personality variables, particularly psychoticism, interacting with cognitive variables, namely IQ (Eysenck, 1983). For great achievement, high IQ is required, but high IQ does not necessarily lead to creativity. A certain element of psychoticism seems to be required, as shown both in real-life studies of highly gifted artists, and in experimental studies using traditional creativity tests. The objection, therefore, does not seem to be a serious criticism of IQ testing.

Biometric Intelligence: A Problem in Taxonomy

All sciences have a dual problem, in that they are concerned with both *taxonomy* and *causal analysis*. Taxonomy or classification usually precedes attempts at causal analysis. Classification of animals preceded Darwin's theory of evolution, to take but one classical example. Without taxonomy, causal analysis is difficult if not impossible. Of course there is no absolute distinction; there is an interaction, in the sense that advances in the causal analysis will help taxonomy, and vice versa. But in essence there is a very important difference, and unfortunately this difference has been neglected far too much by psychologists working in the fields of intelligence.

Classification is thus one of the classic methods of science and is fundamental in all fields of study. This is equally true in biology as in physics. Systems of classification are always at first simple, governed by common-sense appearances, and far removed from the complexities of later developments. Thus, Thales, the first of the Greek philosophers to think about the constitution of the world and its elements, held the theory that everything was originally water, from which earth, air, and living things were later separated out. Later on Anaximander and Anaximenes modified this hypothesis to include earth, air, and fire as well as water as the main elements. These of course were mere prescientific guesses of little value in the actual development of chemistry and physics, but at least they served to pose a problem.

More fruitful was an approach that appears to have originated with the Chinese. In chemistry we are dealing with a fundamental duality which is exemplified by metals and nonmetals; this we now know to be due to a shortage or excess of electrons. There is evidence for tracing the first appreciation of this duality to the Chinese, who already in prehistoric times used red cinnebar as a magic substitute for life blood and had resolved it into its elements, sulphur and mercury. From these notions the Taoist sect developed a system of alchemy from which it is probable that first Indian and then Arabic alchemy was derived. To these two opposites of sulphur and mercury a third element was added by Philipus Aureolus Theophrastus Bombastus von Hohenheim, who called himself Paracelsus to show his superiority to Celsus, the great doctor of antiquity. By adding the neutral *salt* he established the so-called *tria prima* as a foundation of his "spagyric" art of chemistry (Bernal, 1969).

Curious as these ancient methods of classification seem to us yet there is good modern justification for this spagyric system of mercury, sulphur, and salt. We have here a reasonable prevision of three of the four subfields into which the general field of chemistry is now subdivided: that of the rare gases, where all electrons remain attached to atoms; that of metals, where there is an excess of electrons; that of nonmetals, where there is a lack of electrons; and that of salts, where exchanges have taken place between the metal and the nonmetal ions. Even the analogy from external appearance on which the spagyric art was originally based has now found an explanation in terms of quantum theory.

There are certain important lessons to be learned from this brief excursion into ancient chemical history. One of them is that progress in classification is ultimately dependent on, and in turn central to, general development of the science of which it forms a part. Another important idea is this: The principles of classification based on analogies from external appearance may incorporate very important insights without which the development of a science would be very much slower, although of course it is not suggested that we should rest content with arguments from external appearances.

Psychologists who work in the field of classification, whether that of normal or abnormal personality or of intelligence, seldom concern themselves with the history of classification in physics and chemistry. This may be explained in terms of the obvious differences between animate and inanimate matter. However, they also very rarely seem to show any interest in the history of biological classification or *taxonomy*, and this is rather more difficult to understand because most of the problems that occur in psychology have also been dealt with by biologists and botanists at various stages, and a knowledge of their experiences may be of considerable use in dealing with our own problems.

This is not to say that biological taxonomy has been an unqualified success, or has failed to develop problems of its own. Consider the following quotation from Singer (1959):

> We would stress the fact that, from the time of Linnaeus to our own, a weak point in biological science has been the absence of any quantitative meaning in our classificatory terms. What is a class, and does class A differ from class B as much as class C differs from class D? The question can be put for the other classificatory grades, such as order, family, genus and species. In no case can it be answered fully, and in most cases it cannot be answered at all . . . until some adequate reply can be given to such questions as these, our classificatory schemes can never be satisfactory or natural. They can be little better than mnemonics—mere skeletons or frames on which we hang somewhat disconnected fragments of knowledge. Evolutionary doctrine, which has been at the back of all classificatory systems of the last century, has provided no real answer to these difficulties. Geology has given a fragmentary answer here and there. But to sketch the manner in which the various groups of living things arose is a very different thing from ascribing any quantitative value to those groups.

Similarly, Sokal and Sneath (1963) in their classic book on *Principles of Numerical Taxonomy* have this to say:

> It is widely acknowledged that the science of taxonomy is one of the most neglected disciplines in biology. Although new developments are continually being made in techniques for studying living creatures, in finding new characters, in describing new organisms, and in revising the systematics of previously known organisms, little work has been directed towards the conceptual basis of classification—that is, taxonomy in the restricted sense of the theory of classification. Indeed, the taxonomy of today is but little advanced from that of a hundred, or even two hundred, years ago. Biologists have amassed a wealth of material, both of museum specimens and of new taxonomic characters, but they have had little success in improving their power of digesting this material. The practice of taxonomy has remained intuitive and commonly inarticulate, an art rather than a science.

Sokal and Sneath give the following definition of classification: "Classification is the ordering of organisms into groups (or sets) on the basis of their relationships, that is, of their associations by continuity, similarity, or both." They go on to point out that there may be confusion over the term "relationship." As they say, "This may imply relationship by ancestry, or it may simply indicate the overall similarity as judged by the characters of the organisms without any implication as to their relationship by ancestry." The second of these meanings is the one they prefer, and they give it the special name of "phenetic relationship," using this term to indicate that relationship is judged from the phenotype of the organism and not from its phylogeny. In psychology too there is an important distinction corresponding to this, although the alternative to a phenetic relationship is not one based on ancestry but one based on genotypic consideration. We shall take up this point in some detail later on.

In setting up systems of classification we may follow one of two alternative routes named by Sneath (1962) "polythetic" and "monothetic" (from *poly*: "many," *mono*: "one," *thetos*: "arrangement"). As Sokal and Sneath point out:

> The ruling idea of monothetic groups is that they are formed by rigid and successive logical divisions so that the position of a unique set of features is both sufficient and necessary for membership in the group thus defined. They are called monothetic because the defining set of features is unique. Any monothetic system (such as that of Maccacaro, 1958, or in ecology that of Williams and Lambert, 1959) will always carry the risk of serious misclassification if we wish to make natural phenetic groups. This is because an organism which happens to be aberrant in the feature used to make the primary division will inevitably be removed to a category far from the required position, even if it is identical with its natural congeners in every other feature. The disadvantage of monothetic groups is that they do not yield "natural" taxa, except by lucky choice of the feature used for division. The advantage of monothetic groups is that keys and hierarchies are readily made.

Sokal and Sneath go on to list the advantages of polythetic arrangements. Such arrangements, they say, "place together organisms that have the greatest number of shared features, and no single feature is essential to group membership or is sufficient to make an organism a member of the group." They credit Adamson (1727–1806) with the introduction of the polythetic type of system into biology. He rejected the a priori assumptions of the importance of different characters; he correctly realized that natural taxa are based on the concept of "affinity"—which is measured by taking all characters into consideration—and that the taxa are separated from each other by means of correlated features.

It is important to realize that the distinction between polythetic and monothetic methods of classification has important consequences for our definition of intelligence, and our search for a means of adequate measurement. A *monothetic* approach would be that of defining intelligence a priori in terms of learning, or problem solving, or memory, or inductive reasoning; by adopting such a definition, and only using tests of that character, we would arbitrarily prejudge the issue and make it impossible to ever arrive at a more complex and more decisive definition and measurement of intelligence. *Polythetic* methods are indicated and, as we shall see, these imply the use of correlational and factorial analyses.

The analysis by *phenetic relationship* which had become all but universal in biology received a setback when *analysis by relation through ancestry* was reinstated after the publication of *The Origin of Species*. Suddenly Darwin's theory seemed to suggest the basis for the existence of natural systematic categories: Their members were related because of descent from a common ancestry. Unfortunately, history has shown that this enthusiasm could only be short-lived; we cannot make use of phylogeny for classification since in the vast majority of cases phylogenies are unknown. Inviting as the argument from ancestry may appear, therefore, in its Darwinian guise, nevertheless it has to be rejected for reasons given in detail by Hennig (1957), Remane (1956), and Simpson (1961), as well as in *Principles of Numerical Taxonomy* by Sokal and Sneath already quoted.

An exciting recent development has led to the construction of phylogenetic trees by biochemists, who use quantitative estimates of variance between species as regards substances such as DNA and cytochrome c. Fitch and Margoliash (1967), for instance, have succeeded in constructing such a tree, based on data relating to the single gene that codes for cytochrome c, which is very similar to the ''classical'' phylogenic tree. The method is based essentially on the appropriate ''mutation distances'' between two cytochromes, which is defined as the minimal number of component nucleotides that would need to be altered in order for the gene for one cytochrome to code for the other. This number is considered proportional to the number of mutations that have taken place in the descent from the apex of one cytochrome as compared with another. Thus, it is claimed that this new method, which gives a quantitative measure of the event (mutation) which permits the evolution of new species, must give the most accurate of phylogenetic trees. In this way it may be possible to overcome the difficulties in the evolutionary method of classification by descent noted above; it is reassuring that even when based only on a single gene the phylogenetic scheme is remarkably like that obtained by classical methods.

How in fact does a biologist proceed? Sneath (1962) has set the procedures out according to the following four steps:

1. The organisms are chosen, and their characters are recorded in a table.
2. Each organism is compared with every other and their overall resemblance is estimated as indicated by all the characters. This yields a new table, a table of similarities.
3. The organisms are now sorted into groups on the basis of their mutual similarities. Like organisms are brought next to like, and separated from unlike, and these groups or *phenons* are taken to represent the ''natural'' taxonomic groups whose relationships can be represented in numerical form.
4. The characters can now be reexamined to find those that are most constant within the groups that have emerged from the analysis. These can be used as diagnostic characters in keys for identifying specimens.

The last paragraph will make apparent the relevance of this discussion to the study of intelligence. We are faced with a very large number of behaviors, measured by means of tests, questionnaires, observations, or experiments. It is obviously impossible to build separate concepts on each of these variables, and we are faced with the problem of taxonomy. Translating the prescription given by Sneath in the above paragraph, but tracing his steps into the field of psychological measurement, we would say:

1. The tests are chosen, and their characters are recorded in a table.
2. Each test is compared with every other, and their overall resemblance is estimated (by means of correlation coefficients).
3. The tests are now sorted into groups on the basis of their mutual similarities. Like tests are brought next to like, and separated from unlike; in these groups all factors are taken to represent the "natural" taxonomic groups where relationships can be represented in numerical form. Factor analysis is the preferred method to carry out this step.
4. The tests can now be reexamined to find those most constant within the factors that have emerged from the analysis. These can be used as diagnostic characters for identifying abilities. Factor analysts have frequently been criticized for using a methodology that is unlike anything in the natural sciences. Our rather roundabout discussion has been undertaken to indicate that such an accusation is not in fact accurate, and that in taxonomy psychologists who use factor analysis are simply following the identical path that has been prescribed for them by experts in the biological field. The taxonomic analysis of the cognitive field begun by Spearman in 1904, and continued by Thurstone, Thomson, Cattell, Guilford, Vernon, and many others has certainly brought a great deal of clarification into this field, and has helped us to a meaningful classification of mental tests.

I have discussed the outcome of this taxonomic effort many times (Eysenck, 1992), and will not do so again here except to summarize the major agreements:

1. The most important finding is that all cognitive tests correlate positively together, to create what is often called the "positive manifold."
2. The first and the most important factor to emerge in the correlations between any variegated set of tests is the general factor of intelligence or *g*. (Tests differ in their *g* loadings, indicating that some are better measures of intelligence than others.)
3. The nature of tests with high as opposed to low *g* loadings enable us to formulate and test hypotheses concerning the nature of intelligence.
4. In addition to *g*, all tests measure factors specific to each test.
5. In addition to *g*, and specific factors, each measurement carries with it an error factor, as indeed do all scientific measurements.
6. Tests which are similar in content (i.e., verbal, numerical, visual-spatial, memory, etc.) define group factors or primary abilities which are indepen-

dent of *g*. We have no choice but to attempt to postulate and test the importance of such factors.

7. Estimates of *g* are remarkably stable across different batteries of mental tests, even when batteries consist of as few as nine tests. Thorndike (1987) demonstrated this by making up six short nonoverlapping batteries of nine tests each. The tests in each battery were randomly sampled from a large pool of extremely diverse cognitive tests used by the U.S. Air Force, including a great variety of tests from discrimination reaction-time to vocabulary. Seventeen highly diverse "probe" tests were interlocked one at a time into each battery, and the average correlation of the *g* loadings of the 17 probe tests across the six batteries calculated; it turns out to be .85. *g* emerges with a high degree of robustness and consistency for mental test batteries of a very varied character which in this case were for the most part not even good tests of *g*.
8. The prescription that the *g* tests in a battery should be as varied as possible is not very precise, but we now have enough evidence available to enable us to follow this prescription with considerable accuracy. This means that *g* factors obtained from different test batteries can be considered as a statistical estimate of a *true g*, a distinction made by measurement theory between an *obtained* measurement and a *true* measurement. We can estimate the degree to which an obtained measure of *g* approximates a true measure by using a formula given by Kaiser (1968). This indicates that if we determine a *g* from a sample of 20 tests correlating only to a degree of .20, the resulting measure of *g* would have a validity of .91.

The major result of such taxonomic studies is a hierarchical structure much like Figure 1.2, which is taken from the work of Jäger (1967) and his colleagues (Jäger & Tesch–Römer, 1988; Jäger & Hörmann, 1981). Unlike Guilford's (1967) model of the intellect, Jager incorporates the vital *g* factor in his model, which has much greater empirical support than Guilford's.

It is always possible in taxonomic work to argue for alternative methods of classification, if only because causal derivation is difficult or impossible, and because the reasons for classification may be varied. Thus to the biologist the whale may be a mammal, but to the Ministry of Agriculture and Fisheries it may be a fish, for obvious reasons. Hence, there have been many attempts to deny the existence of *g*, and to suggest complex patterns of intercorrelated primaries, or even independent primaries (Guilford, 1967). Improbable as these alternative suggestions are, they are not always mathematically impossible, as it is clearly feasible to rotate factors in any manner whatsoever, thus giving an infinite number of possible solutions. However, as Thurstone (1947) was the first to point out, there are certain preferred solutions (simple structure) which, when they occur in a clear-cut manner, ought to be given preference. This suggestion has been widely accepted, but it is clearly not a mathematical absolute, and may be disregarded if analysts want to do so.

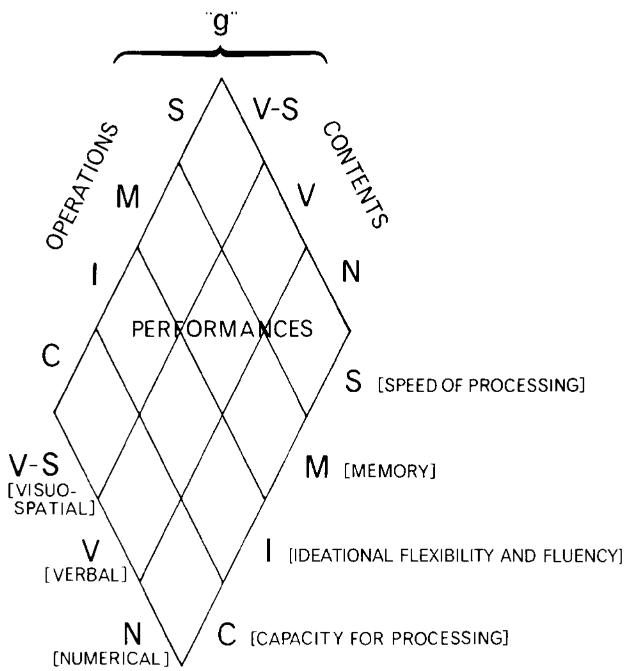

Figure 1.2. The hierarchical structure of intellect (Jager et al., 1988).

This slight degree of subjectivity in taxonomy in general, and factor analysis in particular, makes it necessary to look for causal factors in order to obtain a more universal agreement. There are of course many other reasons for looking at causal factors, and indeed even the earlier workers like Spearman and Thomson attempted to set up theories which might explain the observed phenomena. Thus Spearman (1927) suggested some form of energy as a causal factor for differences in *g*, while Thomson (1939) favored a theory of "bonds," a theory that has been fairly decisively disproved (Eysenck, 1987a). If we take seriously the notion of these rather divergent forms of intelligence suggested in Figure 1.2, then clearly we must look for a causal factor in the biological field, as indeed Galton had already suggested. It is to this search that we now turn. Before doing so, however, it may be useful to point out that most writers looking for a causal theory have adopted good measures of *g* as criteria for such a theory. With all its faults, the psychometric analysis of intelligence has given us very solid results, and has given us excellent measures of *g*; any causal theory that does not account for the psychometric results we have obtained in the past would clearly not be acceptable. Thus it is reasonable to regard *g* as our criterion for judging the adequacy of any biological theory.

The Biological Basis of Intelligence

The major outcome of the taxonomic investigation into the concept of intelligence results in a hierarchical model specifying four types of factors. By far the most important is a general factor, followed by group factors, followed by specific factors, followed by error factors. The nature of a general factor, whether determined by confirmatory factor analysis (Gustafsson, 1984) or multidimensional scaling (Snow, Kyllonan, & Marshalek, 1984) is most closely defined by tests of g_f, such as Raven's Matrices; g_c tests appear at the lower level. This alone should be sufficient to disprove the widely held belief that IQ measures are simply measures of educational achievement and verbal knowledge, a belief still widespread in spite of the strong evidence against it (Sternberg, 1982; Wolman, 1985). But as previously pointed out, taxonomic arguments are impossible to make definitive, and it is usually possible by making arbitrary assumptions of one kind or another to come to a desired conclusion. More impressive are direct tests that require specific theories and experimental studies directed toward a causal analysis of the phenomenon. It is only in recent years that efforts have been made in that direction.

There have been two major lines of attack. The first of these relates to the implementation of the suggestion by Galton, to the effect that reaction times might be a fairly direct measure of biological intelligence. This suggests, and should be supplemented by a theory, that speed of mental processing may be a major causal factor in producing differences in IQ (Eysenck, 1967). The literature has been reviewed by Eysenck (1987b), and more recent advances discussed in other chapters in this book. Here I only summarize the major findings as far as these are relevant to our problem.

1. Measures of DT (decision time) correlate negatively with g.
2. Measures of MT (movement time) correlate negatively with g.
3. Measures of *variability* of DT correlate negatively with g.
4. The more complex the stimulus for RT, the higher the correlation with g, as long as total RT is below 1000 millisecs. Simple RT has quite low correlations with g, choice RT somewhat higher ones, complex RT, like the odd-man-out paradigm (Frearson & Eysenck, 1986), have the highest.
5. Multiple correlations between different RT measures and g are much higher than individual measures, and can be in excess of .70.
6. The correlation between IQ measures and RT is not mediated by speeded IQ tests, but applies equally to so-called power tests.

All these findings, replicated many times over, favor some sort of "speed of mental processing" theory, except number 3 which cannot easily be accommodated by such a theory. There is, of course, a contingency relation between speed and variability of RT (great variability implies the presence of long as well as short RTs, and this precludes very low RTS on the average), but the contingency is such that RTs should correlate higher, rather than lower, with *g*, as compared with variability. We will return to this anomaly in connection with an alternative theory later on.

Of equal interest and importance as work on RT has been the study of IT (inspection time) (see Eysenck, 1986, and the symposium following this reference). Here, importance attaches to *speed of perception* rather than *speed of reaction*, and the evidence may be summarized by saying that there are correlations averaging around .5 between IT and *g*. It is not yet known whether *variability* of IT is highly correlated with *g*, but clearly this is an important question requiring elucidation. However that may be, IT is an important contributor to any R^2 involving measures of DT and MT.

It is unfortunate that most experimenters have used the traditional stimulus in IT studies (comparing a long with a short line); it seems reasonable to expect that a slightly more complex stimulus would correlate more highly with IQ. Thus we might ask subjects to compare two circles, containing different numbers of dots, the task being to identify the circle containing the most dots. Provided the task was easy enough for even retardates to do successfully, if given enough time, and did not last for more than 300 msec. to 500 msec. for average IQ subjects to do, it does seem likely that correlations with IQ exceeding 0.50 would be obtained. Systematic variation of stimuli should in any case throw much light on the mechanics of the phenomenon in question. Correlations between different versions of the IT paradigm could also be used to calculate multiple correlations. A factor analysis of different DT, MT, and IT test scores would be an important contribution to the IQ literature.

A "speed of mental processing" theory would predict most of the results actually found. Cognitive processing must begin with perception (IT), go on to central processing of the information gained (DT), and finally issue in some form of action (MT). The main reasons the mental chronometry involved is relevant to IQ have been spelled out by Jensen (1982a, 1982b).

Essentially, it has been well established in cognitive psychology that the conscious brain acts as a one-channel or *limited capacity* information processing system. As such, it can deal simultaneously only with a very limited amount of information, and this limited capacity also restricts the number of operations that can be performed simultaneously. Speediness of mental processing is advantageous in that more operations per unit of time can be executed without overloading the system. Such operations may involve information entering the system from external stimuli, or from retrieval of information stored in short-term or long-term memory (STM or LTM).

Another advantage is that there is rapid decay of stimulus traces and information, so that there is a clear advantage to speediness of any operations that must be performed on the information while it is still available. Other advantages involve the fact that in order to compensate for limited capacity and rapid decay of incoming information, individuals resort to rehearsal and storage of the information into intermediate or long-term memory, which has relatively unlimited capacity. But this process itself takes time, and therefore uses a general capacity, involving a tradeoff between the storage and the processing of incoming information. Total amount stored and processed is limited by the speed with which these acts are accomplished.

We thus have a fairly coherent theory of speed of mental processing underlying essentially varied accomplishments of *g*. This theory, and the facts on which it is based, are quite incompatible with Binet-type theories emphasizing education, scholastic knowledge, and similar achievements as basic to our conception of intelligence. Inspection time, decision time, and movement time in response to extremely simple stimuli are obviously highly related to differences in *g*-loaded tests, but they cannot be regarded as in any sense measures of crystallized ability, of school learning, or similar types of achievement. The tests are quite novel for practically all subjects, requiring no former knowledge of any kind, and the tasks involved are so simple that even low retardates can carry them out *given enough time*. Yet multiple correlations between tests of this kind and IQ tests are almost as high as are correlations between different IQ tests. This is a fact that requires explanation, and it is difficult to see how one can arrive at such an explanation in terms of orthodox theories emphasizing learning and educational achievement.

It could be argued, and it has been argued, that perhaps reaction and inspection time experiments do not give us a direct insight into brain function. If this is true, different forms of EEG measurement may be used to gain some more insight into the psychophysiology of intelligence (Eysenck, 1982; Eysenck & Barrett, 1985). The study of the EEG itself has proved relatively disappointing, until recently, when computer methods of analysis became available. Gasser and his associates (Gasser, Lucadon-Müller, Verleger, & Bacher, 1983; Gasser, Mocks, Lenard, Bacher, & Verleger, 1983; Gasser, Mocks, & Bacher, 1983) have been most successful in demonstrating that correlations of the order of .5 can be obtained in this field, using variables the choice of which was predicted in terms of a genuine theory. However, most work has been done in relation to evoked potentials, following the early work of Ertl (1973) and Ertl and Schafer (1969). These studies have been extensively reviewed elsewhere (Eysenck & Barrett, 1985) and Eysenck (1986b). The essential breakthrough occurred when the Hendricksons (A. E. & D. E. Hendrickson, 1980; A. E. Hendrickson, 1982; D. E. Hendrickson, 1982) put forward a novel theory to account for existing facts, and predict novel ones. Based on a physiological theory of information processing through the cortex, the Hendricksons argued that individuals with neuronal circuitry that can best maintain the integrity of stimuli will form accessible memories faster than those individuals whose circuitry is more "noisy." In addition, for individuals of low neural integrity, it will be impossible to acquire complex or lengthy information, as the total information content can never be stored in a meaningful way, and no accessible memory can be formed. The integrity of neuronal circuitry is essentially dependent on errorless information processing; the more errors occur (possibly at the synapse) the "noisier" will be the circuitry. IQ, on this hypothesis, should be a function of the integrity of the circuitry, or the absence of errors; the fewer errors, the higher the IQ.

Two measures were derived on the basis of this reasoning, which should correlate with psychometric test intelligence scores, given that such test performance is related to neural transmission integrity. The first measure would be the *complexity* of the waveform, assessed by measuring the contour perimeter of the AEP waveform, a measure originally called the "string" measure, after an early way of measuring this contour perimeter. The second measure would be the *variance* at each point across a number of stimulus waveform epochs. The more intelligent the individual, the longer the contour, and the lower the variance. These two measures would be expected to correlate reasonably well, since they both derive from the same fundamental property of errors in transmission. We thus have a rational measure that can be objectively quantified and correlated with intelligence.

The results of a large-scale study of 219 schoolchildren, using the WAIS as a measure of IQ, gave very positive results which are shown in Table 1.1. The correlations among the WAIS IQ and string, variance, and composite AEP measures are .72, −.72, and −.83, respectively. These data are impressive, but

Table 1.1. Relationship between the EEG Measures and the WAIS Subtests

WAIS test	*Variance*	*String*	*Variance minus string*	*Full WAIS IQ (current study)*	*Full WAIS IQ (published data)*
Information	−.64	.55	−.68	.80	.84
Comprehension	−.50	.53	−.59	.74	.72
Arithmetic	−.57	.56	−.65	.79	.70
Similarities	−.69	.54	−.71	.84	.80
Digit span	−.54	.49	−.59	.71	.61
Vocabulary	−.57	.62	−.68	.79	.83
Verb total	−.69	.68	−.78	.95	.96
Digit symbol	−.28	.32	−.35	.45	.68
Picture completion	−.47	.52	−.57	.67	.74
Block design	−.50	.45	−.54	.70	.72
Picture arrangement	−.36	.45	−.46	.54	.68
Object assembly	−.32	.45	−.44	.55	.65
Peformance total	−.53	.53	−.60	.69	.93
WAIS total	−.72	.72	−.83	1.00	1.00

Note: From *A Model for Intelligence* (p. 205) by H. J. Eysenck, 1982, New York: Springer. Copyright 1982 by H. J. Eysenck. Reprinted by permission.

even more important is a calculation reported by Eysenck and Barrett (1985). What is claimed in the Hendrickson theory is that the combined (variance minus string) measure of AEP is a physiological *cause* of differences in IQ. A factor analysis was carried out, using the 11 WAIS scales and the composite AEP score; only one general factor was extracted to represent, in a direct form, the *g* factor common to all the tests. On this factor, the AEP measure has a loading of .77. We argued that if the general factor obtained from the intercorrelations between all the subtests of the Wechsler is our best index of intelligence, and if the AEP composite measure represents a good measure of intelligence, so defined, then we would expect the factor loadings on the 11 WAIS subtests and the correlations of the subtests with the AEP composite measure to be proportional. Using measures uncorrected and corrected for attenuation, we found that as far as the correlation between factor loadings and composite measure are concerned, the correction makes little difference; rho is .95 for the uncorrected values and .93 for the corrected values. Proportionality, therefore, is almost perfect and strongly supports the view that the AEP is a true measure of intelligence.

The Hendrickson paradigm, which has been replicated successfully several times, is not the only one in the field. Another is the Schafer paradigm (Schafer, 1982). On the basis of well-established facts, he argued that there is a modulation of AEPs, manifested as a tendency for unexpected or "attended" stimuli to produce AEPs of larger overall amplitude, compared with those generated using stimuli, the nature and timing of which is known by the individual. Schafer has extended the scope of this empirical phenomenon, hypothesizing that individual differences in the modulation of amplitude (cognitive neuroadaptability) will relate to individual differences in intelligence. The physiological basis of this relationship is hypothesized to be neural energy as defined by the number of neurons firing in response to a stimulus. A functionally efficient brain will use fewer neurons to process a known stimulus, whereas for a new, unexpected stimulus, the brain will commit large numbers of neurons. This theory has received good support, with correlations with IQ ranging into the eighties.

It is interesting to note that Schafer's hypothesis and results can be explained in terms of the Hendricksons' theory. Processing errors would be expected to delay recognition of repetition essential to adaptation; hence, the loss of AEP amplitude with repetition (adaptation) would be less in low IQ than in high IQ subjects. The evidence suggests this is indeed so, and that the two hypotheses make similar predictions.

Also successful has been a theory of Robinson (1982), which is based on a complex theoretical analysis of the role of the diffuse thalamocortical system, believed to act as a mediator of Pavlovian excitation. The theory is too complex to be reviewed here, but it has given results that again show the dependence of IQ measures on cortical events.

We now seem to have two hypotheses furnishing us with causal theories relating to differences in IQ. The first is the speed of information processing theory, the second the integrity of circuitry hypothesis. It may be suggested that the results leading to the former theory may be explained even better by the second theory; in other words, speed of processing is a function of circuitry integrity. The argument may be developed along these lines. It is well known that information is not processed along one channel, but along a large number, and Sokolov (1960) has argued for the existence of a comparator which acts to assess the incoming information and give the signal for the start of a reaction. If the incoming information is incongruent, due to errors of information processing, the comparator will have to wait for more information to come in, thus delaying the process of reaction. Thus, speed of reaction is essentially a function of errorless processing of information. It would be difficult to reverse the argument; errorless processing cannot be explained in terms of speed of processing.

Even more important is a consideration of the facts that cannot be explained in terms of speed of mental processing, particularly the importance of variability in RT experiments. This is analogous to the variability in AEP experiments, and can easily find the same explanation in terms of errors of processing. It is not argued that the theory is necessarily correct, but merely that it seems to explain all the available facts in a reasonable manner, and generates predictions that can be tested; no more can we ask of any theory.

The Hendricksons argued that the locus of the transmission errors would be the synapse, but recent unpublished evidence from our laboratory seems to negate that hypothesis. Barrett, Daum, and Eysenck (1990) studied the speed of transmission in the ulnar nerve, and while not finding any correlation between IQ and speed, we did find a highly significant negative correlation between variability of transmission speed and IQ. As there are of course no synapses involved, it must be some other property of the neuromechanism that is responsible. Clearly the whole theory is in a very early stage of development and will require much detailed experimental work to make it more specific.

The fact that the positive results of Hendrickson (1982) and Blinkhorn and Hendrickson (1982) have been replicated several times (Haier, Robinson, Braden & Williams, 1983; Robinson, Haier, Braden & Krengel, 1984; Caryl & Fraser, 1985; Stough, Nettelbeck, & Cooper, 1990) is impressive, but two points deserve mention. The first is that while positive overall results have been reported, there are marked differences in particular findings. Thus, Blinkhorn and Hendrickson (1982) found significant correlations only for the Matrices test, but not for verbal tests; Hendrickson (1982) found higher correlations for verbal than for nonverbal tests. Stough et al. (1990) found significant correlations only for verbal and nonverbal Wechsler scales, not for the Matrices test. These and other discrepancies may be due to the very variegated choice of tests, populations, stimuli, and methodologies used by different investigators; this variety makes the positiveness appear particularly promising (positive results can be obtained almost regardless of changing conditions) suggesting considerable *robustness* for the paradigm. But contradictory findings, for example, that there is a significant relationship between IQ and the N140-P200 amplitude (Haier et al., 1983; Robinson et al., 1984) or that there is not (Stough et al., 1990) require some explanation. Clearly a more theory-oriented approach is required, with special attention paid to paradoxical results like those mentioned.

A second point to be stressed is the suggestion that different periods of the AEP may be differentially related to IQ, as shown by Stough et al. (1990). As they point out, "that correlations vary from .38 to .86 when measured over different durations of time suggests that there may be different events occurring at different but precise times, with each resulting in different effects on the string length–IQ correlation. If this is the case, then future research will need to break the string lengths into smaller components (especially within the lengths 100–200 msec.) so that underlying processes can be isolated." To which may be added the suggestion that brain stem evoked potentials may be of particular importance theoretically; they have been found in some unreported studies to have quite high correlations with IQ.

One unfortunate feature of all this work is that most of the studies have relied on small and unrepresentative samples (with the honorable exception of the Hendrickson study). Correlational analyses require hundreds of subjects in order to give manageable standard errors. Restricted range samples (e.g., students) are easily available, but corrections are of doubtful value unless samples are very large indeed—with small samples errors multiply. These are all diseases of early childhood, but they do make more difficult a proper understanding and interpretation of the results obtained thus far.

An important aspect of biological intelligence often neglected is the biochemistry of *g* (Weiss, 1986). This is concerned with glucose and its uptake by the brain; as is well known, glucose is an almost exclusive source of energy as far as the brain is concerned. De Leon et al. (1983), Sinet, Lejenne, and Jerome (1979), Soininen, Jolkkonen, Reinihainen, Halonen, and Riekkinen (1983), and others have shown interesting relations, often quite close, between IQ and glucose uptake. This is an important area deserving attention and development, and which is discussed in Chapter 7 of this book.

DISCUSSION

It will be clear why we may regard the recent work on the physiology of intelligence as producing a revolution in both theory and measurement of intelligence (Eysenck, 1983). Whether we accept the particular theories discussed in this chapter or not, it is clear that the results are quite incompatible with traditional theories of intelligence, and that something new is required, more in line with Galton's original theories than with Binet's.

One interesting and important consequence that would follow from the theory would be that if we seek to improve IQ, it is unlikely to be accomplished by educational and other similar methods; the poor effects of the Head Start program are, of course, well known. One obvious way of influencing the brain directly is by vitamin and mineral supplementation, and Benton and Roberts (1988) have recently shown that such supplementation, comparing the therapy group with a control group, resulted in a significant increase in g_f, but not in g_c, just as would be expected on a biological hypothesis. Similar results are being reported from the United States (Schoenthaler et al., 1986, 1991), suggesting that increases in IQ of between 10 and 20 points can be obtained even in children not obviously undernourished. These are important consequences of a biological theory of intelligence (Eysenck & Eysenck, 1991).

For a proper appreciation of this new model, a detailed consideration of the empirical evidence is of course required, and the other chapters in this book are devoted to such a consideration. The present chapter was intended to present theoretical backgrounds of these recent developments, and present them in a theoretical setting, to emphasize their importance for a better understanding of the concept of intelligence. Just as the concept of the atom has changed drastically over the past 100 years, so the concept of intelligence has been changing, and will no doubt continue to change. Such change does not mean that the concept is scientifically valueless; quite the opposite. It is only if a concept remains stationary that it loses interest; new discoveries will constantly produce changes in our conceptions of the Universe and our place in it, and there are large numbers of new empirical findings that need to be tested and brought together in order to improve our conception of intelligence. No doubt the next few years will continue to provide us with many problems and, we hope, with some solutions as well.

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