

Why History of Psychology?

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There has, in recent years, been a growth of interest in the history of psychology. There was the founding of Cheiron, an International Society for the Study of the History of Psychology, and National Societies have currently been founded in the United States, in England, and now in Germany. There are Journals devoted to the behavioural sciences in the United States, the history of Psychology in Spain, and fitful efforts promulgate the topic in other countries. Ever since Boring produced his history of experimental psychology, others have followed in his footsteps, and the topic is being taught in special courses at American Universities and elsewhere. Whence does this interest come, and why are we concerned with the history of psychology? And can we really talk about the history of a science which only recently celebrated its 100th Anniversary? And what do we mean by "history" in this context?

It is often said that psychology has a long past, but a short history. What is meant by this, of course, is that man has been concerned with psychological problems since the dawn of history, and priests, medical men, politicians and many others have tried to solve psychological problems concerned with education, personality, mental disorder, motivation, ability and emotion for thousands of years hence the long past. Psychology as a science, however, using theoretical models, control groups, the hypothetico-deductive method, statistical estimates of probability and mathematical models, only goes back a very few years - hence the "short history". It may help to realize how short this history is when I tell you that I have personally known and, talked to such figures as Kohler and Piaget, Kretschmer and Thurstone, Spearman and Pieron, Luria and Tolman, Skinner and Boring, Koffka and Haldane, Burt and Bartlett, Donald Hebb and Gordon Allport - eminent figures many of whom have in turn been in contact with the founders of our science, men like Wundt, Pavlov, Galton and Kraepelin. Thus one might say that people like myself constitute the third generation of psychologists in the history of psychology as science - a very short period of time to study in the name of history!

Perhaps we are wrong in contrasting the long past and the short history in this categorical manner. Perhaps it might be wiser to look upon the development of psychology rather than as a continuum, just as a history of physics, or astronomy, or chemistry is a continuum. All these sciences began with common sense observations, continued in a mishmash of original ideas, superstitions and new discoveries, before becoming organized into what we now recognize as the hard sciences. Before achieving this end, however, all sciences had to undergo an ordeal by quackery - astronomy had to separate itself from astrology, Chemistry from alchemy, and so forth. Psychology is still fighting, as a science to separate itself from such a pseudo-sciences as psychoanalysis, existentialism and other, similar movements and disciplines. This is a hard battle, and until it is won, we will always be, as William James said, the hope of a science, rather than a science.

It is reasonable to compare psychology with the hard sciences? Those who adhere to the hermeneutic point of view would doubt it, suggesting that for many reasons human behaviour is not subject to the ordinary rules of science. The hard sciences are "cumulative", a term that has at least two meanings in the scientific enterprise. In the broadest sense scientific results are cumulative if empirical laws and theoretical structures build on one another so that later developments extend and unify earlier work. This idea has been called "conceptual or theoretical cumulateness" (Hedges, 1987). A narrower and less subjective indicator of cumulateness is the degree of agreement among replicated experiments or the degree to which related experimental results fit into a simple pattern that makes conceptual sense. This might be called "empirical cumulateness" (Hedges, 1987). Hedges has compared the empirical cumulateness of psychological research with that of research in the physical sciences, and his data suggest that the differences may be less striking than previously imagined.

The most common quantitative method used in reviews in the physical sciences involves the use of weighted least squares (weighted regression). In the physical sciences, the most influential techniques are those introduced by Birge (1932), which have become standard to the physical sciences. Hedges (1987) compared the outcome of replications of experiments in the physical and social sciences, and found that "about 45% of the reviews in both domains exhibit a statistically significant disagreement where no studies were omitted from the reviews. In both domains deletion of data from some studies substantially improved the empirical consistency of the research." (p. 453.) In other words, empirical cumulateness was present as much

in the data from psychology as it was in the data from physics. This finding has many important consequences, and we will return to it at the end of this discussion. We may note here, however, that the data do not suggest a sharp division between the hard and the soft sciences.

What, then, is the connection between the past of psychology, and its history? If there is a continuum, how does it operate? We may say, briefly, that there are two kinds of psychology, just as there are two kinds of physics. Everyone known, from experience, a great deal about the way the laws of physics operate. We manage to walk and jump, to throw and catch, to kick and run, avoiding obstacles, using and countering gravitational forces, with an accuracy that is almost incredible. A computer, its memory loaded with Newtonian theories and empirical constants, would find it quite difficult to predict exactly what vectors to impress on a tennis ball in order to hit the corner of the service court with a speed of over 100 miles an hour, a feat regularly achieved by the leading tennis players without any benefit of knowledge of physics! If we did not have knowledge of this kind, life could not go on at all, and scientific physics has grown out of this common knowledge by imperceptible steps over the centuries.

In a similar way, the man in the street known from experience a great deal about himself and other people, about emotion and ability, motivation and memory, learning and forgetting. This knowledge is acquired in the course of living, and is necessary for successful living, and it is clear that some people are much better at acquiring such knowledge than are others. Psychology as a science has grown out of this common reservoir of psychological knowledge, gradually systematizing it through the efforts of philosophers, teachers, medical men, and many others. We will see that early workers in this field have anticipated some at least of our latest and proudest developments, so that it does make good sense to talk about a continuum, rather than an abrupt change from past to history.

If we now ask people Why they study the history of psychology, the first and most obvious answer is simply general interest. As mountaineers climbing Mount Everest have usually answered questions about why they do it by saying: "Because it is there!", so many psychologists answer the question about why they study history simply by saying, in essence: "Because it is there!". Our science has developed over the centuries, and there is an intrinsic interest in following these developments, possibly in the hope of learning something for the future from studying the past. If, as Dionysus of Halicarnassus said: "History is

philosophy teaching by example", then it may well be useful for us to look at these examples, and see what they can teach us .

It is also interesting to meet kindred spirits who have faced the same problems we face, and to see how they coped with these problems. If, as Thomas Carlyle said: "The history of the world is but the biography of great men", then we may well benefit from meeting these great men, at least through their writings, and try to assimilate what they have to teach. It would be an error to imagine that men like Hippocrates and Galen, Kant and Hume, Descartes and Spinoza had less insight into the human condition than we do, and while they did not use what we now consider to be scientific methods, their observations were full of insight and, as we can now prove, correct in many important ways. Thus a general interest in the history of psychology is understandable and indeed obvious, but there is many more specific reasons why we should devote more effort than we do at present to study our past.

The first of these reasons is simply that we may learn from the ideas and theories introduced many years ago to order our own ideas better. Let us consider as an example recent work on the differentiation between traits and states, as in the case for instance of the Spielberger Trait Anxiety and State Anxiety Scales. This is an important notion, which is often attributed either to R.B. Cattell or to C. Spielberger. Actually in its Modern form it is implicit in my hierarchical Model of personality (Eysenck, 1947, p. 28-30). I there describe four levels at which we can study personality, beginning at the bottom with the specific response level. Where specific responses are reliably correlated over time, we have the habitual response level. Different types of habitual responses combine into traits, and different traits into types, such as introversion-extraversion, or neuroticism-stability. Clearly state descriptions of personality refer to the specific response level, trait descriptions refer to the habitual response level. What I call "traits are based on the intercorrelations between different habitual responses, as types are based on the correlations between such traits .

However, I have no wish to claim priority in respect of the state-trait distinction. It was already explicitly developed by Cicero (1927) over 2,000 years ago. Having defined the concept of disease, and having compared physical and mental diseases, he goes on to say the following. (I have used my own translation, as the official ones are rather clumsy.)

"We now come to the analogy of health, and make use finally of this comparison with disease. Some men are more prone to some diseases, other men to others, and consequently we say of certain people that they are liable to catch cold, while others are liable to attacks of colic. We do not say this because they are suffering at the moment from these disorders, but because they frequently do so. In the same way some men are prone to fear, others to some other disorder, and as a consequence we speak in some cases of an anxious-temper (anxietas), and hence of anxious people. In other cases We might speak of irritability (iracundia), which is different from anger (ira). It is one thing to be irritable, another thing to be angry, just as an anxious temper (anxiety) is different from feeling anxious (anger). Not all men who are at times anxious are of an anxious temperament, nor are those who have an anxious temperament always feeling anxious. In the same way there is a difference between intoxication and habitual drunkness, and it is one thing to be a gallant and another thing to be in love. (p. 354.)

Here we have a clear and very explicit differentiation of state anxiety (angor) and trait anxiety (anxietas), both defined in exactly the same way as Cattell, or Spielberger, or I would define these concepts. Cicero goes on to discuss these concepts, as well as that of bodily and mental disease, in some detail; there is no need to go any further to indicate how much psychology would have benefited if those who dealt with the concepts of anxiety, neuroticism, etc., and constructed questionnaires accordingly, had made this distinction from the beginning, and had been aware of Cicero's distinction between state and trait.

Consider, as another example, Lykken's (1981) recent Sock on the lie detector: "A Tremor in the Blood". You will find a detailed description of the theory and method involved in lie detections in a book written by Daniel Defoe (1730) over 250 years ago. This is what Defoe has to say: "Guilt carries Fear always about with it; there is a Tremor in the Blood of a Thief, that, if attended to, would effectually discover him; and if charged as a suspicious Fellow, on that Suspicion only I would always feel his Pulse, and I would recommend it to Practice. The innocent Man which knows himself clear and has no Surprise upon him; when they cry, Stop Thief, he does not start; or strive to get out of the Way; much less does he tremble and shake, change Countenance or look pale, and less still does he run for it and endeavour to escape.

It is true some are so harden'd in Crime that they will boldly hold their faces to it, carry it off with an Air of Contempt and outface even a

Pursuer; but take hold of his Wrist and feel his Pulse, there you shall find his Guilt; a fluttering Heart, an unequal Pulse, a sudden Palpitation shall evidently confess he is the Man in spite of a bold Countenance or a false Tongue: This they cannot conceal; 'tis in vain to counterfeit there; a conscious Heart will discover itself by a faltering Pulse; the greatest Stock of Brass in the Face cannot hide it. or the most Firm Resolution of a harden'd Offender conceal and cover it: The Experiment perhaps has not been try'd, and some may think it is not a fair Way, even with a Thief, because 'tis making the Man an Evidence against himself: As for that, I shall not enter into the Enquiry farther than this; if it is agreeable to Justice to apprehend a Man upon Suspicion, if the Particulars are probable and well grounded; it cannot then be unlawful by any Stratagem that is not injurious in itself, to seek out collateral Grounds of Suspicion, and see how one thing concurs with another.

It may be true, that this Discovery by the Pulsation of the Blood cannot be brought to Certainty, and therefore it is not to be brought into Evidence; but I insist, if it be duly and skillfully observ'd, it may be brought to be allow'd for a just Addition to other Circumstances, especially if concurring with other just Grounds of Suspicion. "

Or consider the following clear anticipation of Pavlovian conditioning by Lope de Vega, taken from his book: "El Capellan de la Virgen". "Saint Ildefonso used to scold me and punish me lots of times. He would sit me on the bare floor and make me eat with the cats of the monastery. Their cats were such rascals that they took advantage of my penitence. They drove me mad stealing my choicest morsels. It did no good to chase them away. But I found a way of coping with the beasts in order to enjoy my meals when I was being punished, I put them all in a sack, and on a pitch black night took them out under an arch. First I would cough and then immediately Whale the daylights out of the cats. They whined and shrieked like an infernal pipe organ. I would pause for awhile and repeat the operation - first a cough, and then a thrashing. I finally noticed that even without beating them, the beasts moaned and yelped like the very devil whenever I coughed. I then let them loose. Thereafter, whenever I had to eat off the floor, I would cast a look around. If an animal approached my food, all I had to do was to cough, and how that cat did scat! "

These are rather insubstantial cases, but a more important one perhaps is the anticipation of Skinner's "Token Economy" by Alexandar Maconochie (Berry, 1958 ; Eysenck, 1972). Maconochie was appointed Superintendent of Norfolk Island, one of the most cruel and soul-

Another example of the end

destroying of all the Australian convict settlements, in 1840, He was horrified by the prevailing cruelty of punishment, and began to introduce his 'mark system of prison discipline'. His purpose was rehabilitation of the prisoners; as he put it: "Vice is a disease and penal science just moral surgery. The means it employs must often be painful; but its object should always be benevolent - always the speedy discharge of the patient."

In order to achieve this end, Maconochie proposed the substitution of a task rather than a time sentence; instead of being sentenced to imprisonment for a period of time, the offender should be sentenced to be imprisoned until he had performed a specified quantity of labour. To specify and quantify this amount of labour is of course difficult; Maconochie suggested that the prisoner should be ordered to earn by labour and other forms of good conduct a fixed number of 'marks of commendation', and thus his period of detention should end only when he had done so. On first entering the prison, the offender would suffer a short period of restraint and deprivation; this would shortly be followed by a second stage during which he could earn privileges, as well as shelter and food, by the earnings from his labour and good conduct. Purchases could be made by computing the value of the goods in 'marks', and setting them off against the 'marks' earned by the prisoner. As Barry puts it, 'the performance of allotted tasks would enable him to earn a daily tally of marks, for example, ten marks, but by frugal living, constant industry beyond the allotted task, and exemplary behaviour and demeanor, he could add to the daily tally. Disciplinary offences should not be punished by the customary prison methods of violence, deprivation, or enforced labour, but by fines expressed in marks, and by the withdrawal of privileges.'

In due course, prisoners were permitted to join with other prisoners, and engage in the performance of joint work projects in which misconduct of one member is punished by loss of marks by the whole group. 'As a prisoner progressed through the system, the restraints upon him should be lessened, and the final period of his detention should resemble as much as possible the conditions likely to be encountered on release, the expressed purpose of this stage being to prepare him for the release which the whole system was devised to enable him to achieve by his own efforts. The fundamental principle was: nothing for nothing; everything must be earned. Throughout the period of detention anything that tended to degrade the prisoner, or to deprive him of the character of a "social being" should be avoided. Brutal punishments, such as the use of leg irons, the wearing of chains,

"spreadeagling", the gag and the lash, should not be used. These ideas are not entirely original; Richard Whately, Archbishop of Dublin, and the Quakers James Backhouse and George Walker had already suggested some quantitative system of withdrawal of privileges as a substitute for the use of punishment; Maconochie was the first to put them into a usable form, and try them out in an actual experiment.

Was the experiment successful? It is difficult to judge at this distance of time, but there is a good deal of evidence to suggest that it was surprisingly successful, and the rehabilitation by means of such a "token economy" as implemented by Maconochie is possible for most criminals. For evidence, the accounts by Barry or Eysenck may be consulted; here our interest is not so much in the success of the method, but in its anticipation of the much later work of Skinner, Ayllon and Azrin; there is little in the Ayllon and Azrin (1968) book that is not foreshadowed in Maconochie's work. It is interesting to speculate how much the rehabilitation of prisoners might have benefited from work along the lines suggested by Maconochie! Equally, social psychology and our study of the laws of learning and motivation might have benefited considerably had psychologists known about his work.

Allied to the possibility of learning from the past is a third reason for looking at history, namely to decide on questions of priority, and to assess the novelty and value of contributions. To some extent this is bound up with the points already discussed; Cicero clearly anticipated modern advocates of state-trait distinction, and Maconochie anticipated Skinner and others in the design of token economies. Consider now the widely adopted division Freud made between the id, the ego and the super ego. These refer respectively to the instinctive and motivational aspects of human behaviour, those concerned with self interest and adaptation, along rational lines, and those related to the moral, ethical and social teachings of religion, and other social institutions. Clearly this is a valuable distinction, but it goes back for over 2,000 years to Plato whose fable of the charioteer and his two horses, the Bad, impulsive run-away horse, and the good obedient horse has been widely quoted. The charioteer corresponds to Freud's ego, trying to hold the balance between the instinctive forces of the id (the bad horse) and the social demands and moral imperatives imposed by society (the good horse). Here, as so often, what is new in Freud isn't true, and what is true isn't new; the valuable aspects of his distinction go back to Plato; the implications Freud added, however, have not been found to be of any special value.

Another example of the odd way in which we attribute priority, without recognizing the claims of antiquity, relates to Freudian symbolism, i.e. the use of sharp, pointed objects to symbolize the male genitals, and curved objects and containers the female genitals. But of course there is nothing specifically Freudian in this symbolism, which has been universally known for thousands of years.

An J.N. Adams has pointed out in his book "The Latin Sexual Vocabulary" , "no objects are more readily likened to the penis than sharp instruments, and it is likely that metaphors from this romantic field abound in all languages". In Latin, symbolic terms to denote the penis are, for instance *virga* (rod), *vectis* (stake), *hasta* (lance), *rutabulum* (rake, poker), *terminus* (boundary marker), *temo* (pole), *vomer* (plough), *clavus* (tiller, as a nautical metaphor) . Many other examples are given by Adams, and he also points out that "the snake was felt to have phallic significance by "Latin-speakers", so even here Freud did not add anything new.

The vulgar term for the female genitals, *cunus*, is on a par with *mentula* and is hardly used outside graffiti and epigrams. However, metaphors abound. Adams says: 'The frequency (in Latin and other languages) of the metaphor of the field, garden, meadow, etc. , applied to the female *pubenda* reflects in part the external appearance of the organ, and in part the association felt between the fertility of the field and that of females. The metaphor complements the verbal metaphors of sowing and ploughing used as the male role in sexual intercourse."

Another example is the concept of the unconscious, which is often attributed to Freud. Yet, as Whyte (1962) has shown, there were at least 200 philosophers and thinkers before Freud who explicitly recognized the existence of unconscious mentation. But Freud added again, specific theories which were not true; this specifically Freudian unconsciousness has by now been given up even by many of his followers, and plays little part in modern psychoanalytic theories. I have discussed all the issues involved in great detail elsewhere (Eysenck, 1985) and will not do so again here.

Let me now look at another advantage of studying history, namely the interest which comes from tracing the development of a concept, or a theory, through the years. In my book on *Reminiscence, Motivation and Personality* (Eysenck and Frith, 1977), I was particularly intrigued to see how the some ideas and theories developed quite independently in Germany and the English-speaking countries. *Reminiscence*, of course,

is a phenomenon in learning theory in which there is little improvement during the period of learning, but a great deal of improvement during a rest period following that learning; reminiscence is defined as the increment in performance found from the end of the learning period to the beginning of the post-rest performance, although some authors prefer to make certain slight adjustments to these data. In the English-speaking literature, the discovery of the phenomenon is usually credited to Ballard (1913), and a theoretical interpretation in terms of dissipation of inhibition during rest credited to the general theory of Hull, and the specific application to the phenomenon by Ammons, Kimble and others. Yet the phenomenon is clearly and explicitly described and defined by Oehrle (1895), a pupil of Kraepelin, and in this and the following issues of *Psychologische Arbeiten* will be found a theoretical account which clearly anticipates Hull and his followers, and contains essential elements to those postulated 50 years later! Our own work led us to believe that these theories were essentially wrong, and we substituted an entirely different type of theory, involving consolidation, a concept in turn put forward by Muller and Pilzecker (1900), almost simultaneously with Kraepelin's dissipation of inhibition theory. There is no time here to enter into the details of these developments, but there is a modern trend, particularly in Germany, to disregard earlier theoretical and experimental work done in this country and attribute greater importance to more recent American studies which essentially copy and imitate the earlier German work.

Another area in which I have worked myself, and have traced the developments of the concepts involved for over 2,500 years, is the area of personality known as extraversion-introversion. A brief outline of this history can be found elsewhere (Eysenck 1973; Eysenck & Eysenck, 1985). We begin with the doctrine of the four temperaments, developed by Hippocrates and later on by Galen. Immanuel Kant made these four temperaments the basis of his theory of personality, and gave descriptions which ring as true today as they did then. Wundt took up the development and suggested that the four temperaments should be looked upon as the four quadrants of a circle in which the major axes were labelled emotional-unemotional (now called neuroticism-stability), and unchangeable-changeable (now identified as introversion-extraversion). Correlational and factor analytic studies, begun by Heymans in Holland at the turn of the century, and continued by Spearman and his students, and later on by myself, verified in all its essential aspects the picture presented by Kant and Wundt.

Heymans was the first to make deductions from the theory of extraversion-introversion, and test these deductions by means of laboratory experiments. Otto Gross, followed later by William MacDougall, suggested anticipations of the arousal theory which I was to put forward many years later, based then on physiological developments coming years after the prescient suggestions of Cross and MacDougall. Thus a theory which I have suggested to constitute a paradigm in personality research (Eysenck & Eysenck, 1985) shows a systematic development from its indistinct beginnings 2,500 years ago to a more highly developed state now. It would be difficult to say in This development where the past ends and the history begins!

But development is not always as straightforward as this, and science often shown signs of pendulum movement as in the case of our conceptions of light. Originally suggested to be wave-like by Huyghens, Newton found it to be rather like the movements of particles, and his theory survived until the beginnings of the 19th century, when Young, Fresnel, and others demonstrated the unquestionable wave like nature of light. We now know that light has the qualities both of waves and particles, thus arresting the swing of the pendulum in the middle between both extremes.

In psychology there has been a similar swing of the pendulum where the relative contributions of heredity and environment to psychological qualities are concerned. Popular stress on genetic factors was followed by Locke and Rousseau in their doctrine of extreme environmentalism. In the 19th century, Darwin and Spencer directed attention again to genetic factors, but another swing of the pendulum in the years between the World Wars created an atmosphere in which again more interest was paid to environmental factors, a trend which became overwhelmingly strong in the post-Hitlerian period. The mixture of the swing of the pendulum and political ideology, favouring the doctrines of equality, caused an almost complete cessation of work on genetic causes of intelligence and personality.

As I have pointed out elsewhere (Eysenck, 1982), there has been a tendency to equate a denial of genetic determinance with left-wing, socialist and communist ideology, and a stress on genetic factors with right-wing and Fascist ideology. Nothing could be further from the truth.

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 & 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 in 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).

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, which 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.

In recent years, much work has been carried on in the Soviet Union, in Poland and in the DDR to indicate that genetic factors are as powerful, if not more so, in these countries than they are in democratic

countries. Heritabilities over 80% have been found in intelligence tests in twin studies in Moscow, and other studies reviewed by Eysenck (1982), and Friedrich & Kabat vel Job (1986) show that in all essentials there is agreement about the genetic basis of personality and intelligence between the democratic capitalist countries and the socialist Soviet countries . Thus here too the pendulum has come to rest between unworkable extremes, Here too, then, the history of development in psychology is in many ways similar to the history of development in physics .

Let us now look at another reason for studying the history of psychology. It is said that problems are originally raised in philosophy which are later on solved by science, and while no doubt only partly true , there is some reason to suspect that for psychology in particular the connections with philosophy are closer than many experimentalists think. Consider as one example the problem of the body-mind relation, so powerfully raised by Descartes 300 years ago. Many psychologists have followed him in declaring the two "substances" of mind and matter to be completely different, so that there would be no real interaction between them. Idealists have come to regard matter as unreal; materialists have come to regard mind as unreal, whereas interactionists have doubted the proper separation of mind and matter, and believe that both exist and interact. The evidence now seems fairly convincing in suggesting that the interactionists are essentially right in their submission. Just as physicists have had to give up their idea that time and space are entirely different aspects of physical reality, and have come round to adopting a space-time continuum, so psychologists have had to give up the notion of a complete separation between body and mind, and will have to make do with a body-mind continuum. I will quote just one recent example to illustrate the point, and link it with a statement of the theory made 4,000 years ago by Mahabharata, an Indian wise man concerned with the nature of disease. This is what he said: "There are two classes of disease - bodily and mental. Each arises from the other. Neither exist without the other, Mental disorders arise from physical ones and like wise physical disorders arise from mental ones. " Recent work carried out by Dr. Grossarth-Maticek and myself in Heidelberg has demonstrated clearly how true this is, and how even the more serious medical disorders, such as cancer and coronary heart disease, are vitally dependent on psychological factors, and can be prevented by suitable applications of behaviour therapy (Eysenck, 1987a, b).

In these studies, three large samples of males and females were interviewed, given tests of personality, given information concerning smoking, drinking etc., and were then followed up over a period varying from 10 to 13 years. Population 1 consisted of elderly people selected at random and resident in a small Yugoslav town. The second sample consisted of a younger population, randomly chosen within given age and sex composition limits, resident in Heidelberg. The third group, similar to the second in age and sex composition, consisted of individuals who were suffering from psychological stress. The members of the third group, also resident in Heidelberg, were nominated by members of the second group, who chose those among their relatives and friends whom they knew to be suffering from psychological stress. It was postulated that there existed a cancer-prone and a coronary heart disease prone type, showing certain personality characteristics tapped by the questionnaires and inventories used .

The following were the major findings from this series of investigations .

- 1) In all three groups, cancer-prone probands, as defined in terms of their questionnaire scores died very significantly more frequently from cancer than from coronary heart disease.
- 2) Coronary heart disease-prone probands, as defined in terms of their personality scores, died very significantly more frequently from coronary heart disease than from cancer .
- 3) Probands not belonging to either the cancer prone or the coronary heart disease-prone type suffered very few deaths from either cancer or coronary heart disease.
- 4) Prediction of death from cancer or coronary heart disease was very much more accurate when made on the basis of personality and when made on the Basis of smoking, drinking and other external factors.
- 5) Stress produced an increase in mortality of about 40% over comparable , non-stressed probands .
- 6) Used prophylactically, cognitive behaviour therapy aiming to change the behaviour of cancer-prone and coronary heart disease-prone probands in the direction of normality was successful in preventing death from cancer or coronary heart disease in probands belonging to such therapy groups, as compared with probands belonging

to nontreated control groups. To take but one example, of 50 cancer-prone probands exposed to therapy, none died of cancer; of 50 cancer-prone probands in the control group, 16 died of cancer.

These data show a very strong interaction between personality and stress, on the one hand, and diseases like cancer and coronary heart disease on the other. This interaction is much stronger than that observed between smoking and these diseases, although the medical literature has played up the importance of smoking and has played down the importance of personality and stress. It should be noted, incidentally, that the relationship between personality and stress, on the one hand, and disease on the other is maintained when the effects of smoking are statistically controlled, and that smoking and personality demonstrate a synergistic effect, in the sense that the relationship between smoking and disease only exists among people predisposed by personality to develop cancer or coronary heart disease; in the psychologically healthy population there is no such relation.

Results such as these are incompatible with Cartesian statements about the relationship between body and mind, and strongly urge an interactionist view. These results bear out the theories about the relationship between personality and disease which were already articulated by Hippocrates and Galen, and have been reiterated repeatedly over the centuries by many physicians who reported keen observations on patients suffering from cancer or coronary heart disease. There is a close link between modern research and ancient history, and it has clearly repaid us to take seriously these early observations.

Up to now we have discussed some of the ways in which the studies of the history of psychology, and the reading of the works of the great men who contributed to this development, can help us in becoming better psychologists. I would like to suggest, however, that reading the history of psychology is not enough. If we want to develop psychology into a science, then it may well repay us also to extend our reading to the history and philosophy of science. Let us return to a point raised at the beginning of this discussion, namely that of cumulativeness in science. It was shown there that empirical cumulativeness as indexed by Birge's ratio, was not dissimilar when comparing the hard sciences with the soft sciences. However, much more important is what he called conceptual or theoretical cumulativeness. By this is meant that in the broadest sense scientific results are cumulative if empirical laws and theoretical structures build on one another so that later developments extend and

unify earlier work. This definition is simply a restatement of Kuhn's (1962) view of the importance of paradigms in science. Kuhn's views are too well known to need extensive restatement, but it is perhaps of interest in a discussion of the history of science that his views were anticipated to a large extent by a German physician, L. Fleck (1935, English translation 1979) in his book on the "Entstehung und Entwicklung einer Wissenschaftlichen Tatsache". An excellent application of Kuhn's theories to the social sciences is given by Barry Barnes (1982). Essentially, a paradigm is an agreed body of knowledge, incorporating also some existing problem-solutions or methods. This specific kind of consensus is the basis of normal science, the typical mode of operation of a scientific community, and the mode for which the training of the scientist serves as an appropriate preparation. The advent of normal science marks the coming of age of a scientific field, the point at which really effective, productive research begins. All recognized sciences have passed such a watershed, and it can be questioned whether those subjects which have yet to do so deserve to be called sciences at all (Kuhn, 1970, chapter 2).

Clearly, psychology has not yet passed this watershed. Just take, as a typical example, the area of personality study, and (consider the most successful text book of all, that by Hall & Lindzey (1985)). This clearly does not contain any agreement on subject matter, methodology, or results. What it gives is essentially a series of chapters based eponymously on theories proposed by different authors, ranging from Freud, Jung, and Adler through Henry Murray, Gordon Allport, and William Sheldon to such Modern writers as George Kelly, Raymond Cattell, and Albert Bandura. Not only is there no paradigm; there is no search for a paradigm, and no apparent recognition that such a paradigm would be desirable! Thus clearly Hall & Lindzey have learned nothing from the history of science; they do not recognize what is needed in the development of a science, and substitute a Dutch auction for the organic growth and development that is characteristic of a paradigm.

Personality of course is not the only part of psychology to suffer in this way. The growth and importance of schools is characteristic of learning theory as well of personality; the famous Hillgard & Marquis book on Conditioning and learning (Kimble, 1961) is written on the same principle, and shown the some weaknesses in this equally central area of psychology.

Let me return to the area of personality. I have argued (Eysenck & Eysenck, 1985) that there does exist the beginnings of a paradigm in

this field, but that its recognition is delayed by the fact that most people, failing to recognize the importance of constructing a paradigm, prefer to strike out along independent lines and construct more and more apparently new constructs and scales, without realizing that these are simply slight modifications of existing concepts and scales. Thus there is no theoretical cumulativeness, no aggregation of knowledge, and no paradigm.

An alternative to the present practice would be to accept what has been repeatedly proven, reject what has repeatedly shown to be wrong and attempt to integrate anomalous findings with a developing theory. Anomalous findings have always beset scientific theories; there are no good reasons for rejecting such theories. Newton it will be remembered, tried unsuccessfully to solve the three body problem, i.e. the relationship between sun, moon and earth, and failed to integrate the movements of the moon into his mathematical system. Many other anomalies appeared over the years, but nevertheless his system was perhaps the most successful ever developed in the hard sciences. Psychologists are prone to reject theories which are not perfect, just on the basis of a few anomalies, rather than attempt to find explanations for these anomalies in terms of the theory in question, or to make small changes in the theory when these are called for by anomalous results. This may be because psychologists have little knowledge of the history of science, or the way theories are developed. Thus the study of the history of science may be an important ingredient in the development of a psychologist into a scientist.

Another aspect of the development of a science which is related to the above is continuity in the development of a research programme. At the moment, what we have is an almost hysterical jumping on a bandwagon when a new concept is developed, a brief period of work on the concept, and then an almost total rejection when the concept is found to be less than 100% perfect. Examples are the interesting notion of Lewin's level of aspiration, Festinger's theory of cognitive dissonance, or Hull's system of learning theory. None of these deserves the fulsome praise heaped upon them at the beginning, nor the radical criticism and abandonment which mark their end. Indeed these and many other theories were abandoned just when they were beginning to show signs of consistent improvement and understanding; they were rejected for a lack of perfection which is inherently unlikely to be found in scientific theories. The value and importance of scientific theories is measured in shades of grey, not in black and white!

A study of the history and philosophy of science may therefore be useful for us in improving our own methods of work, our theorising, and our development of new concepts. We might add conversely, that a study of a history of psychology may be of value to historians and philosophers of science. The excellent histories of behaviour therapy by Kazdin (1970) and Schorr (1984), for instance, may serve as current and still developing examples of the creation of a new paradigm, and quite generally as an example of a revolution in science, in the sense intended by Kuhn (1962). To have available for interview and discussion many of those who took part in this revolution is an advantage possessed by few of the disciplines studied by historians of science, and it seems a pity that so little use has been made of it other than by psychologists themselves.

We may now sum up our analysis of why it is important that we should study the history of psychology. It is of intrinsic interest in itself, because it shows us how we came to be where we are. It contains important theories and conceptions which we may have to rediscover if we do not take the trouble to unearth them from history. It teaches us that what appeared to be novel and original ideas were in fact elaborated thousands of years ago. It enables us to trace the development of a theory, from early beginnings to later fruition. It enables us to discover the importance, and the disturbing role the *Zeitgeist* may play, and may thus teach us to avoid its influence. It may show us the close relationship between philosophical problems and Modern research, and the light each may throw on the other. extending our study to that of the history of science, we may learn what we as psychologists do wrong, and teach us how to avoid such errors. Last but not least, we may integrate what we can learn from the history of psychology with the philosophy and history of science; here our relative youth is a veritable advantage, because we can study history in the making. All of these are powerful reasons why we should study the history of psychology, and the history of science as well. As persons we are rooted in the history of our family, As citizens we are rooted in the history of our country. As scientists, we are part of the historical development which we would be foolish to ignore. I welcome the newborn interest shown in so many countries in the history of our science, and hope that this interest will grow and develop until every new recruit to the science of psychology becomes, acquainted with its history as causation.

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INTRODUCCION

Binet y Simon
experimentales
inseparable o
sobre L'intellect
correcciones
(1911a)

Según algunos de los pioneros, juntamente con la inteligencia y tanto con existiendo una similitud como es medida por la Spearman. Así la proximidad temporal mutuamente beneficiosa. Pero Oleron (195 llega a afirmar que Binet es el que introduce la psicología científica.

Para Claparède (1911), bajo su diversidad aparente, los múltiples trabajos de Binet forman los anillos de una cadena de inteligencia, que es, a fin de cuentas, lo que sondeaba Binet cuando estudiaba la fatiga, la fuerza muscular, la forma de escritura, la sugestionabilidad, etc.

Los conceptos de pensamiento e inteligencia se utilizan en este artículo, siguiendo a Binet, como sinónimos. El término pensamiento es, en efecto, polémico. Y así en el *Vocabulaire Technique et Critique de la*