The Social Application of Pavlovian Theories

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PAVLOV, AND TO A LESSER extent Bechterev, made three great contributions to psychology. It would probably not be an exaggeration to say that these contributions have been absolutely fundamental in turning psychology into a science, but only one of these has really caught on in the West, and formed the basis of a large body of research. The aspect of Pavlovian theory that has caught on has been the experimental study of conditional responses (CR) as part of learning theory, associated with the detailed physiologic analysis of these responses, the dependence on a variety of stimulus parameters, and the analysis of the types of responses observed. Research along these lines has been of a high quality, has been very fruitful, and represents a great achievement in the study of human learning.

There are, however, two other great areas of interest that Pavlov originated, and that have fared less well at the hands of his successors in the West. The first of these is the topic of *individual differences*. Pavlov was very impressed with the great differences found in the speed and strength of conditioning, the varying rates of extinction, and the many other individual differences to be observed in his canine subjects, and he attempted to base a whole theory of personality on his findings. The theory has been elaborated in the U.S.S.R. by Teplov, Nebylitsyn and many others; useful summaries of this work will be found in books by Gray (1972, 1973), Mangan (1982), Nebylitsyn and Gray (1972), and many others. Here let me note merely that Pavlov never averaged results over several dogs, but always reported results from single animals, on a particular occasion; he very carefully noted the particular temperament and past history of the dog in accounting for deviations from general rules and norms.

In this he was in line with the European tradition. As I have pointed out elsewhere (Eysenck and Frith 1977), at the turn of the century German psychologists like Kraepelin and Müller were very careful not to lose individuality of their subjects in averaged results; they always paid close attention to the personalities and the circumstances of life of their subjects at the time the tests were made and did not hesitate to relate personality to the empirical parameters of their experiments. American psychology has gained much by using advanced and sophisticated statistical methods, but it has also lost a great deal by concentrating on averages, and disregarding individual differences. Cronbach (1957) has argued in favor of reuniting the two disciplines of scientific psychology, by which he means the experimental approach, studying the effect of the independent on the dependent variable, and correlational psychology or psychometrics, dealing with individual differences. I have argued many times (Eysenck 1967 and 1981) that the main effects in analyis of variance are too often so weak as to account for only a small portion of the variance, while the error terms looms enormous. The reason for this, I would suggest, is simply that individual differences are relegated to the error terms; where they are properly studied, and where they form part of the original theory, the error term would be much smaller, and the interaction between main effects and individual differences would become subject to experimental control.

A third region of investigation that was close to the heart of Pavlov, but that has been very much neglected by American psychologists, has been the study of *social issues* and the degree to which social behavior is determined by Pavlovian conditioning. In his old age Pavlov became very interested in abnormal psychology, and put forward several hypotheses related to the possible connection between psychiatry and the theory of conditioning. Bechterev, too, was convinced that

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the answer to social problems lay in the study of conditional responses, and his evaluation of *objective psychology* as a basis for social action was probably even more pronounced than that of Pavlov. Yet if we read the current literature on conditioning there will be little mention of social effects, and a perusal of the literature in social psychology will reveal little by way of mention of conditional responses. Thus two out of the three great innovations pioneered by Pavlov have been greatly neglected by Western psychologists, much to our disadvantage.

Spence and his associates have tried to relate conditioning and anxiety, suggesting that anxiety as a drive multiplies with habit strength to produce stronger performance. I have reviewed the literature on these proposals elsewhere (Eysenck 1973) and will not do so again here; Spence's suggestion that anxiety or neuroticism, as a personality trait, might lead to stronger and more lasting conditional responses has led to a great deal of empirical work, and has been a useful staging post for the new development of later and more adequate theories. Hull, too, wrote about the importance of individual differences, suggesting that they should be measured in terms of the parameters of his system, but he never carried out any empirical work along these lines, and with the demise of his system his suggestions likewise ceased to be of interest. Skinner is one of the few who attempted to build a whole social psychology on the basis of the law of reinforcement, but his success has been partial, and he almost certainly grossly exaggerated the area over which this law holds sway. He is often quoted as denying the importance of individual differences in personality, and of the genetic factors responsible for these, but this is incorrect; at the debate between us held at the Montreal Annual General Meeting of the APA, Skinner explicitly agreed that individual differences and their genetic causes were of considerable importance in the study of human behavior. Yet in spite of this conviction he and his followers have not been tempted to carry out the necessary empirical studies that alone could give substance to these beliefs.

My own initial work on the relationship between personality and conditioning was based on two hypotheses. The first of these was that introverted people had stronger excitatory potential (in Pavlovian language) or cortical arousal (in modern language) than did extraverted people, who would be characterized by inhibitory potential or lack of cortical arousal (Eysenck 1957 and 1967). The second hypothesis, derived directly from Pavlov, might be phrased in the following way: the strength and duration of conditional responses are determined in part by the degree of cortical arousal of the subject. This general rule is, however, qualified by Pavlov's law of *transmarginal inhibition*, so that if cortical arousal (produced by intensity of stimulation) is greater than a certain optimal point, any further increase in the intensity of the unconditional stimulus would lead to a decline in the strength and duration of conditional responses.

The deduction was made (and verified in actual experimental studies) that with *suboptimal* strength of unconditional stimulus (US), introverts would condition *better* than extraverts, while with *supra-optimal* strengths of US extraverts would condition better than introverts. A study by Eysenck and Levey (1973) verified this prediction, and there is now a large literature on the relationship between extraversion and conditioning that suggests that these original hypotheses were in the right direction (Levey and Martin 1981).

At one point there appeared to be considerable disagreement between Spence and myself in relation to the personality factors most closely related to conditioning. Spence found in his work that neuroticism was positively correlated with conditioning, introversion was not; we found in our work that introversion was related to conditioning, neuroticism was not. The problem was resolved by a proper study of the parameters of the experiments conducted, respectively, by Spence and my own group. Spence produced a *maximum* amount of anxiety in his subjects by not reassuring them about possible electric shocks, by having all the apparatus exposed, by not explaining the purpose of the experiment; in our work exactly the opposite course was followed, and we aimed at a *minimum* degree of anxiety among our subjects. Thus differences in neuroticism or anxiety were maximized in Spence's set-up and minimized in ours; as a consequence neuroticism became an important variable for Spence, but was not found to be so by us. In my original presentation (Eysenck 1967) of the cortical arousal hypothesis, I had pointed out that autonomic activation can serve to increase cortical arousal, and this seems to have happened in the typical Spence-type experiment; when individuals suffer a high degree of anxiety, the resulting cortical arousal is so strong as to wipe out the many differences that might exist between extraverts and introverts. Hence the conclusion seems to be, not that Spence or I was right or wrong, but that predictable consequences follow from observed differences in the parameters of the experiments conducted by us.

Figures 1 and 2 show the rate of acquisition of conditioned eyeblink responses under conditions

theoretically favorable to introverts, *i.e.*, relatively weak USs (Figure 1), and under conditions relatively favorable to extraverts, *i.e.*, strong USs. The differences are of course all well beyond the 1% level of significance (Eysenck and Levey 1973).

Differences in personality determine not only the *frequency* of conditional responses, but also what Martin and Levey (1969) and Levey and Martin (1981) have called the "*work rate*," *i.e.*, the percentage of times that the conditioned response prevents the puff of air from actually reaching the eye. These two types of conditional response measures are statistically independent of each other, yet for both there is a significant difference favoring the introverts (Martin and Levey). Results for the frequency of CR are shown for one experiment in Figure 3, and for the work rate in Figure 4. It should be noted that the differences between extraverts and introverts are quite large in all these studies, although we have never chosen very extreme groups to exemplify extraverts and introverts. Usually we divide our sample at the mean, or else use extraverts and introverts who on the questionnaire occupy the top and the bottom thirds of the distribution. A greater degree of selection would inflate the differences to a considerable degree.

There are large numbers of experimental studies showing that cortical arousal is relevant to the results obtained (Eysenck 1967 and 1981); thus work on sensory thresholds, on vigilance, on physiologic responsiveness, on sensitivity to pain and sensory deprivation, on memory and learning, and many other experimental topics has shown personality, particularly extraversion–introversion to interact with experimental parameters to produce predictable results. I have argued elsewhere (Eysenck 1981) that experimental psychology, social psychology, educational psychology, industrial psychology, abnormal psychology, and indeed any aspect of psychology cannot truly become a scientific discipline unless it pays attention to individual differences and uses these within the general theoretic framework of a given experiment. If this is not done, entirely incorrect and nonreplicable results may be achieved. Three examples, briefly considered, must suffice to illustrate the point. Shigehisa and Symons (1973) looked at the literature on intersensory effects in the measurement of sensory thresholds; theory suggested that the perception of visual, auditory, tactile, pressure, pain, and olfactory stimuli could be facilitated by simultaneous *heteromodal* stimulation, but the evidence was quite inconclusive, with some studies favoring the hypothesis, others contradicting it, and yet others showing no effect

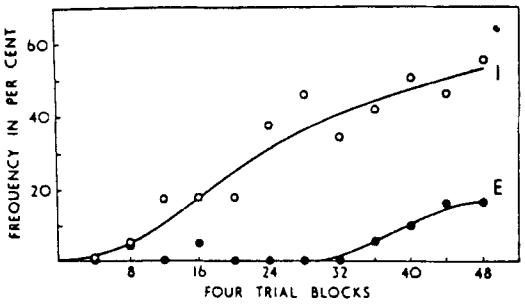

FIG. 1. Acquisition of eyeblink conditioned responses of introverts and extraverts on a condition theoretically considered favorable to introverts.

either way. Shigehisa and Symons suggested that these contradictory results might be due to the operation of the "law of inversion," *i.e.*, Pavlov's law of protective inhibition or transmarginal inhibition. They also suggested that an implication of this was that one must take personality into account, with introverts reaching the optimal point at a lower level of intensity than ambiverts, and ambiverts than extraverts.

Using varying intensities of light as heteromodal stimuli, they measured auditory thresholds and found results diagrammed in Figure 5; it will be seen that to begin with an *increase* in the intensity of heteromodal stimulation does indeed lead to lower auditory thresholds, but the law of inversion soon supervenes for introverts, later on for ambiverts, and not at all (within the limits of the experiment) for extraverts. Thus an increase in the intensity of light might lower the threshold, raise it, or leave it where it was before, depending entirely on the personality of the subject involved! Shigehisa and Symons replicated their work on other samples, and also reversed

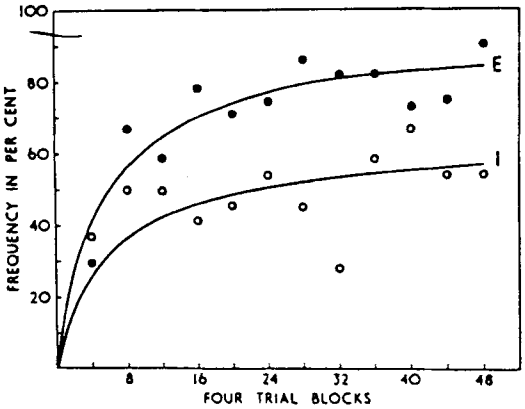

FIG. 2. Acquisition of eyeblink conditioned responses of introverts and extraverts on a condition theoretically considered favorable to extraverts.

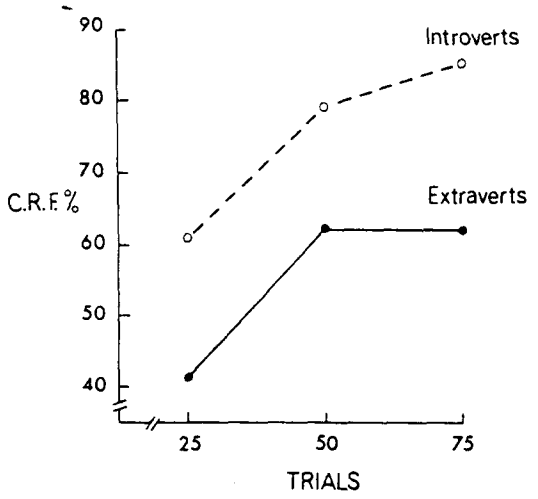

FIG. 3. Frequency of conditioned responses of introverts and extraverts in eyeblink conditioning trials.

the stimuli, using auditory stimulation as the heteromodal source and measuring the threshold for light stimulation, with similar results.

Howarth and Eysenck (1968) studied the effects of recall interval on recall of paired associates, predicting on the basis of Walker's theorem that extraverts, having low cortical arousal and hence poor memory consolidation, would do well in their recall a short time after learning, but then forgetting would set in soon and lower their scores dramatically. For introverts, whose high cortical arousal would facilitate consolidation, the opposite course was predicted; in other words, introverts were predicted to show *reminiscence* rather than forgetting. Figure 6 shows the results of the experiment, and it will be clear that introverts and extraverts show exactly contradictory effects of recall interval, which no amount of averaging could reconcile.

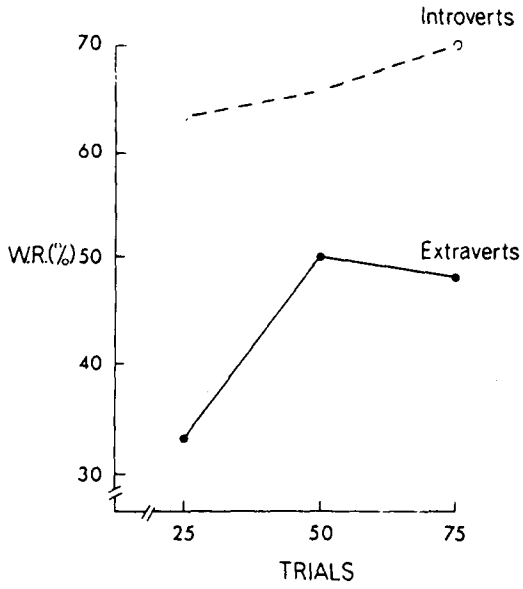

FIG. 4. Work ratio of introverts and extraverts in eyeblink conditioning trials.

Last, an experiment by Weisen (1965), who had his subjects sit in a dark and quiet room, systematically pushing a button. Once the operant level had been determined, contingent reinforcement was introduced by the subjects obtaining three seconds of bright lights and loud jazz music played on a juke box for pushing the button at a higher rate. As predicted, extraverts pushed harder to obtain the sensory stimulation, while introverts reduced their rate of pushing in order to avoid the extra stimulation (Figure 7). The experiment showed the opposite effect when the room was brightly lit and loud music was being played continuously; pushing the button more rapidly could obtain quiet and darkness. Under those conditions extraverts pushed less, introverts more than during the operant level period. The experiment illustrates the important point that studies of motivation must take personality into account; clearly the loud noise and bright music were *positive reinforcers* for extraverts, but constituted a *punishment* for introverts.

Figure 8 will illustrate the general theory in which predictions such as those for Weisen's experiment are based. Generally it is known that medium levels of sensory stimulation (and other collative qualities, to use Berlyne's term) have the highest positive hedonic tone, while very high and very low levels of stimulation, leading to pain or sensory deprivation, are usually associated with negative hedonic tone. The high arousal level of the introvert shifts his optimum level *below* that of the population average, and the low level of cortical arousal of the extravert shifts his level *above* that of the population average, as indicated in the diagram. This leads to the prediction that introverts would be more tolerant of sensory deprivation, extraverts of pain, and both these predictions have in fact been verified several times.

The general theory of individual differences I have put forward here as important links with Pavlovian theories advocated by Teplov, Nebylitsyn, and others in the U.S.S.R. In particular there appears to be a strong similarity between the notion of extraversion–introversion, and that of the strong *versus* weak nervous system in the U.S.S.R. For a review of these similarities and differences, Eysenck (1981) and Mangan (1982) may be consulted. It cannot be

assumed that these dimensions of personality are indeed identical, but some quite striking similarities have been found in empirical studies, and certainly the Pavlovian paternity would not be denied by those who have worked on the extraversion–introversion differences in the West!

We must now turn to the application of the principles and findings discussed so far to social behavior. I will concentrate on neurotic and criminial behavior, but will introduce the topic by reference to sexual attitudes and behaviors, because they illustrate certain principles particularly clearly and explicitly. Figure 8 suggests that extraverts require *higher* levels of stimulation, or stronger stimulants in order to reach what is to them the optimal point of stimulation, giving the highest hedonic tone. Introverts require a much lower level of sensory stimulation. Given that sexual stimulation produces considerable arousal, we would formulate the obvious deduction that extraverts would seek out such stimulation to a much larger extent than would introverts, or, to put it in another way, they would seek out more arousing types of sexual stimulation than introverts. This is one source of prediction to be made in this field.

The other source relates to a theory that will be discussed in more detail later on, namely that socialized behavior is based on the acquisition of a "conscience" on the part of individuals, and that the acquisition of this conscience is not a mysterious religious affair, but occurs in line with the laws of Pavlovian conditioning (Eysenck 1977). If we are willing to assume for the moment that this hypothesis is essentially correct, then it could be argued that introverts, acquiring conditional responses more readily than extraverts, would be more likely to act in conformity with socialized mores which discouraged many types of sexual behavior, such as premarital and extramarital activities, perverted conduct, etc.

Taking these two kinds of prediction together, Eysenck (1976) made the following seven major predictions: 1) extraverts will have intercourse *earlier* than introverts; 2) extraverts will have intercourse *more frequently* than introverts; 3) extraverts will have intercourse *with more different partners*; 4) extraverts will have intercourse in *more different positions* than introverts; 5) extraverts will indulge in more *varied* sexual behavior outside intercourse; 6) extraverts will indulge in *longer* precoital loveplay than introverts; 7) extraverts will show *quicker habituation* to sexual stimuli than introverts.

A number of studies reporting quite large scale research efforts have been reported elsewhere (Eysenck 1976), including work done in the U.K., in the U.S.A., and elsewhere; it would seem that all the predictions have been verified at a high level of statistical significance. Furthermore it was shown that genetic factors play a prominent part in most of these behavior patterns and the attitudes associated with them (Eysenck 1976, Martin *et al.* 1977).

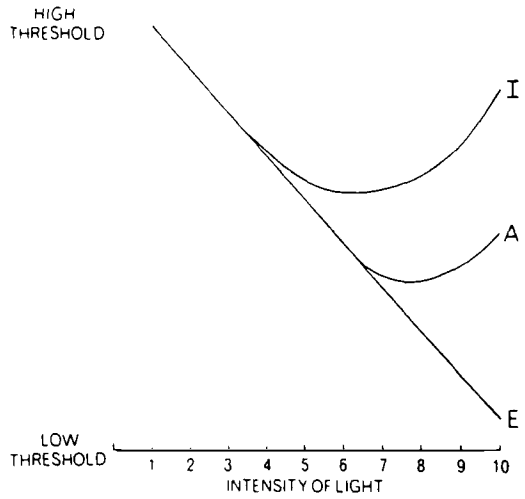

FIG. 5. Schematic diagram of effects of heteromodal stimulation on auditory thresholds for introverts, ambiverts, and extraverts.

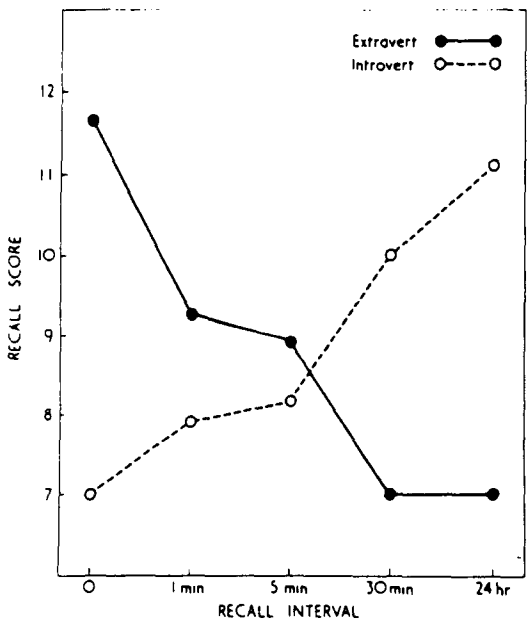

FIG. 6. Influence of recall interval on recall score of extraverts and introverts. From Howarth and Eysenck 1968.

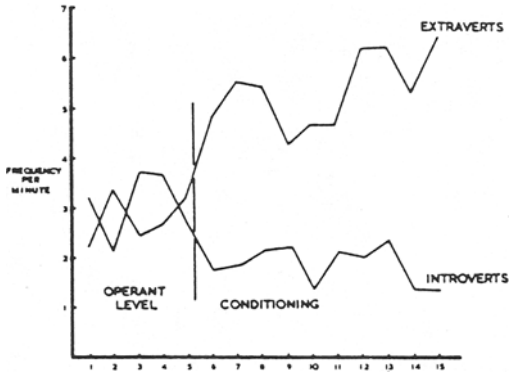

FIG. 7. Frequency of bellpushing of extraverts and introverts, respectively, when reinforcement is an increase in sensory stimulation (Weisen 1966).

Similar arguments can be formulated to predict smoking and drinking habits of extraverts and introverts (Eysenck 1980). A great deal of research has been done in these fields, and the outcome has been very much as one would expect on the basis of the hypothese stated. A large number of social phenomena (Wilson 1981), educational phenomena (Eysenck 1978), and other types of life events could be mentioned here. However, it is not the purpose of this article to survey the whole scene, but merely to suggest ways in which Pavlovian thinking and experimental work on classic conditioning can be, and has been related to personality, and through personality with social behavior.

In general, all predictions and explanations in this area are based on the fundamental conception, also advocated originally by Pavlov, of man as a *biosocial organism*. There has been a tendency on the part of behaviorists and cognitive psychologists to overstress environmental influences and regard man as a creature whose

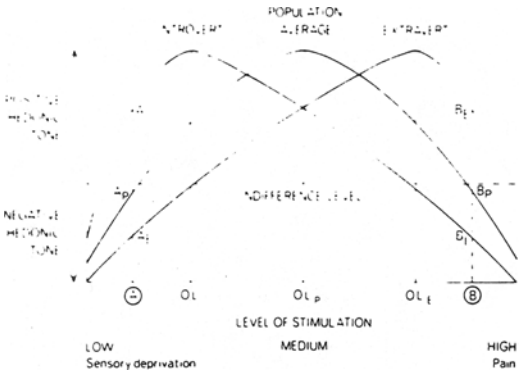

FIG. 8. Hedonic tone in introverts, ambiverts, and extraverts as a function of level of stimulation.

actions are determined entirely by rewards and punishments meted out by society. Such a conjecture leaves out of account the strong biologic constraints within which any such system must work. Sociobiologists and others, including the early adherents of the instinct doctrine, overstressed genetic and biologic determinants, and under-rated the importance of social factors. Only a clear recognition of the importance of both can rescue psychology from a one-sided neglect of important and indeed vital factors in the explanation of human conduct.

In this connection it may be helpful to adopt the doctrine of the *triune brain* (McLean 1973), *i.e.*, the morphologic and functional division of the brain into a reptilian, a paleonmammalian, and a neomammalian brain. This division is illustrated in Figure 9, and while it is not intended to suggest that all McLean has to say about these three parts of the triune brain is correct and well supported, there is no doubt that there are marked differences between the neocortex and paleocortex, and that these are of vital relevance to conditioning theory and its application to social life in general. The neocortex is peculiarly human, in that its development has been such as to overshadow and enclose all the other parts of the brain, which is not true of other mammals. In particular, speech is associated with the huge neocortex of man, and speech is one dominant factor in the social life of humans. However, it is easy to over-rate the cognitive language-function, and rational aspects of human behavior. Motivation is almost entirely supplied by the emotional system of the paleocortex (the limbic system), and this system does not seem to speak any human language, but uses Pavlovian conditioning (and possibly instrumental conditioning) as its language. This fact signifies both the importance and limitations of the Pavlovian system as an all-embracing explanation of human conduct; it explains certain aspects, but not others. Even though it is true that Pavlov tried to include the second signaling system in his teaching, and explained that words may act as conditioned stimuli and conditioned responses, nevertheless he left large areas of human behavior to be accounted for according to principles other than classical conditioning.

However, when we turn to the areas of neurosis and criminality, it would appear that we have here a very strong determinant in the paleocortex, rather than in the rational system of the neocortex. There is good evidence of genetic determination of neurosis and criminality (Eysenck 1977), and this immediately suggests that the origins of neurosis and "conscience" would be intimately connected with the conditioning system.

Eysenck (1968, 1976, 1979) has amended Watson's original theory about the origin of neuroses, and the extinction of neurotic symptoms, a theory that suffered from many overwhelming difficulties of both an experimental and a clinical kind. Watson postulated a traumatic event as forming the beginning of a neurotic disorder, through neutral events accompanying the traumatic one acquiring anxiety-provoking response strength by a process of Pavlovian conditioning. However, for peace-time neuroses at least there is little evidence of traumatic events as initiating the development of such neuroses. In war-time neuroses the position is different, but it is peace-time neuroses we are mainly concerned with, and here clearly Watson's theory does not work. In the second place, even if we assumed some kind of traumatic initiating event producing strong unconditional responses to become associated with the neutral stimuli, learning theory suggests that extinction should soon supervene, following upon the CS-only presentation usual in everyday life circumstances; it would be very rare to find the conditional stimuli related to the neurotic illness occurring always in conjunction with the US! CS-only presentation thus should lead to extinction, and neurotic illnesses as such, lasting for a long period of time, should be impossible.

In the third place, laboratory experiments on conditioning have shown a close dependence of the process of conditioning on precise timing of the CS–US interval; such precise timing is unlikely to be found in everyday life circumstances, and where such split-second timing is missing, conditioning does not occur in the laboratory. Another important objection is that in laboratory conditions the CR is never stronger than the unconditional response (UR), yet in the development of neurotic disorders this is often found. The precipitating US in a neurosis is often accompanied by a relatively mild UR; repeated exposure of the CS only leads, not to extinction, but to an incrementation of the CR until it becomes much stronger than the UR. This is a fatal objection to the Watson model as it stands; it simply does not account for the fact of neurotic disorders.

My own attempts to overcome these difficulties, and provide the basis of a theory that would be more in line with the experimental and clinical facts, takes its crucial lead from the distinction made by Grant between Pavlovian A and Pavlovian B type conditioning. Most behavior therapists, insofar as they take a conditioning point of view, argue in terms of Pavlovian A conditioning; I believe that this is fundamentally erroneous. The two types of conditioning differ

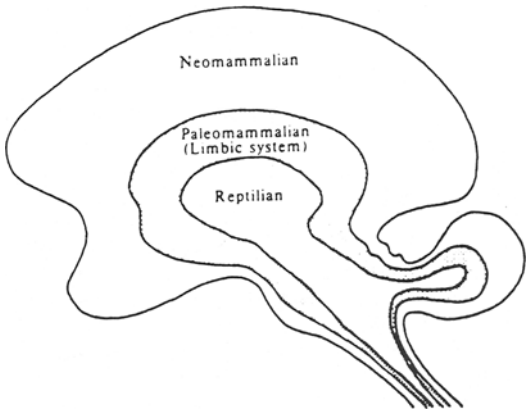

FIG. 9. McLean's view of the triune brain, showing roughly the position of the reptilian, paleomammalian, and neomammalian cortex.

with respect to two fundamental aspects. Type B conditioning provides its own *motivation* in the shape of the US; type A conditioning requires external motivation, *i.e.*, through hunger imposed on the dog that is to be conditioned to salivate to the sound of a bell. In the second place, in type B conditioning the CR is similar to, and identical with, the UR, whereas in type A conditioning the two are essentially *dissimilar*. On both counts the kind of conditioning involved in the development of the neurosis is clearly type B conditioning, and most of the errors that have accrued depend on the erroneous identification of neurosis and its development with Pavlovian A conditioning.

From the fact that CR and UR are essentially identical in type B conditioning, I have deduced a corollary that I think is fundamental to the development of neurotic disorders, namely the law

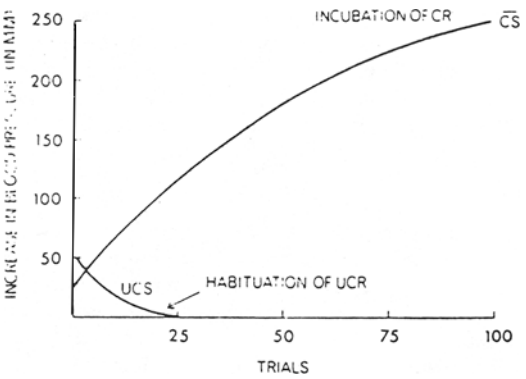

FIG. 10. Habituation of UCR and incubation of CR, measured by increase in blood pressure.

of *incubation of anxiety* (Eysenck 1968). When we normally speak of presentation of the CS only, and the extinction that follows, we look at the situation from the point of view of the experimenter, rather than the subject. This is not important in Pavlovian A conditioning, because the CR and the UR are essentially dissimilar, and hence can be discriminated by the animal. However, when CR is identical with UR, then the notion of CS-only as failing to provide reinforcement loses much of its value. Admittedly the CS is not followed by the US leading to a UR, but it is followed by a CR that is largely undistinguishable from the UR. Hence we do get reinforcement, at least as far as the subject of the experiment is concerned; the experimenter himself of course is fully aware that he does not himself provide the reinforcement, and hence may be misled into expecting a distinction. The subject of the experiment, however, being unable to differentiate between CR and UR, does receive reinforcement, and hence is likely to produce an incrementation of the CS–CR connection, rather than extinction.

I have labeled this phenomenon of incrementation of CR after *CS-only* presentation the *Napalkov phenomenon* (Eysenck 1967), because it was clearly demonstrated by a Russian physiologist working with dogs. Figure 10 shows in diagrammatic form its findings. The US is a pistol shot fired behind the ear of the dog; the UR is an increase in blood pressure in millimetres. Repetition of the US leads quickly to habituation of UR. The actual UR is rather small to begin with, and in no way the kind of traumatic event postulated by Watson.

The development of the CR is quite different, and resembles the insiduous onset of neurotic disorders after a rather weak conditioning trial. After a single pairing of CS and shot, the US is never repeated, but repetition of the CS-only leads to a remarkable increase in size of the CR, until it reaches the very high level of 250 mm, and in some animals becomes chronic at that level, mimicking a psychosomatic type of disorder. It is suggested that this laboratory experiment demonstrates the incubation of anxiety phenomenon which I consider fundamental to the development of neurosis. Details of the theory are given elsewhere (Eysenck 1979, 1982a, and 1982b). Many consequences follow from the theory regarding the treatment of neurotic disorders, and I have argued that extinction of the CR is an essential feature of all successful methods of treatment, and accounts for such successes as are found in spontaneous remission, psychotherapy, and psychoanalysis (Eysenck 1980). Testable deductions can be derived from the theory, and are discussed in the literature.

Criminality, in a sense, indicates the opposite of neurosis. In neurosis an undesired CR has been acquired, and needs to be extinguished as a method of cure. Criminality represents the absence of a "conscience," normally acquired through a process of conditioning, and treatment required for the acquisition of such a "conscience" must be through the application of conditioning methods. In this process the antisocial activities of the child are the conditional stimuli, the penalties imposed by parents, teachers, and peers are the unconditional stimuli, the pain/anxiety produced by these punishments are the URs, leading to the acquisition of CRs similar in nature to the URs. Through a process of generalization the child thus develops a "conscience" consisting essentially of the CRs thus acquired, and generalized to all conditions labeled "bad," "wicked," and "naughty"; this habit of labeling of course helps in the process of generalization. Here again many consequences follow, most of them testable, and such tests as have been carried out have in general favored the theory (Eysenck 1977). There is no space here to discuss the matter in detail, but it should be noted that personality correlates of both neurosis and criminality have been found, and that these are very much as was deduced from the theory outlined above.

The burden of this article is that of the three great leads given by Pavlov, Western psychologists have only taken up one, and have neglected the other two. I am suggesting here that Pavlov's theories are as powerful and useful in relation to personality and individual differences, and in relation to social behavior, as they have been proved to be in relation to learning, and that in the future we might with great advantage go back to Pavlov in trying to understand both individual differences and social behavior. The abysmal failure of non-Pavlovian psychology to arrive at experimentally ascertained and socially valuable truth in these fields suggests that such a return to Pavlov might be a wise move, leading to considerable improvement in the sad state of psychology today. Geniuses have an uncomfortable habit of proving right, even when the evidence for their beliefs is minimal at the time; we do not have that many geniuses in psychology to be able to disregard the promptings of one of the few!

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