A THREE-FACTOR THEORY OF REMINISCENCE

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A three-factor theory of reminiscence is suggested, making use of the concepts of consolidation, reactive inhibition and conditioned inhibition. It is further suggested that the reminiscence phenomenon is highly task-specific, in the sense that different tasks call differentially for the various processes hypothesized. Furthermore, it is suggested that differences in drive conditions, personality, fatigue, drug administration and many other variables impose definite limits to the replication of research findings, and that only specific studies of the influence of these variables, within a given theoretical context, can lead to a proper quantitative theory of reminiscence.

INTRODUCTION

Reminiscence is usually defined in terms of increments in *learning* which occur during a rest period (Hovland, 1951, p. 653): this author warns that before reminiscence 'can be considered a fundamental *learning* phenomenon, explanation of it in terms of fatigue, motivation, and artifacts of measurement must be eliminated'. Osgood (1953), on the other hand, defines reminiscence as 'a temporary improvement in *performance*, without practice', and: 'The term "reminiscence" refers to the objective fact of improved performance' (p. 509, our italics). It is true that learning is usually indexed in terms of performance, and to that extent the two definitions may be considered equivalent, but it is also true that modern learning theory makes a radical distinction between learning and performance; learning may or may not issue in performance, depending on various conditions which require careful investigation. Some of these conditions are indeed mentioned by Hovland in the sentence quoted above, but the terms used are not precise enough to carry much meaning. Would Hull's concept of 'reactive inhibition' be considered equivalent to 'fatigue', or would it be considered as 'negative motivation'? As long as we have no agreed definition of terms such as these, there might be difficulties in the way of un-ambiguously demonstrating the phenomenon under investigation. Furthermore, to recognize 'artifacts of measurement' implies knowledge of the true principles of measurement; there is no agreement on just how measurement ought to proceed.

Ammons (1947a), to take but one example, has suggested a correction for 'warm-up decrement' which depends for its plausibility on the interpretation of the post-rest improvement in performance *after* the first trial as 'warm-up'; if Eysenck's (1956b) explanation of this phenomenon in terms of extinction of conditioned inhibition is preferred, the 'correction' is seen as an artifact which distorts measurement. It can also be shown that results of experiments may be influenced quite powerfully by the definition of single trials used; in the case of the pursuit rotor, for instance, Ammons (1947b) has used 1 min trials, Adams & Reynolds (1954) 30 sec trials, and Eysenck (1956b) 10 sec trials; in view of the marked changes in performance during the first minute or two of post-rest practice such apparently unimportant differences in choice of trials length may lead to quite different results. As an example, Fig. 1 is taken from a study by Eysenck & Willett (1961); it will be seen that, when the

performance of the high-drive and the low-drive groups is plotted in terms of 10 sec trials, the low-drive group has a reminiscence score which is significantly lower than that of the high-drive group. If the results were plotted in terms of 1 min periods instead, no differences in reminiscence would be apparent. It may be suggested that all results should be reported in terms of 10 sec trials; these short trials could always be statistically combined later, if inspection demonstrated that no information was lost by doing so, whereas, if only relatively long trials are recorded, there is no way in which information on shorter trial lengths could be recovered.
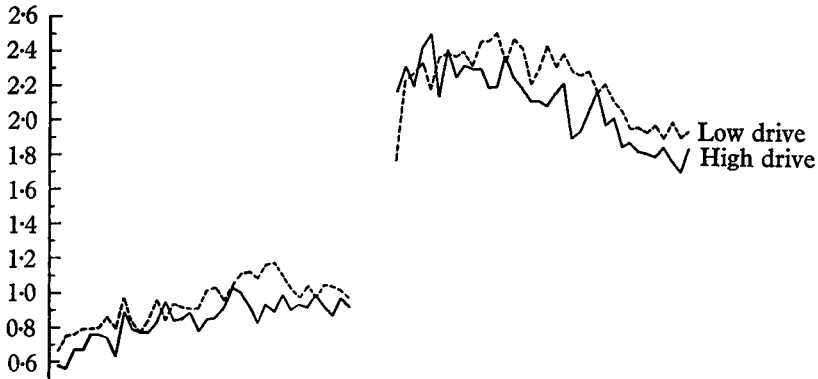

Fig. 1. Reminiscence in high- and low-drive groups. Highly significant differences are obtained when scoring is done in 10 sec periods but these would disappear if scoring were done over 1 min periods (from Eysenck & Willett, 1961).

The difference in the definition of reminiscence between 'increments in learning' and 'increments in practice' becomes important because these differences are to some extent tied up with two sets of theories which have usually been considered antagonistic. It will be suggested that theories of the Hull (1943)–Kimble (1949)–Ammons (1947a) type, involving such concepts as reactive inhibition and conditioned inhibition, are closely identified with a definition of the reminiscence phenomenon involving performance and performance decrement, while theories involving such concepts as consolidation and perseveration (Eysenck, 1964a) lead rather to definitions involving learning and the neural fixation of learning. It will further be suggested that *both* these sets of theoretical concepts are required to explain the facts of reminiscence, so that instead of being considered *alternative* explanations they should rather be regarded as being *complementary*. Finally, it will be suggested that the degree to which reminiscence is a learning or a performance phenomenon, and is therefore subject to explanation in terms of consolidation or inhibition, depends very much on the task in question; theories of reminiscence are *task-specific*, and it is dangerous to extrapolate hypotheses beyond the particular tests used. We shall in the main be concerned with pursuit-rotor learning, but will occasionally extend our discussion to other types of performance tests; verbal learning and nonsense-syllable learning are excluded from our discussion because of the great difficulties which seem to attend the very demonstration of reminiscence in their field. It is not unlikely that a further principle (interference) plays a much greater part in verbal learning than in the type of behaviour with which we are here concerned.

INHIBITION THEORIES OF REMINISCENCE

In the past twelve years or so, Hullian theories have been widely used as explanations of reminiscence phenomena, and the associated ones attending massed practice as opposed to spaced practice. The phenomena in question are as follows.

(1) Massed practice results in performance increments which are inferior to those achieved by spaced practice (e.g. Eysenck, 1956b).

(2) The superiority of spaced practice is within limits proportional to the length of rest pauses between periods of practice (e.g. Adams, 1954).

(3) Shift from massed to spaced practice, or vice versa, leads to shift in performance such as to make the shifted group resemble the other group more and more, and finally become indistinguishable from it (e.g. Adams & Reynolds, 1954).

(4) Programmed rest periods produce reminiscence (improved performance) in groups with massed practice, but not in groups with sufficiently spaced practice. Even after reminiscence, the performance of groups with massed practice does not usually reach that of groups with spaced practice (e.g. Eysenck, 1956b). (Ammons (1947a) has introduced the convenient nomenclature of 'temporary work decrement' for that part of the inferiority of the massed practice group to the spaced practice group which is made good during rest; the remainder he calls 'permanent work decrement'. As we have seen under (3) above, the permanent work decrement is not really 'permanent', of course).

(5) Reminiscence is a negatively accelerated function of length of pre-rest massed practice (e.g. Ammons, 1947b). (Actually this statement is not quite accurate: reminiscence first increases and then decreases as a function of amount of pre-rest practice; cf. Ammons, 1947b; Irion, 1949; Adams & Reynolds, 1954; Feldman, 1964a.)

(6) Reminiscence is a negatively accelerated function of length of rest period (e.g. Ammons, 1947b).

There are in addition a few phenomena which appear during the post-reminiscence period, and which must be considered in connexion with it; they are dependent on the sequence of rest following massed practice, and do not appear when spaced practice is used, or when massed practice is used without the interposition of a rest period. These phenomena are:

(7) The first post-rest trial (which is used to define reminiscence in conjunction with the last pre-rest trial) is followed by a rapid upswing in performance; this is sometimes referred to as 'warm-up', but will here be called 'post-rest upswing' in order to avoid the theoretical implications of the former term (e.g. Ammons, 1947a).

(8) Post-rest upswing is followed by post-rest downswing, i.e. a steady trend of performance in the downward direction. Ammons (1947a) suggests that this downward trend ends, and is reversed, when it reaches the point which a comparable massed practice group would have reached if it had not been given a rest period. Denny's (1951) results give some support to this contention, but Ammons's own data (1947b) do not. He used thirty-five groups of subjects in all, combining seven lengths of rest period ($\frac{1}{3}$, 2, 5, 10, 20, 60 and 360 min) and five lengths of pre-rest practice periods

($\frac{1}{3}$, 1, 3, 8, and 17 min), and claims that 'recovery after...decline can be easily identified in 16 of the 35 curves...and presumably would have occurred in all if post-rest practice had continued beyond 8 min'. From Ammons's data it is possible to plot the post-rest downswing (and of course the preceding upswing) of the groups sub-divided into the seven different 'rest' groups and the five different 'practice' groups. Regardless of what might have happened if practice had continued, the fact remains that, if sixteen of the thirty-five groups show recovery after decline, nineteen show continued decline; this does not suggest that Ammons is justified in claiming that his results support his thesis. The data suggest rather the following generalizations: (8a) Groups with short rest periods ($\frac{1}{3}$, 2 min) show little post-rest upswing or post-rest downswing. (8b) Groups with longer rest periods show marked post-rest upswing and post-rest downswing, but no trace of a reversal. (8c) Groups with short pre-rest practice ($\frac{1}{3}$, 1 min) show neither post-rest upswing nor post-rest downswing. (8d) Groups with longer pre-rest practice show both post-rest upswing and post-rest downswing, but no trace of reversal.

Kimble (1949) uses Hull's concept of *reactive inhibition* (I_R) to account for the temporary work decrement, while Hull's concept of *conditioned inhibition* ($_sI_R$) is used to account for permanent work decrement. According to his account, massed practice produces I_R, which, being a negative drive, impedes performance; hence the inferiority of massed to spaced practice. I_R dissipates during rest, hence the phenomenon of reminiscence. When I_R has grown to be equal to D, the positive drive under which the subject is working during pre-rest massed practice, performance stops and we have a 'block' (Bills, 1931, 1964) or an involuntary rest pause (I.R.P.; Eysenck, 1957); during this I.R.P. I_R dissipates and thus allows performance to begin again. I.R.P.'s are reinforcing, and, as they occur when the subject is resting from the task in question, he is being conditioned not to work in the total stimulus situation of the particular task; hence the habit of not working ($_sI_R$) is being established. Habits do not dissipate during rest; hence $_sI_R$ gives rise to permanent work decrement. However, habit can be extinguished, and Denny, Frisbey & Weaver (1955) have argued that the shift from massed to spaced practice, by eliminating massing (the unconditioned stimulus), should lead to the extinction of $_sI_R$ (the conditioned response). Along similar lines Eysenck (1956b) has tried to explain the post-rest upswing as being due to extinction of $_sI_R$; according to him the UCS is the massing condition of practice including the I.R.P.'s—when these I.R.P.'s are missing in the immediate post-rest period, owing to the dissipation of I_R, extinction must occur. Thus this set of hypotheses would explain all the phenomena in question, with the exception of the post-rest downswing; this remains quite mysterious on any theory hitherto proposed.

This general theory, and the various parts thereof, have been criticized by Koch (1954), Gleitman, Machmias & Neisser (1954), Adams (1961), Jensen (1961) and others; it has been defended by Feldman (1963), among others, who specifically answered certain arguments put forward by Adams (1961). There would appear to be little point in entering into this controversy at this point, but two issues require to be mentioned.

(a) In using the Hullian formulation, Eysenck (1957) has made one important change in his conceptualization which appears to be dictated by the pressure of

experimental investigations. Hull accepts the Mowrer–Miller 'work hypothesis', according to which inhibition is a function of the actual physical work done by the organism. Bilodeau (1952), Ellis, Montgomery & Underwood (1952), Bilodeau & Bilodeau (1954), and others, have adduced convincing evidence to the contrary. The writer prefers, as does Walker (1958), a central rather than a peripheral type of hypothesis, relating inhibition to the amount of continued attention required by the task (i.e. a 'mental work' hypothesis rather than a physical one). Evidence for the existence of reminiscence effects in almost purely perceptual tasks (C. H. Ammons, 1955), and studies of bilateral transfer effects (Ammons & Ammons, 1951; Grice & Reynolds, 1952) further serve to discredit the peripheral hypothesis.

(b) It is sometimes said that there is no direct evidence for concepts such as I_R and $_sI_R$, and Gleitman *et al.* (1954) have, for instance, deduced certain experimental consequences from Hull's postulate of conditioned inhibition which they seem to regard as so unlikely that in the absence of experimental enquiry they are prepared to throw overboard the theory. In putting this hypothesis to the experimental proof, Kendrick (1960) was able to show that the predicted consequences did in fact occur, thus furnishing us with positive proof for the existence of a mechanism very closely resembling $_sI_R$ in its operation. Similarly, I.R.P.'s have been identified by Bills (1931) and more recently Spielmann (1963) and Eysenck (1964*d*). This identification is important as the concept of conditioned inhibition stands or falls with the presence of I.R.P.'s in massed practice.

It would of course have been preferable if I.R.P.'s had been identified in pursuit-rotor work, rather than in different activities; Ammons, Ammons & Morgan (1958) have failed to find any such direct support, as have several workers in our own laboratories. The reason would appear to be the high level of time off target on the pursuit rotor, which is confounded with any I.R.P.'s which may occur. Statistical analysis can only disentangle these two sources of error if I.R.P.'s occurred in some regular or rhythmic fashion. The Spielman and Eysenck studies suggest that this is not so, and that I.R.P.'s occur in a random fashion. This difficulty would therefore appear to be almost insuperable in any learning task, such as the pursuit rotor, where an identical index is used for I.R.P.'s and failure to perform perfectly.

We may note at this point also that consolidation phenomena have been independently verified (Glickman, 1961), so that this concept also is not introduced *ad hoc* to serve the purpose of giving the semblance of a proper theoretical interpretation. The position is exactly the opposite: what is known of the workings of the human brain demands that I.R.P.'s, $_sI_R$ and consolidation should occur in massed practice on the pursuit rotor, and our task is to use these theoretical concepts to their best advantage.

THE PARTIAL FAILURE OF THE INHIBITION THEORIES

In addition to being able to account for the phenomena discussed in the preceding section, inhibition has been used to make two additional predictions, relating to motivation and to personality. Kimble (1950) suggested that subjects working under conditions of high motivation should show greater reminiscence than subjects working under conditions of low motivation, and his work and that of Wasserman (1951),

Eysenck & Maxwell (1961), Eysenck & Willett (1961), Willett & Eysenck (1962) and Feldman (1964a) has indeed shown that this prediction is in accordance with the facts. Kimble's hypothesis was predicated on the assumption that I_R was, as Hull had postulated, a negative drive state; subjects working under a high drive (D) would be able to tolerate a high degree of I_R, and would thus be able to dissipate more I_R during rest.

Eysenck (1956a) suggested that extraverted subjects should show greater reminiscence than introverted subjects, and some twenty studies have since been carried out to investigate this postulated relationship between personality and reminiscence (Eysenck, 1962b). The great majority have given positive results, although the degree of relationship found tended on the whole to be rather low. Eysenck derived his prediction from the general hypothesis that extraverts would be characterized by greater inhibitory cortical potentials, introverts by greater excitatory cortical potentials (Eysenck, 1957). An excellent discussion of the neurophysiology of inhibition is available in Diamond, Balvin & Diamond (1963), and some recent direct evidence on the relation between personality and inhibition is given by Savage (1964) with respect to E.E.G. patterns, Shagass & Schwartz (1963) with respect to evoked potentials, and Claridge & Herrington (1960) with respect to sedation thresholds; Eysenck (1963) has recently related his conception of inhibition and excitation to the activity of the ascending reticular formation.

While at first the confirmation of these two hypotheses might appear to strengthen the inhibitory theory of reminiscence, it can be shown that the details of the experiments in question do not in fact support the theory. Taking the motivation experiments first, it can be stated that the theory demands that the pre-rest performance of the high-drive group should be superior to that of the low-drive group, at least initially; after pulling even further ahead from the moment that I_R in the low-drive group begins to produce I.R.P.'s, the two curves should run parallel from the moment that I_R in the high-drive group also begins to produce I.R.P.'s. Thus at the end of the pre-rest period there should be a clear-cut superior performance on the part of the high-drive group; the greater degree of I_R tolerated by this group, because balanced by the greater drive, should then allow it to dissipate more I_R during rest, thus producing greater reminiscence. The extensive studies by Eysenck & Maxwell (1961), Eysenck & Willett (1961), Willett & Eysenck (1962) and Feldman (1964a) have shown that as far as pre-rest performance is concerned there are no differences between high-drive and low-drive groups; the substantial differences in reminiscence found were all due to post-rest differences in performance. This finding is incompatible with an inhibition hypothesis.

In the case of the relation between personality and reminiscence, a similar condition obtains. According to the hypothesis, greater inhibition in the extraverted group should produce a greater performance decrement; the dissipation of this greater performance decrement would then show up in the form of greater reminiscence. Most studies have simply correlated reminiscence with extraversion, but, while the positive coefficients usually found would certainly be compatible with the hypothesis, they might with equal ease have resulted from a set of scores where pre-rest performance was equal, but post-rest performance favoured the extraverts. A special study to investigate this point was carried out by Eysenck (1964b), who showed that

this second possibility was much closer to the facts; he failed to find any pre-rest difference in performance between his extraverted and introverted subjects. This finding too is incompatible with the inhibition hypothesis.

A third prediction was made from the inhibition hypothesis by Rachman (1962), and here too the prediction was confirmed, but the details of the confirmation served to discredit the inhibition hypothesis. Rachman argued that any strong 'alien' stimulus, such as a loud buzzer, if applied shortly before the rest period on a massed-practice pursuit-rotor task, should have the effect of *disinhibiting* part of the I_R accumulated up to this point; this would improve performance and lower reminiscence. This lowering of reminiscence was indeed found, both by himself and by Feldman (1964*b*), but the effect of the alien stimulus was not to raise performance pre-rest, but rather to lower it post-rest! This result, too, must therefore be counted as disconfirming the inhibition hypothesis. An even more crucial experiment might be one in which the alien stimulus was applied during the rest period, rather than during the pre-rest period; an experiment somewhat along these lines will be discussed in a later section.

These three experiments were performed in order to test predictions made from the basis of the inhibition theory, and while they verified the prediction they did so in a manner which in fact discredited the theory. The next experiment to be discussed was performed with the express intention of testing the inhibition theory directly. Inhibition theory postulates *depression of pre-rest performance* as the crucial factor in reminiscence; reminiscence is due to recovery from this depression. In his experiment, Eysenck (1964*a*) divided 300 subjects into groups equated for initial ability on the pursuit rotor, but either showing or not showing depression of performance during the last 90 sec of pre-rest practice. On the inhibition theory it would be expected that those subjects showing most depression of performance pre-rest (i.e. a depression theoretically due to inhibition) would dissipate most I_R during rest, thus showing greater reminiscence than subjects not showing any pre-rest performance depression. Nothing of the kind was in fact found; reminiscence scores were completely independent of amount of pre-rest performance decrement. While this experiment too is not crucial, it does argue strongly against the inhibition theory.

CONSOLIDATION THEORIES OF REMINISCENCE

A consolidation or 'perseveration' theory of memory was first put forward by Müller & Pilzecker (1900), although not in relation to reminiscence. According to this theory, a neural fixation process is assumed to continue after the organism is no longer confronted with the set of stimuli which constitute the learning task. This fixation process plays a crucial part in efficient retention, according to the consolidation hypothesis, and anything that interferes with perseveration is assumed to have an adverse effect on the subject's ability to transfer material acquired to the permanent memory store. Between the wars this theory fell into disrepute, and McGeoch & Irion (1952) dismissed it as lacking 'any great generality' in their discussion of theories of reminiscence. However, recent work has rendered it respectable again, and there is now some direct evidence to demonstrate its relevance to reminiscence phenomena.

Work on retrograde amnesia, for instance, is difficult to explain on any other lines (Russell & Nathan, 1946), although, being only clinical, it is of course not well enough controlled to be convincing by itself (Coons & Miller, 1960). Experimental work with electro-convulsive shock is more convincing; it has been shown, both with humans and with animals, that, if shock is given between learning and remembering, then genuine retrograde amnesia is produced. It has further been shown that the longer shock was delayed after learning the less was the resulting amnesia (Flescher, 1941; Zubin & Barrera, 1941; Williams, 1950; Duncan, 1949; Cronholm & Molander, 1958; for review see Campbell, 1960). Anoxia (Hayes, 1953; Thompson & Pryer, 1956) and anaesthesia (Leukel, 1957; but see Russell & Hunter, 1937) are other experimental procedures which have given positive results in this connexion, as has the direct stimulation of certain midbrain structures (Glickman, 1958; Thompson, 1958). The evidence for some sort of consolidation process is thus rather convincing, and there is now even some physiological evidence to suggest in more precise terms the how and where of consolidation (Stellar, 1957; Burns, 1958). Glickman (1961) has furnished a fairly recent review of the evidence, and there are in addition some even more recent studies suggesting the detailed working of the determination of reminiscence by consolidation.

Of particular interest in this connexion is the work of Walker (1958) on what he calls *action decrement*. While much of his own work has dealt with verbal learning and animal studies, general relevance is claimed for the main generalizations arrived at (Walker & Tarte, 1963). These two writers summarize the propositions of their theory as follows: '(1) The occurrence of any psychological event...sets up an active, perseverative trace process which persists for a considerable period of time. (2) The perseverative process has two important dynamic characteristics: (*a*) permanent memory is laid down during this active phase in a gradual fashion; (*b*) during the active period, there is a degree of temporary inhibition of recall, i.e. action decrement (this negative bias against repetition serves to protect the consolidating trace against disruption). (3) High arousal during the associative process will result in a more intensely active trace process. The more intense activity will result in greater ulti- mate memory but greater temporary inhibition against recall.' We shall attempt to adapt these notions, in a somewhat modified form, to the problems of pursuit-rotor learning; for the moment let us only notice that no form of consolidation hypothesis by itself can suffice to explain the phenomena associated with reminiscence we have listed. Consolidation theory is adequate to explain reminiscence itself, and the dif- ferences between massed and spaced learning; it cannot explain permanent work decrement, post-rest upswing, or the facts of shifting from massed to spaced practice.

When a theory is clearly not self-sufficient to explain a set of phenomena, one is naturally somewhat reluctant to make use of the theory at all; it seems preferable to try and make do with a smaller set of explanatory variables if possible. There is, however, one experimental study which seems to indicate without any doubt the prime importance of consolidation for reminiscence. In this study Rachman & Grassi (1965) used one control group and three experimental groups. All groups practised for 5 min on the pursuit rotor under conditions of massing, and all groups were given a rest period of 4 hr during which they left the laboratory, before being retested for reminiscence. Equally, all groups were retained for 10 min immediately following the

pre-rest practice. During this period, the control group rested, while the three experimental groups practised on a reversed-cue (mirror) pursuit rotor, it being hypothesized that this practice would interfere with consolidation. One of the experimental groups (group A) carried out this practice during the first 3 min after the 5 min pre-rest practice; the next experimental group (group B) carried out this practice during the 4th to the 6th min, while the third experimental group (group C) carried out this practice during the 7th to the 9th min. Inhibition theory would predict that all four groups would have identical reminiscence scores, 4 hr of rest being quite adequate for all inhibition acquired during the 5 min of practice to dissipate. Consolidation theory, on the other hand, would predict most reminiscence for the control group, least for group A, with groups B and C intermediate. The results bore out the prediction derived from the consolidation theory at an acceptable level of statistical significance.

Of equal interest is a set of experiments which, although not using pursuit-rotor practice or even human subjects, seems to have given strong support to the consolidation theory of memory. In a series of studies McGaugh & Petrinovich (1959), McGaugh, Westbrook & Thomson (1962), and Breen & McGaugh (1961) have injected stimulant drugs into rats after completion of learning periods, and tested the rats after the drug effects had worn off; comparison with control groups demonstrated the superiority of the drug-treated groups, and the authors concluded that the experiments could best be interpreted as showing that drug administration 'improves maze performance by facilitating post-trial consolidation of the neurophysiological process underlying memory' (McGaugh *et al.* 1962, p. 172). Some criticisms have been made of this work (Thiessen, Schlesinger & Calhoun, 1961), but McGaugh & Petrinovich (1963) have succeeded in answering these satisfactorily; it would appear therefore that we must accept this evidence in favour of the consolidation hypothesis.

A COMBINED INHIBITION–CONSOLIDATION THEORY OF REMINISCENCE

The theory to be suggested here combines the essential features from the Kimble two-factor (inhibition) theory and the consolidation hypothesis. We may reconstruct the course of events during pursuit-rotor learning somewhat as follows. (i) During pre-rest practice, I_R builds up and finally enforces I.R.P.'s; the point at which I.R.P.'s begin to occur depends on the drive level under which the subject is working. No permanent memory traces are laid down, and hence no learning takes place. (This statement may require qualification; I.R.P.'s may provide occasions for laying down permanent memory traces, but the very short periods in question are not likely to influence our argument to any great extent.) I.R.P.'s provide the reinforcement for the growth of $_SI_R$, but this also, being a habit, fails to lay down permanent memory traces. (ii) A programmed rest pause allows consolidation of the pursuit-rotor habit to take place, following a negatively accelerated curve of acquisition; this provides the basis of the reminiscence phenomenon. The rest pause also allows $_SI_R$ to consolidate; this habit too follows a negatively accelerated curve of acquisition. The consolidation of $_SI_R$ provides the basis for the permanent work decrement. (iii) Resumption of work after the rest pause produces extinction of $_SI_R$, due to non-reinforcement; $_SI_R$ begins to accumulate again once sufficient I_R has been built up

to produce I.R.P.'s. Working against the post-rest upswing produced by this extinction process is the still-continuing consolidation process; as our quotation from Walker and Tarte has made clear, we conceive of consolidation and work as mutually interfering processes. This interference produces post-rest downswing, which in turn ceases when consolidation is complete; at this point we may then return to the gentle upward-sloping course characteristic of massed practice without rest pause interference.

This theory would seem to account for all the phenomena listed above (p. 165). How does it handle the phenomena of personality and motivation? With respect to the former phenomenon, Eysenck (1964b) has suggested that the differences in conditionability characteristic of extraverted and introverted subjects (Eysenck, 1962a) may be responsible. Introverts condition quickly and strongly, and accordingly will form more $_sI_R$ in the course of the pre-rest practice period; hence after rest they will show greater permanent work decrement. The extinction of $_sI_R$ after resumption of work will then cause the differences in performance between extraverts and introverts to disappear. The plot of the detailed results given by Eysenck (1964b) bears this analysis out in all important particulars. An alternative and rather implausible hypothesis might be that extraverts consolidate better, or learn better in the first place; this hypothesis would be purely *ad hoc* and would in any case not account for the gradual disappearance of the observed differences after resumption of post-rest practice.

As regards motivation, it is to be noted that the major reminiscence effects are lasting, rather than transitory; in other words, the hypothesis that high drive leads to better learning and/or consolidation than low drive is not untenable. No detailed working out of this suggestion will be given here as the point is not central to our argument, and as there are a number of other problems which are in more urgent need of discussion. Normally one would probably have preferred to assume that drive facilitates *learning* rather than consolidation, but the recent work of McGaugh and his colleagues already referred to makes any confident assertion unwise.

TASK-SPECIFIC FEATURES OF REMINISCENCE

Reminiscence in pursuit-rotor learning is almost entirely due to consolidation; in a task such as tapping (Grassi, 1964) it is almost entirely due to reactive inhibition (Fig. 2)—there is in such a task no learning that could consolidate! Why is it that in one task reactive inhibition has no effect in depressing performance, i.e. on the pursuit rotor, while on another one, such as tapping, there is a very profound effect? Why is it that some tasks, such as the pursuit rotor, are almost unaffected by differences in drive, while others, such as conditioning (Willett, 1964), are very much affected? It would appear that there are several dimensions along which tasks can be ranged, and it may be suggested that an investigation of these dimensions could be of very great importance in understanding the phenomena associated with learning and reminiscence. We may postulate three main dimensions.

(1) In the first place, we have tasks which require new learning, such as pursuit-rotor performance; here consolidation of this new learning is obviously of prime importance. At the other end of the continuum are well-practised tasks not involving

new learning, such as tapping (Grassi, 1964), vigilance (Buckner & McGrath, 1963), and visual after-effects (Holland, 1963). Here there is no consolidation, but only reactive inhibition. Other tasks are intermediate, such as inverted alphabet printing, or the pathways test.
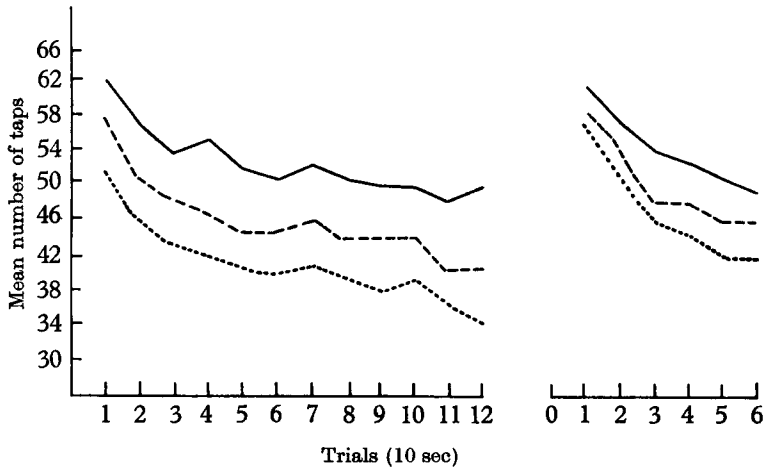

Fig. 2. Reminiscence on tapping task in three groups of subjects (from Grassi, 1964). —, Normal; - - -, behaviour disorder; ..., brain damage.

(2) In the second place, we may order our tasks along a continuum according to the degree to which they are likely to be influenced by the blocks which we may regard as the only demonstrable evidence of reactive inhibition. Self-paced tasks, such as pursuit-rotor performance, would be at one extreme, being almost immune to the effects of inhibition, while experimenter-paced tasks, such as short-time vigilance tests or experimenter-paced reaction time tests, would be at the other. It may be surmised that pursuit-rotor work would be relatively independent of the blocks that might occur, because performance decrements occurring during the block could be made good by improved performance immediately after the block. It should not be impossible to test this assumption experimentally. Broadbent (1953) has also commented on the fact that blocks sometimes do and sometimes do not produce work decrement, and he has suggested a possible task parameter to account for these differences. In the vigilance tasks studied by him, the signal to be detected may be presented for a long or a short time; if a block occurs during the presentation of a short-time signal, this will be missed, and the block will produce a performance decrement. If the block occurs during the presentation of a long-time signal, the signal will still be there after the block has disappeared, and will therefore be noted; there will be no performance decrement. In the case of tasks such as tapping, we may suggest a somewhat analogous mechanism: the block produces a marked slowing down during one of the taps, but this may be made good by a particularly quick series of taps immediately following the block, made possible by the shedding of inhibition which accompanies the involuntary rest pause or block. Thus we have found that extraverts have many more blocks in tapping than do introverts, yet their actual output is equal to that of the introverts (Spielmann, 1963; Eysenck, 1964d).

(3) In the third place, we must turn to the effects of drive. Tasks differ from each other along a continuum, the one end of which is characterized by experimental operations which are of an all-or-none character—you either carry them out properly, or not at all. An intelligence test is perhaps a good example—provided an individual agrees to carry out the test at all, degree of motivation does not seem to have any great influence on his performance (Eysenck, 1944; Tiber & Kennedy, 1964). Other tasks are infinitely variable, in the sense that all types of intermediate performance are possible; the serial reaction time test would be a good example of this other extreme of the continuum, and so would tapping. The hypothetical regression lines of 'Type 1' tasks and 'Type 2' tasks are plotted in diagrammatic form in Fig. 3;
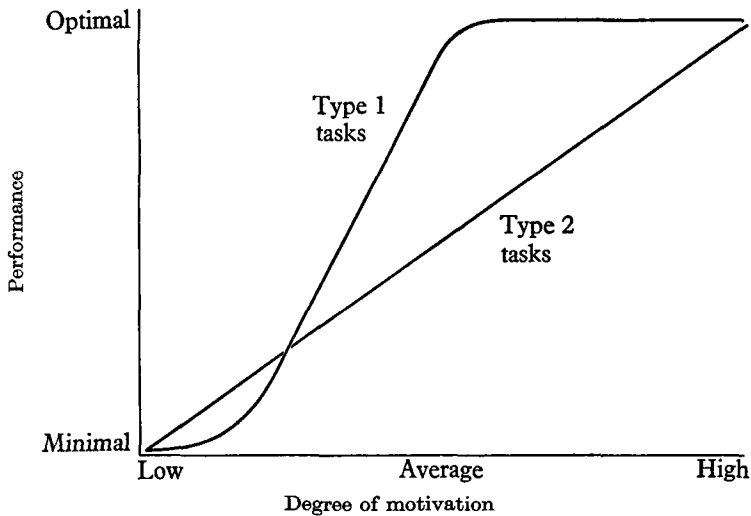

Fig. 3. The relation between performance and degree of motivation in two types of tasks.

they indicate the relative independence of the former, and the close dependence of the latter, on the existing state of drive of the experimental subject. The failure of increased drive to produce greater performance on the pursuit-rotor task would in terms of this analysis be explained as being due to this task being of Type 1; the subject either carries out the task as best he can, or he fails completely to carry it out. The task is not infinitely variable; even a slight general decrement means that the target is hardly ever reached. Type 1 and Type 2 tasks only serve to define the extremes of a continuum, of course; most tasks will be intermediate between these two extremes. The differentiation here intended is very similar to that made by economists when they talk about demand being 'elastic' or 'inelastic' when plotted as a function of price. It should be borne in mind, of course, that other principles, such as the Yerkes–Dodson law, must also be taken into account before making any detailed predictions.

While we may perhaps speak of 'task specificity' in connexion with the role of motivation, or the performance decrements produced or not produced by I_R, it should be remembered that generalizations can only be made for specific populations tested under specified conditions. Brain-damaged subjects, or chronic schizo-phrenics, may show such low degrees of motivation as might not be found in any

normal group, and for them the relative invulnerability of pursuit-rotor performance to drive might cease to hold true. Conditions of fatigue or drug injection may have effects on normal subjects which temporarily shift their performance to parts of the three-dimensional task model which are outside the boundaries of normal groups. There is some evidence that fatigue produces unusually long I.R.P.'s (Bjerner, 1949; Williams, Lubin & Goodnow, 1959), and, while the usual I.R.P.'s on the pursuit rotor may be too short to depress performance, very long I.R.P.'s may be impossible to correct by greater effort immediately succeeding the rest pauses. All these qualifications must be held in mind when making predictions in relation to any specific task for any specific population.

It will be clear from what has been said that it is not possible to speak of a theory of reminiscence in any meaningful fashion; reminiscence is not a single phenomenon with a single explanation, but rather a broad descriptive term covering several different phenomena. Pursuit rotor reminiscence, in terms of our theory, is due to consolidation; reminiscence in tapping, or vigilance, in terms of our theory, is due to the dissipation of I_R; reminiscence in rotating spiral after-effects, in terms of our theory, is due to the dissipation of $_SI$ (stimulus satiation). Other tasks may combine these different mechanisms in varying proportions, or, as in the case of verbal learning, introduce other mechanisms, such as interference. Generalization across tasks is clearly dangerous and difficult.

A QUANTITATIVE MODEL OF PURSUIT-ROTOR REMINISCENCE

Several attempts have been made, notably by Kimble & Shatel (1952), to quantify such concepts as I_R and $_SI_R$ in connexion with pursuit-rotor learning, and demonstrate their growth curves with changes in duration of rest and duration of pre-rest practice; similarly, Eysenck & Willett (1961a) and Feldman (1964a) have plotted the growth of I_R as a function of drive. All this work was done with the explicit assumption that reminiscence was a function of inhibition; if it is admitted that reminiscence is instead a product of consolidation, then it might be thought that the curves and formulae hitherto used to link reminiscence and I_R may now be used to give a quantitative formulation of the growth and decline of the consolidation process instead. (Work on $_SI_R$ is not implicated in this change of theory, as it is still assumed that permanent work decrement is due to conditioned inhibition; provided that consolidation is allowed to proceed in full, the failure of the reminiscence effect to reach the level of a comparable spaced practice group is an adequate measure of $_SI_R$.)

However, there are clearly some difficulties in the way of any simple transformation. Consider Fig. 4, which shows the growth of reminiscence as a function of pre-rest work period duration, for groups working under high and low drive respectively. It would seem possible to regard these curves as measures of *learning*; longer pre-rest work periods give rise to greater learning, which approaches asymptotic values dependent on the drive under which the subject is working. However, as pointed out before, there is also the possibility that the consolidation process is affected by drive, so that the shapes of the resulting curves might be the joint effects of (a) degree of learning, and (b) amount of consolidation. Until this problem is resolved we cannot

make any straightforward identification between theoretical variables and observable values.

There is one observation which points to the possible relevance of consolidation in determining the shape of the curve of reminiscence as a function of pre-rest practice. Ammons (1947b), Feldman (1964a), and others, have found that this curve, once it has reached a plateau, tends to decline again; thus reminiscence after 20 min of practice is less than after 5 min. It is possible to account for this curious fact along the following lines. (a) Learning follows the course of a negatively accelerated exponential function; i.e. most learning takes place in the first few minutes. (b) Consolidation, once it is started during a rest pause, continues to run its course, until it ceases after a definite period of time has elapsed. (c) Memory traces are available for consolidation only for a limited period of time. Let us assume that memory traces are in either one or the other of two states, i.e. either available or not available, and
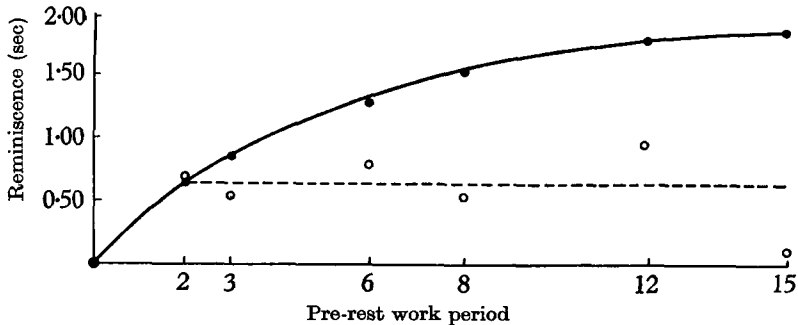

Fig. 4. Reminiscence as a function of pre-rest work period in high- and low-drive groups (from Willett & Eysenck, 1962). ●—●, High drive; O---O, low drive.

that they cease to be available after 15 min. This means that a rest pause introduced after 15 min of practice is just in time to permit all the accumulated memory traces to consolidate and enter the permanent memory storage. A rest introduced after 16 min will exclude all memory traces laid down during the first minute of practice; but this minute has produced the greatest amount of learning (cf. (a) above). Consequently the total amount of learning transferred to permanent memory storage will be less than in the case of the 15 min practice period, and accordingly the curve plotted in Fig. 4 will begin to decline. As pre-rest practice is extended more and more, the decline will continue, because more and more the large amounts of learning that have taken place during the first few minutes will become unavailable, and the relatively small amounts of learning that have taken place during the last few minutes will take their place. This hypothesis may have to be amplified to take into account the possibility that memory traces can exist in more than two states, but within its limitations it does account for a very mysterious effect. If this account be accepted, then we could use the hypothesis in turn to investigate along quantitative lines the disappearance of memory traces under conditions where consolidation is made impossible.

Having in our interpretation lost the possibility of plotting the progress of I_R growth directly, we must search for alternative ways of measuring or indexing this

variable. One possibility is indicated in an early study by Eysenck (1956*b*), who gave eleven 2 min practice sessions divided by 5 min intervals; as shown in Fig. 5 the post-rest upswing and post-rest downswing phenomena are clearly seen in all but the first two and possibly the last trials. On our interpretation the existence of the post-rest upswing demonstrates that during the preceding period enough I_R has developed to produce I.R.P.'s, which in turn cause $_SI_R$ to develop; it is the extinction of this $_SI_R$ which is shown on the graph as post-rest upswing. (Adams, 1963, has criticised this demonstration, but Feldman, 1963, has rebutted these criticisms.) It would seem to follow from our theory that, if the practice periods were shortened sufficiently, I_R would be prevented from building up to a sufficient level to produce I.R.P.'s, and no $_SI_R$ could develop; hence under these conditions we should have no post-rest upswing, but only post-rest downswing. Feldman (1964*a*) has reported such an experiment, subjects practising for fifteen 20 sec periods, separated by 40 sec rest
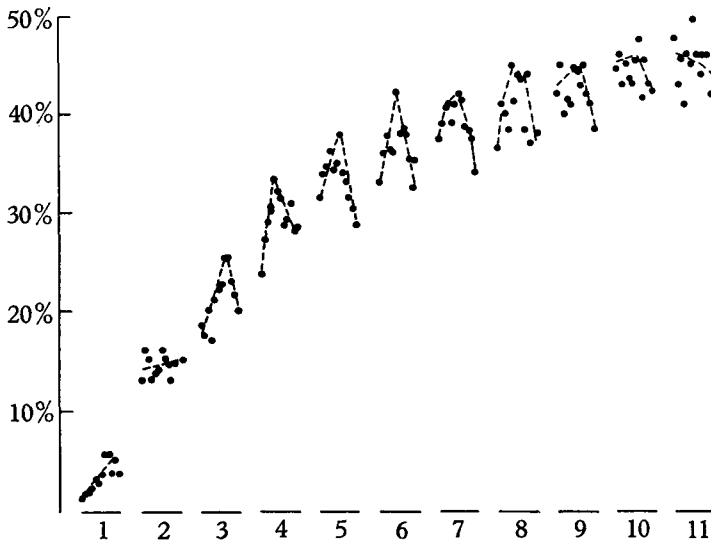

Fig. 5. Post-rest upswing and downswing phenomena. As observed during eleven 2 min practice sessions divided by 5 min intervals (from Eysenck, 1956*b*).

pauses (Fig. 6). It will be seen that our prediction is verified: there is no post-rest upswing on any of the trials, only post-rest downswing. These two practice periods (2 min and 20 sec respectively) thus straddle the moment when I.R.P.'s develop, and repeating the experiment with various intermediate periods should disclose the precise length of pre-rest practice required to produce I.R.P.'s, and consequently post-rest upswing. It may be added, parenthetically, that in Fig. 6 both high-drive and low-drive groups fail to develop post-rest upswing; it follows from our general set of hypotheses that with increase in the length of the pre-rest practice period the low-drive group should show post-rest upswing earlier than the high-drive group. Pre-rest practice of 90 sec would seem to be just on the borderline; Fig. 7 is reproduced from Star (1957), and shows results from sixteen 90 sec work periods separated by 5 min rest periods. It will be seen that post-rest upswing is present but in a very

rudimentary form only. The subjects were students, so that nothing is known about their level of motivation; it would seem that this experiment could with advantage be repeated on groups working under known conditions of high and low motivation.
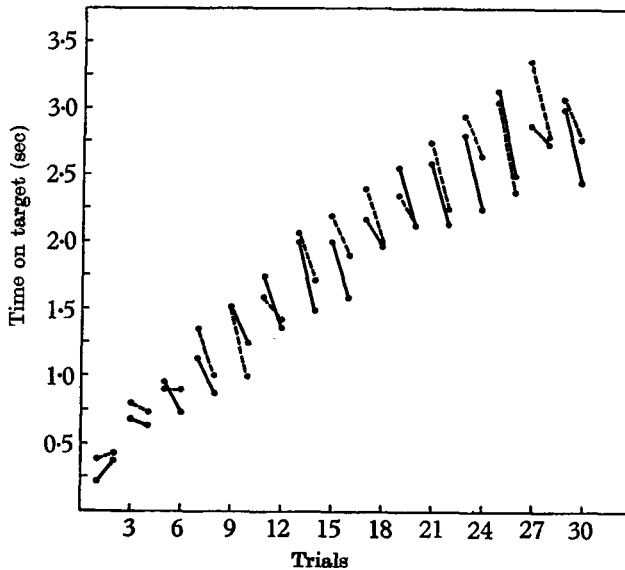

Fig. 6. Post-rest downswing and lack of post-rest upswing as observed during fifteen 20 sec practice periods separated by 40 sec rest periods (Feldman, 1964a). – – –, High drive —, low drive.
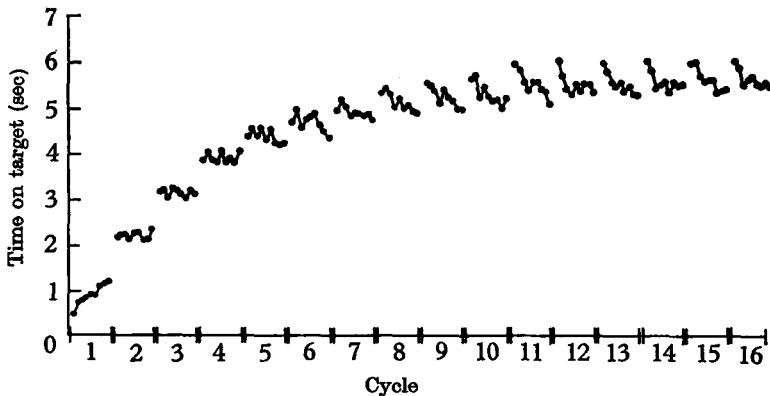

Fig. 7. Border-line development of post-rest upswing and downswing during sixteen 90 sec work periods separated by 5 min rest periods (Star, 1957).

Discussion

The theory tentatively presented here has many weaknesses, which derive from several different sources. Clearly, for instance, what we have to say about the influence of drive on different types of tasks, while receiving some support from such studies as those reported by Eysenck (1964c), must remain speculative until the whole theory of motivation is placed on a sounder and more widely acceptable basis.

Similarly, our discussion of personality correlates of reminiscence must remain speculative until there is a greater amount of agreement on the basic and most fundamental concepts and laws in that field. In other words, any theory of reminiscence will be limited by the failure of associated fields in psychology to put their own house in order.

Within the theory itself, the most obvious drawback is the lack of clear quantification of hypothesized variables. Kimble's and Ammons's two-factor theories held out a promise of quantification of I_R and $_SI_R$, but this promise was clearly premature. The greater complication attending greater sophistication in theory makes observable phenomena less likely to serve directly as measures of underlying theoretical concepts. This situation has led some observers, such as Adams (1963), to suggest giving up theory altogether and relying entirely on inductive studies. This would seem to be a counsel of despair; good theories are the end result of a long process of refinement of bad theories progressively improved through checking and testing of deductions, and the belief that good theories will materialize suddenly if only enough inductive work not guided by any theory is carried out seems to lack support in the history of science.

A third difficulty lies in the task-specific nature of many of the concepts involved. If this suggestion of a close relation between type of task and implication of inhibition and/or consolidation be accepted, and if we agree additionally that this relation itself is modified according to the type of person tested, his drive and fatigue state, and any chemical (drug) influence to which he may have been subjected, then it will be clear that any truly quantitative statement of a proper theory of reminiscence is still very much in the future. However, these are difficulties implicit in the subject-matter of psychology, and cannot be shirked or avoided. They may serve to explain the occasional failure of one experimenter to confirm results reported by another; while such variables as size of target, speed of rotation and stance of subject can be controlled (but frequently are not), such variables as the drive or personality of the subject are hardly ever stated by the original experimenter, and would in any case be extremely difficult to measure or control.

In spite of these admitted difficulties, the theory here presented may be useful in suggesting fruitful ways of designing future experiments in this field, of testing the various intertwined strands of the hypotheses involved, and in making clear the enormous complexity of what at first seemed a simple and clear-cut phenomenon.

REFERENCES

ADAMS, J. A. (1954). Psychomotor performance as a function of intertrial rest interval. *J. exp. Psychol.* **48**, 131–3.

ADAMS, J. A. (1961). The second facet of forgetting: a review of warm-up decrement. *Psychol. Bull.* **58**, 257–73.

ADAMS, J. A. (1963). Comment on Feldman's 'reconsideration of the extinction hypothesis of warm-up in motor behavior'. *Psychol. Bull.* **60**, 460–3.

ADAMS, J. A. & REYNOLDS, B. (1954). Effect of shift in distribution of practice conditions following interpolated rest. *J. exp. Psychol.* **47**, 32–6.

AMMONS, C. H. (1955). An experimental study of temporary and permanent inhibitory effects associated with acquisition of a simple perceptual–motor skill. Louisville, Kentucky: unpublished Ph.D. thesis.

AMMONS, R. B. (1947a). Acquisition of motor skill: I. Quantitative analysis and theoretical formulation. *Psychol. Rev.* **54**, 263–81.

AMMONS, R. B. (1947b). Acquisition of motor skill: II. Rotary pursuit performance with continuous practice before and after a single rest. *J. exp. Psychol.* **37**, 393–411.

AMMONS, R. B. & AMMONS, C. H. (1951). Bilateral transfer of rotary pursuit skill. *Amer. Psychologist*, **6**, 294 (abstract).

AMMONS, R. B., AMMONS, C. H. & MORGAN, R. L. (1958). Subskills in rotary pursuit as affected by rate and accuracy requirements and by distribution of practice. *J. gen. Psychol.* **58**, 259–79.

BILLS, A. G. (1931). Blocking: a new principle of mental fatigue. *Amer. J. Psychol.* **43**, 230–45.

BILLS, A. G. (1964). A study of blocking and other response variables in psychotic, brain-damaged and personality-disturbed patients. *Beh. Res. Ther.* **2**, 99–106.

BILODEAU, E. A. (1952). Decrements and recovery from decrements in a simple work task with variation in force requirements at different stages of practice. *J. exp. Psychol.* **44**, 96–100.

BILODEAU, I. McD., & BILODEAU, E. A. (1954). Some effects of work loading in a repetitive motor task. *J. exp. Psychol.* **48**, 455–67.

BJERNER, B. (1949). Alpha depression and lowered pulse rate during delayed actions in a serial reaction test: a study in sleep deprivation. *Acta physiol. scand.* **16** (Suppl. 65), 1–93.

BREEN, R. A. & McGAUGH, J. L. (1961). Facilitation of maze learning with post-trial injections of picrotoxin. *J. comp. physiol. Psychol.* **54**, 498–501.

BROADBENT, D. E. (1953). Neglect of the surroundings in relation to fatigue decrements in output. In *Fatigue* (eds. W. F. Floud and A. T. Welford). London: H. K. Lewis.

BUCKNER, D. N. & McGRATH, J. J. (ed.) (1963). *Vigilance: a Symposium.* London: McGraw-Hill.

BURNS, B. D. (1958). *The Mammalian Cerebral Cortex.* London: Arnold.

CAMPBELL, D. (1960). The psychological effects of cerebral electroshock. In *Handbook of Abnormal Psychology* (ed. H. J. Eysenck). London: Pitman.

CLARIDGE, G. S. & HERRINGTON, R. N. (1960). Sedation threshold, personality, and the theory of neurosis. *J. ment. Sci.* **106**, 1568–83.

COONS, E. E. & MILLER, N. E. (1960). Conflict versus consolidation of memory traces to explain 'retrograde amnesia' produced by E.C.S. *J. comp. physiol. Psychol.* **53**, 524–31.

CRONHOLM, B. & MOLANDER, L. (1958). Influence of an interpolated E.C.S. on retention of memory material. *U. Stockholm psychol. Lab. Rep.* no. 61.

DENNY, M. R. (1951). The shape of the post-rest performance curve for the continuous rotary pursuit task. *Mot. Skills Res. Exch.* **3**, 103–5.

DENNY, M. R., FRISBEY, N. & WEAVER, J. (1955). Rotary pursuit performance under alternate conditions of distributed and massed practice. *J. exp. Psychol.* **49**, 48–54.

DIAMOND, S., BALVIN, R. S. & DIAMOND, F. R. (1963). *Inhibition and Choice.* New York: Harper and Row.

DUNCAN, C. P. (1949). The retroactive effect of electroshock on learning. *J. comp. physiol. Psychol.* **42**, 32–44.

ELLIS, D. S., MONTGOMERY, V. & UNDERWOOD, B. J. (1952). Reminiscence in a manipulative task as a function of work surface height, pre-rest practice, and interpolated rest. *J. exp. Psychol.* **44**, 420–7.

EYSENCK, H. J. (1944). The effect of incentives on neurotics, and the variability of neurotics as compared with normals. *Brit. J. med. Psychol.* **20**, 100–3.

EYSENCK, H. J. (1956a). Reminiscence, drive and personality theory. *J. abnorm. soc. Psychol.* **53**, 328–33.

EYSENCK, H. J. (1956b). 'Warm-up' in pursuit rotor learning as a function of the extinction of conditioned inhibition. *Acta psychol., The Hague*, **12**, 349–70

EYSENCK, H. J. (1957). *Dynamics of Anxiety and Hysteria.* London: Routledge and Kegan Paul.

EYSENCK, H. J. (1962a). Conditioning and personality. *Brit. J. Psychol.* **53**, 299–305.

EYSENCK, H. J. (1962b). Reminiscence, drive and personality—revision and extension of a theory. *Brit. J. soc. clin. Psychol.* **1**, 127–40.

EYSENCK, H. J. (1963). The biological basis of personality. *Nature, Lond.,* **199**, 1031–4.

EYSENCK, H. J. (1964*a*). An experimental test of the 'inhibition' and 'consolidation' theories of reminiscence. *Life Sci.* **3**, 175–88.

EYSENCK, H. J. (1964*b*). Personality and reminiscence—an experimental study of the 'reactive inhibition' and the 'conditioned inhibition' theories. *Life Sci.* **3**, 189–98.

EYSENCK, H. J. (ed.) (1964*c*). *Experiments in Motivation.* Oxford: Pergamon.

EYSENCK, H. J. (1964*d*). Involuntary rest pauses in tapping as a function of drive and personality. *Percept. mot. Skills,* **18**, 173–4.

EYSENCK, H. J. & MAXWELL, A. E. (1961). Reminiscence as a function of drive. *Brit. J. Psychol.* **52**, 43–52.

EYSENCK, H. J. & WILLETT, R. A. (1961). The measurement of motivation through the use of objective indices. *J. ment. Sci.* **107**, 961–8.

FELDMAN, M. P. (1963). A reconsideration of the extinction hypothesis of warm-up in motor behavior. *Psychol. Bull.* **60**, 452–9.

FELDMAN, M. P. (1964*a*). Pursuit rotor performance and reminiscence. In *Experiments in Motivation* (ed. H. J. EYSENCK). Oxford: Pergamon.

FELDMAN, M. P. (1964*b*). Drive and pursuit rotor reminiscence: the effect of an alien stimulus. In *Experiments in Motivation* (ed. H. J. EYSENCK). Oxford: Pergamon.

FLESCHER, P. (1941). L'amnesia retrograda dopo l'elettro shock. *Schweiz. Arch. Neurol. Psychiat.* **48**, 1–28.

GLEITMAN, H., MACHMIAS, J. & NEISSER, U. (1954). The S–R reinforcement theory of extinction. *Psychol. Rev.* **61**, 23–33.

GLICKMAN, S. E. (1958). Deficits in avoidance learning produced by stimulation of the ascending reticular formation. *Canad. J. Psychol.* **12**, 97–102.

GLICKMAN, S. E. (1961). Perseveration, neural processes and consolidation of the memory trace. *Psychol. Bull.* **58**, 218–33.

GRASSI, J. (1964). Experimental investigation of cortical inhibition in children. London: unpublished Ph.D. thesis, University of London Library.

GRICE, G. R. & REYNOLDS, B. (1952). Effects of varying amounts of rest on conventional and bilateral transfer 'reminiscence'. *J. exp. Psychol.* **44**, 247–52.

HAYES, K. J. (1953). Anoxia and convulsive amnesia in rats. *J. comp. physiol. Psychol.* **46**, 216–17.

HOLLAND, H. C. (1963). Massed practice and reactive inhibition, reminiscence and disinhibition in the spiral after-effect. *Brit. J. Psychol.* **54**, 261–72.

HOVLAND, C. I. (1951). Human learning and retention. In *Handbook of Experimental Psychology* (ed. S. S. STEVENS). London: Chapman and Hall.

HULL, C. L. (1943). *Principles of Behavior.* New York: Appleton-Century-Crofts.

IRION, A. L. (1949). Reminiscence in pursuit-rotor learning as a function of rest and of amount of pre-rest practice. *J. exp. Psychol.* **39**, 492–9.

JENSEN, A. R. (1961). On the reformulation of inhibition in Hull's system. *Psychol. Bull.* **58**, 274–98.

KENDRICK, D. C. (1960). The theory of 'conditioned inhibition' as an explanation of negative practice effects: an experimental analysis. In *Behaviour Therapy and the Neuroses* (ed. H. J. Eysenck). Oxford: Pergamon.

KIMBLE, G. A. (1949). An experimental test of a two-factor theory of inhibition. *J. exp. Psychol.* **39**, 15–23.

KIMBLE, G. A. (1950). Evidence for the role of motivation in determining the amount of reminiscence in pursuit rotor learning. *J. exp. Psychol.* **40**, 248–53.

KIMBLE, G. A. & SHATEL, R. B. (1952). The relationship between two kinds of inhibition and the amount of practice. *J. exp. Psychol.* **44**, 355–9.

KOCH, S. (ed.) (1954). Clark versus Hull. In *Modern Learning Theory.* New York: Appleton-Century-Crofts.

LEUKEL, F. R. (1957). The effect of E.C.S. and pentothal anaesthesia on maze learning and retention. *J. comp. physiol. Psychol.* **50**, 300–6.

McGAUGH, J. L. & PETRINOVICH, L. (1959). The effect of strychnine sulphate on maze-learning. *Amer. J. Psychol.* **72**, 99–102.

McGAUGH, J. L. & PETRINOVICH, L. (1963). Comments concerning the basis of learning enhancement with central nervous system stimulants. *Psychol. Rep.* **12**, 211–14.

McGAUGH, J. L., WESTBROOK, W. H. & THOMSON, C. W. (1962). Facilitation of maze learning with post-trial injections of 5,7-diphenyl-1,3-diazadamantan-6-ol (1757 I.S.). *J. comp. physiol. Psychol.* **55**, 710–13.

McGEOCH, J. A. & IRION, A. L. (1952). *The Psychology of Human Learning.* New York: Longmans, Green and Co.

MÜLLER, C. E. & PILZECKER, A. (1900). Experimentelle Beiträge zur Lehre vom Gedächtnis. *Z. Psychol.* Suppl. 1.

OSGOOD, C. E. (1953). *Method and Theory in Experimental Psychology.* New York: Oxford University Press.

RACHMAN, S. (1962). Disinhibition and the reminiscence effect in a motor learning task. *Brit. J. Psychol.* **53**, 149–57.

RACHMAN, S. & GRASSI, J. (1965). Reminiscence, inhibition and consolidation. *Brit. J. Psychol.* **56**, 157–62.

RUSSELL, R. W. & HUNTER, W. S. (1937). The effects of inactivity produced by sodium amytal on the retention of the maze habit in the albino rat. *J. exp. Psychol.* **20**, 426–36.

RUSSELL, W. R. & NATHAN, P. W. (1946). Traumatic amnesia. *Brain*, **69**, 280–300.

SAVAGE, R. D. (1964). Electro-cerebral activity, extraversion and neuroticism. *Brit. J. Psychiat.* **110**, 98–100.

SHAGASS, C. & SCHWARTZ, M. (1963). Neurophysiological dysfunction associated with some psychiatric disorders. *Psychiat. Res. Rep.* **17**, 130–52.

SPIELMANN, I. (1963). The relation between personality and the frequency and duration of involuntary rest pauses during massed practice. London: unpublished Ph.D. thesis, University of London Library.

STAR, K. H. (1957). An experimental study of 'reactive inhibition' and its relation to certain personality traits. London: unpublished Ph.D. thesis, University of London Library.

STELLAR, E. (1957). Physiological psychology. *Annu. Rev. Psychol.* **8**, 415–36.

THIESSEN, D. D., SCHLESINGER, K. & CALHOUN, W. H. (1961). Better learning: neural enhancement or reduced interference? *Psychol. Rep.* **9**, 493–6.

THOMPSON, R. (1958). The effect of intracranial stimulation on memory in cats. *J. comp. physiol. Psychol.* **51**, 421–6.

THOMPSON, R. & PRYER, R. S. (1956). The effect of anoxia on the retention of a discrimination habit. *J. comp. physiol. Psychol.* **49**, 297–300.

TIBER, N. & KENNEDY, W. G. (1964). The effects of incentives on the intelligence test performance of different social groups. *J. consult. Psychol.* **28**, 187–8.

WALKER, E. L. (1958). Action decrement and its relation to learning. *Psychol. Rev.* **65**, 129–42.

WALKER, E. L. & TARTE, R. D. (1963). Memory storage as a function of arousal and time with homogeneous and heterogeneous lists. *J. verb. Learn. verb. Behav.* **2**, 113–19.

WASSERMAN, H. N. (1951). The effect of motivation and amount of pre-rest practice upon inhibitory potential in motor learning. *J. exp. Psychol.* **42**, 162–72.

WILLETT, R. A. (1964). Eyeblink conditioning and situation-induced anxiety drive. In *Experiments in Motivation* (ed. H. J. Eysenck). Oxford: Pergamon.

WILLETT, R. A. & EYSENCK, H. J. (1962). An experimental study of human motivation. *Life Sci.* no. 4, 119–27.

WILLIAMS, H. L., LUBIN, A. & GOODNOW, J. J. (1959). Impaired performance with acute sleep loss. *Psychol. Mon.* **73**, no. 14.

WILLIAMS, M. (1950). Memory studies in electric convulsion therapy. *J. Neurol. Neurosurg. Psychiat.* **13**, 30–5.

ZUBIN, J. & BARRERA, S. E. (1941). Effect of electric convulsive therapy on memory. *Proc. Soc. exp. Biol., N.Y.*, **48**, 596–7.