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DRIVES AND THE C.N.S. (CONCEPTUAL NERVOUS SYSTEM)[1]

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The problem of motivation of course lies close to the heart of the general problem of understanding behavior, yet it sometimes seems the least realistically treated topic in the literature. In great part, the difficulty concerns that c.n.s., or "conceptual nervous system," which Skinner disavowed and from whose influence he and others have tried to escape. But the conceptual nervous system of 1930 was evidently like the gin that was being drunk about the same time; it was homemade and none too good, as Skinner pointed out, but it was also habit-forming; and the effort to escape has not really been successful. Prohibition is long past. If we *must* drink we can now get better liquor; likewise, the conceptual nervous system of 1930 is out of date and—if we must neurologize—let us use the best brand of neurology we can find.

Though I personally favor both alcohol and neurologizing, in moderation, the point here does not assume that either is a good thing. The point is that psychology is intoxicating itself with a worse brand than it need use. Many psychologists do not think in terms of neural anatomy; but merely adhering to certain classical frameworks shows the limiting effect of earlier neurologizing. Bergmann (2) has recently said again that it is logically possible to escape the influence. This does not change the fact that, in practice, it has not been done.

Further, as I read Bergmann, I am not sure that he really thinks, deep down, that we should swear off neurologizing entirely, or at least that we should all do so. He has made a strong case for the functional similarity of intervening variable and hypothetical construct, implying that we are dealing more with differences of degree than of kind. The conclusion *I* draw is that both can properly appear in the same theory, using intervening variables to whatever extent is most profitable (as physics for example does), and conversely not being afraid to use some theoretical conception merely because it might become anatomically identifiable.

For many conceptions, at least, MacCorquodale and Meehl's (26) distinction is relative, not absolute; and it must also be observed that physiological psychology makes free use of "dispositional concepts" as well as "existential" ones. Logically, this leaves room for some of us to make more use of explicitly physiological constructs than others, and still lets us stay in communication with one another. It also shows how one's views concerning motivation, for example, might be more

[1] Presidential address, Division 3, at American Psychological Association, New York, September, 1954. The paper incorporates ideas worked out in discussion with fellow students at McGill, especially Dalbir Bindra and Peter Milner, as well as with Leo Postman at California, and it is a pleasure to record my great indebtedness to them.

influenced than one thinks by earlier physiological notions, since it means that an explicitly physiological conception might be restated in words that have—apparently—no physiological reference.

What I propose, therefore, is to look at motivation as it relates to the c.n.s.—or conceptual nervous system—of three different periods: as it was before 1930, as it was say 10 years ago, and as it is today. I hope to persuade you that some of our current troubles with motivation are due to the c.n.s. of an earlier day, and ask that you look with an open mind at the implications of the current one. Today's physiology suggests new psychological ideas, and I would like to persuade you that they make psychological sense, no matter how they originated. They might even provide common ground—not necessarily agreement, but communication, something nearer to agreement—for people whose views at present may seem completely opposed. While writing this paper I found myself having to make a change in my own theoretical position, as you will see, and though you may not adopt the same position you may be willing to take another look at the evidence, and consider its theoretical import anew.

Before going on it is just as well to be explicit about the use of the terms motivation and drive. "Motivation" refers here in a rather general sense to the energizing of behavior, and especially to the sources of energy in a particular set of responses that keep them temporarily dominant over others and account for continuity and direction in behavior. "Drive" is regarded as a more specific conception about the way in which this occurs: a hypothesis of motivation, which makes the energy a function of a special process distinct from those S-R or cognitive functions that are energized. In some contexts, therefore, "motivation" and "drive" are interchangeable.

MOTIVATION IN THE CLASSICAL (PRE-1930) C.N.S.

The main line of descent of psychological theory, as I have recently tried to show (20), is through associationism and the stimulus-response formulations. Characteristically, stimulus-response theory has treated the animal as more or less inactive unless subjected to special conditions of arousal. These conditions are first, hunger, pain, and sexual excitement; and secondly, stimulation that has become associated with one of these more primitive motivations.

Such views did not originate entirely in the early ideas of nervous function, but certainly were strengthened by them. Early studies of the nerve fiber seemed to show that the cell is inert until something happens to it from outside; therefore, the same would be true of the collection of cells making up the nervous system. From this came the explicit theory of drives. The organism is thought of as like a machine, such as the automobile, in which the steering mechanism—that is, stimulus-response connections—is separate from the power source, or drive. There is, however, this difference: the organism may be endowed with three or more different power plants. Once you start listing separate ones, it is hard to avoid five: hunger, thirst, pain, maternal, and sex drives. By some theorists, these may each be given a low-level steering function also, and indirectly the steering function of drives is much increased by the law of effect. According to the law, habits—steering functions—are acquired only in conjunction with the operation of drives.

Now it is evident that an animal is often active and often learns when there is little or no drive activity of the kinds listed. This fact has been dealt with in two ways. One is to postulate additional drives—activity, exploratory, manipulatory, and so forth. The other is to postulate acquired or learned drives, which obtain their energy, so to speak, from association with primary drives.

It is important to see the difficulties to be met by this kind of formulation, though it should be said at once that I do not have any decisive refutation of it, and other approaches have their difficulties, too.

First, we may overlook the rather large number of forms of behavior in which motivation cannot be reduced to biological drive plus learning. Such behavior is most evident in higher species, and may be forgotten by those who work only with the rat or with restricted segments of the behavior of dog or cat. (I do not suggest that we put human motivation on a different plane from that of animals [7]; what I am saying is that certain peculiarities of motivation increase with phylogenesis, and though most evident in man can be clearly seen with other higher animals.) What is the drive that produces panic in the chimpanzee at the sight of a model of a human head; or fear in some animals, and vicious aggression in others, at the sight of the anesthetized body of a fellow chimpanzee? What about fear of snakes, or the young chimpanzee's terror at the sight of strangers? One can accept the idea that this is "anxiety," but the anxiety, if so, is not based on a prior association of the stimulus object with pain. With the young chimpanzee reared in the nursery of the Yerkes Laboratories, after separation from the mother at birth, one can be certain that the infant has never seen a snake before, and certainly no one has told him about snakes; and one can be sure that a particular infant has never had the opportunity to associate a strange face with pain. Stimulus generalization does not explain fear of strangers, for other stimuli in the same class, namely, the regular attendants, are eagerly welcomed by the infant.

Again, what drive shall we postulate to account for the manifold forms of anger in the chimpanzee that do not derive from frustration objectively defined (22)? How account for the petting behavior of young adolescent chimpanzees, which Nissen (36) has shown is independent of primary sex activity? How deal with the behavior of the female who, bearing her first infant, is terrified at the sight of the baby as it drops from the birth canal, runs away, never sees it again after it has been taken to the nursery for rearing; and who yet, on the birth of a *second* infant, promptly picks it up and violently resists any effort to take it from her?

There is a great deal of behavior, in the higher animal especially, that is at the very best difficult to reduce to hunger, pain, sex, and maternal drives, plus learning. Even for the lower animal it has been clear for some time that we must add an exploratory drive (if we are to think in these terms at all), and presumably the motivational phenomena recently studied by Harlow and his colleagues (16, 17, 10) could also be comprised under such a drive by giving it a little broader specification. The curiosity drive of Berlyne (4) and Thompson and Solomon (46), for example, might be considered to cover both investigatory and manipulatory activities on the one hand, and exploratory, on the other. It would also comprehend the "problem-seeking" behavior recently studied by Mahut and Havelka at McGill (unpublished studies). They have shown that the rat which is offered a short, direct path to food, and a longer, variable and indirect pathway involving a search for food, will very frequently prefer the more difficult, but more "interesting" route.

But even with the addition of a curiosity-investigatory-manipulatory drive, and even apart from the primates, there is still behavior that presents difficulties. There are the reinforcing effects of incomplete copulation (43) and of saccharin intake (42, 11), which do not reduce to secondary reward. We must not multiply drives beyond reason, and at this point one asks whether there is no alternative to the theory in this form. We come, then, to the conceptual nervous system of 1930 to 1950.

MOTIVATION IN THE C.N.S. OF 1930–1950

About 1930 it began to be evident that the nerve cell is not physiologically inert, does not have to be excited from outside in order to discharge (19, p. 8). The nervous system is alive, and living things by their nature are active. With the demonstration of spontaneous activity in c.n.s. it seemed to me that the conception of a drive system or systems was supererogation.

For reasons I shall come to later, this now appears to me to have been an oversimplification; but in 1945 the only problem of motivation, I thought, was to account for the *direction* taken by behavior. From this point of view, hunger or pain might be peculiarly effective in guiding or channeling activity but not needed for its arousal. It was not surprising, from this point of view, to see human beings liking intellectual work, nor to find evidence that an animal might learn something without pressure of pain or hunger.

The energy of response is not in the stimulus. It comes from the food, water, and oxygen ingested by the animal; and the violence of an epileptic convulsion, when brain cells for whatever reason decide to fire in synchrony, bears witness to what the nervous system can do when it likes. This is like a whole powder magazine exploding at once. Ordinary behavior can be thought of as produced by an organized series of much smaller explosions, and so a "self-motivating" c.n.s. might still be a very powerfully motivated one. To me, then, it was astonishing that a critic could refer to mine as a "motivationless" psychology. What I had said in short was that any organized process in the brain is a motivated process, inevitably, inescapably; that the human brain is built to be active, and that as long as it is supplied with adequate nutrition will continue to be active. Brain activity is what determines behavior, and so the only behavioral problem becomes that of accounting for *in*activity.

It was in this conceptual frame that the behavioral picture seemed to negate the notion of drive, as a separate energizer of behavior. A pedagogical experiment reported earlier (18) had been very impressive in its indication that the human liking for work is not a rare phenomenon, but general. All of the 600-odd pupils in a city school, ranging from 6 to 15 years of age, were suddenly informed that they need do no work whatever unless they wanted to, that the punishment for being noisy and interrupting others' work was to be sent to the playground to play, and that the reward for being good was to be allowed to do more work. In these circumstances, *all* of the pupils discovered within a day or two that, within limits, they preferred work to no work (and incidentally learned more arithmetic and so forth than in previous years).

The phenomenon of work for its own sake is familiar enough to all of us, when the timing is controlled by the worker himself, when "work" is not defined as referring alone to activity imposed from without. Intellectual work may take the form of trying to understand what Robert Browning was trying to say (if anything), to discover what it is in Dali's paintings that can interest others, or to predict the out-come of a paperback mystery. We systematically underestimate the human need of intellectual activity, in one form or another, when we overlook the intellectual component in art and in games. Similarly with riddles, puzzles, and the puzzle-like games of strategy such as bridge, chess, and *go;* the frequency with which man has devised such problems for his own solution is a most significant fact concerning human motivation.

It is, however, not necessarily a fact that supports my earlier view, outlined above. It is hard to get these broader aspects of human behavior under laboratory study, and when we do we may expect to have our ideas about them significantly modified. For my views on the problem, this is what has happened with the experiment of Bexton, Heron, and Scott (5). Their work is a long step toward dealing with the realities of motivation in the well-fed, physically comfortable, adult human being, and its results raise a serious difficulty for my own theory. Their subjects were paid handsomely to do nothing, see nothing, hear or touch very little, for 24 hours a day. Primary needs were met, on the whole, very well. The subjects suffered no pain, and were fed on request. It is true that they could not copulate, but at the risk of impugning the virility of Canadian college students I point out that most of them would not have been copulating anyway and were quite used to such long stretches of three or four days without primary sexual satisfaction. The secondary reward, on the other hand, was high: $20 a day plus room and board is more than $7000 a year, far more than a student could earn by other means. The subjects then should be highly motivated to continue the experiment, cheerful and happy to be allowed to contribute to scientific knowledge so painlessly and profitably.

In fact, the subject was well motivated for perhaps four to eight hours, and then became increasingly unhappy. He developed a need for stimulation of almost any kind. In the first preliminary exploration, for example, he was allowed to listen to recorded material on request. Some subjects were given a talk for 6-year-old children on the dangers of alcohol. This might be requested, by a grown-up male college student, 15 to 20 times in a 30-hour period. Others were offered, and asked for repeatedly, a recording of an old stock-market report. The subjects looked forward to being tested, but paradoxically tended to find the tests fatiguing when they did arrive. It is hardly necessary to say that the whole situation was rather hard to take, and one subject, in spite of not being in a special state of primary drive arousal in the experiment but in real need of money outside it, gave up the secondary reward of $20 a day to take up a job at hard labor paying $7 or $8 a day.

This experiment is not cited primarily as a difficulty for drive theory, although three months ago that is how I saw it. It *will* make difficulty for such theory if exploratory drive is not recognized; but we have already seen the necessity, on other grounds, of including a sort of exploratory-curiosity-manipulatory drive, which essentially comes down to a tendency to seek varied stimulation. This would on the whole handle very well the motivational phenomena observed by Heron's group.

Instead, I cite their experiment as making essential trouble for my own treatment of motivation (19) as based on the conceptual nervous system of 1930 to 1945. If the thought process is internally organized and motivated, why should it break down in conditions of perceptual isolation, unless emotional disturbance intervenes? But it did break down when no serious emotional change was observed, with problem-solving and intelligence-test performance significantly impaired. Why should the subjects themselves report (*a*) after four or five hours in isolation that they could not follow a connected train of thought, and (*b*) that their motivation for study or the like was seriously disturbed for 24 hours or more after coming out of isolation? The subjects were reasonably well adjusted, happy, and able to think coherently for the first four or five hours of the experiment; why, according to my theory, should this not continue, and why should the organization of behavior not be promptly restored with restoration of a normal environment?

You will forgive me perhaps if I do not dilate further on my own theoretical difficulties, paralleling those of others, but turn now to the conceptual nervous system of 1954 to ask what psychological values we may extract from it for the theory of motivation. I shall not attempt any clear answer for the difficulties we have considered—the data do not seem yet to justify clear answers—but certain conceptions can be formulated in sufficiently definite form to be a background for new research, and the physiological data contain suggestions that may allow me to retain what was of value in my earlier proposals while bringing them closer to ideas such as Harlow's (16) on one hand and to reinforcement theory on the other.

Motivation and C.N.S. in 1954

For psychological purposes there are two major changes in recent ideas of nervous function. One concerns the single cell, the other an "arousal" system in the brain stem. The first I shall pass over briefly; it is very significant, but does not bear quite as directly upon our present problem. Its essence is that there are two kinds of activity in the nerve cell: the spike potential, or actual firing, and the dendritic potential, which has very different properties. There is now clear evidence (12) that the dendrite has a "slow-burning" activity which is not all-or-none, tends not to be transmitted, and lasts 15 to 30 milliseconds instead of the spike's one millisecond. It facilitates spike activity (23), but often occurs independently and may make up the greater part of the EEG record. It is still true that the brain is always active, but the activity is not always the transmitted kind that conduces to behavior. Finally, there is decisive evidence of primary inhibition in nerve function (25, 14) and of a true fatigue that may last for a matter of minutes instead of milliseconds (6, 9). These facts will have a great effect on the hypotheses of physiological psychology, and sooner or later on psychology in general.

Our more direct concern is with a development to which attention has already been drawn by Lindsley (24): the nonspecific or diffuse projection system of the brain stem, which was shown by Moruzzi and Magoun (34) to be an *arousal* system whose activity in effect makes organized cortical activity possible. Lindsley showed the relevance to the problem of emotion and motivation; what I shall attempt is to extend his treatment, giving more weight to cortical components in arousal. The point of view has also an evident relationship to Duffy's (13).

The arousal system can be thought of as representing a second major pathway by which all sensory excitations reach the cortex, as shown in the upper part of Fig. 1; but there is also feedback from the cortex and I shall urge that the *psychological* evidence further emphasizes the importance of this "downstream" effect.

In the classical conception of sensory function, input to the cortex was via

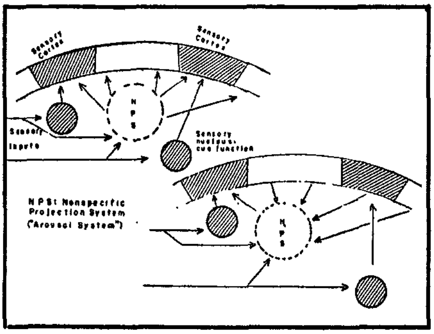

FIG. 1

the great projection systems only: from sensory nerve to sensory tract, thence to the corresponding sensory nucleus of the thalamus, and thence directly to one of the sensory projection areas of the cortex. These are still the direct sensory routes, the quick efficient transmitters of information. The second pathway is slow and inefficient; the excitation, as it were, trickles through a tangled thicket of fibers and synapses, there is a mixing up of messages, and the scrambled messages are delivered indiscriminately to wide cortical areas. In short, they are messages no longer. They serve, instead, to tone up the cortex, with a background supporting action that is completely necessary if the messages proper are to have their effect. Without the arousal system, the sensory impulses by the direct route reach the sensory cortex, but go no farther; the rest of the cortex is unaffected, and thus learned stimulus-response relations are lost. The waking center, which has long been known, is one part of this larger system; any extensive damage to it leaves a permanently inert, comatose animal.

Remember that in all this I am talking conceptual nervous system: making a working simplification, and abstracting for psychological purposes; and all these statements may need qualification, especially since research in this area is moving rapidly. There is reason to think, for example, that the arousal system may not be homogeneous, but may consist of a number of subsystems with distinctive functions (38). Olds and Milner's (37) study, reporting "reward" by direct intracranial stimulation, is not easy to fit into the notion of a single, homogeneous system. Sharpless' (40) results also raise doubt on this point, and it may reasonably be anticipated that arousal will eventually be found to vary qualitatively as well as quantitatively. But in general terms, psychologically, we can now distinguish two quite different effects of a sensory event. One is the *cue function,* guiding behavior; the other, less obvious but no less important, is the *arousal* or *vigilance function.* Without a foundation of arousal, the cue function cannot exist.

And now I propose to you that, whatever you wish to call it, arousal in this sense is synonymous with a general drive state, and the conception of drive therefore assumes anatomical and physiological identity. Let me remind you of what we discussed earlier: the drive is an energizer, but not a guide; an engine but not a steering gear. These are precisely the specifications of activity in the arousal system. Also, learning is dependent on drive, according to drive theory, and this too is applicable in general terms—no arousal, no learning; and efficient learning is possible only in the waking, alert, responsive animal, in which the level of arousal is high.

Thus I find myself obliged to reverse my earlier views and accept the drive conception, not merely on physiological grounds but also on the grounds of some of our current psychological studies. The conception is somewhat modified, but the modifications may not be entirely unacceptable to others.

Consider the relation of the effectiveness of cue function, actual or poten-

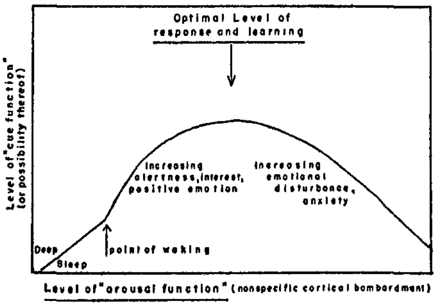

FIG. 2

tial, to the level of arousal (Fig. 2). Physiologically, we may assume that cortical synaptic function is facilitated by the diffuse bombardment of the arousal system. When this bombardment is at a low level an increase will tend to strengthen or maintain the concurrent cortical activity; when arousal or drive is at a low level, that is, a response that produces increased stimulation and greater arousal will tend to be repeated. This is represented by the rising curve at the left. But when arousal is at a high level, as at the right, the greater bombardment may interfere with the delicate adjustments involved in cue function, perhaps by facilitating irrelevant responses (a high D arouses conflicting $_SH_R$'s?). Thus there will be an optimal level of arousal for effective behavior, as Schlosberg (39) has suggested. Set aside such physiologizing completely, and we have a significant behavioral conception left, namely, that the same stimulation in mild degree may attract (by prolonging the pattern of response that leads to this stimulation) and in strong degree repel (by disrupting the pattern and facilitating conflicting or alternative responses).

The significance of this relation is in a phenomenon of the greatest importance for understanding motivation in higher animals. This is the *positive attraction of risk taking,* or mild fear, *and of problem solving,* or mild frustration, which was referred to earlier. Whiting and Mowrer (49) and Berlyne (4) have noted a relation between fear and curiosity—that is, a tendency to seek stimulation from fear-provoking objects, though at a safe distance. Woodworth (50) and Valentine (48) reported this in children, and Woodworth and Marquis (51) have recently emphasized again its importance in adults. There is no doubt that it exists. There is no doubt, either, that problem-solving situations have some attraction for the rat, more for Harlow's (16) monkeys, and far more for man. When you stop to think of it, it is nothing short of extraordinary what trouble people will go to in order to get into more trouble at the bridge table, or on the golf course; and the fascination of the murder story, or thriller, and the newspaper accounts of real-life adventure or tragedy, is no less extraordinary. This taste for excitement *must* not be forgotten when we are dealing with human motivation. It appears that, up to a certain point, threat and puzzle have positive motivating value, beyond that point negative value.

I know this leaves problems. It is not *any* mild threat, *any* form of problem, that is rewarding; we still have to work out the rules for this formulation. Also, I do not mean that there are not secondary rewards of social prestige for risk taking and problem solving—or even primary reward when such behavior is part of lovemaking. But the animal data show that it is not always a matter of extrinsic reward; risk and puzzle can be attractive in themselves, especially for higher animals such as man. If we can accept this, it will no longer be necessary to work out tortuous and improbable ways to explain why human beings work for money, why school children should learn without pain, why a human being in isolation should dislike doing nothing.

One other point before leaving Fig. 2: the low level of the curve to the right. You may be skeptical about such an extreme loss of adaptation, or disturbance of cue function and S-R relations, with high levels of arousal. Emotion is persistently regarded as energizing and organizing (which it certainly is at the lower end of the scale, up to the optimal level). But the "paralysis of terror" and related states do occur. As Brown and Jacobs (8, p. 753) have noted, "the presence of fear may act as an energizer . . . and yet lead in certain instances to an increase in immobility." Twice in the past eight months, while this address was being prepared, the Montreal newspapers reported the behavior of a human being who, suddenly finding himself in extreme danger but with time to escape, simply made no move whatever. One of the two was killed; the other was not, but only because a truck driver chose to wreck his truck and another car instead. Again, it is reported by Marshall (27), in a book that every student of human motivation should read carefully, that in the emotional pressure of battle no more than 15 to 25 per cent of men under attack even fire their rifles, let alone use them efficiently.

Tyhurst's (47) very significant study of behavior in emergency and disaster situations further documents the point. The adult who is told that his apartment house is on fire, or who is threatened by a flash flood, may or may not respond intelligently. In various situations, 12 to 25 per cent did so; an equal number show "states of confusion, paralyzing anxiety, inability to move out of bed, 'hysterical' crying or screaming, and so on." Three-quarters or more show a clear impairment of intelligent behavior, often with aimless and irrelevant movements, rather than (as one might expect) panic reactions. There seems no doubt: the curve at the right must come down to a low level.

Now back to our main problem: If we tentatively identify a general state of drive with degree of arousal, where does this leave hunger, pain, and sex drives? These may still be anatomically separable, as Stellar (45) has argued, but we might consider instead the possibility that there is just one general drive state that can be aroused in different ways. Stellar's argument does not seem fully convincing. There are certainly regions in the hypothalamus that control eating, for example; but is this a *motivating* mechanism? The very essence of such a conception is that the mechanism in question should energize *other* mechanisms, and Miller, Bailey, and Stevenson (31) have shown that the opposite is true.

But this issue should not be pressed too far, with our present knowledge. I have tried to avoid dogmatism in this presentation in the hope that we might try, for once, to see what we have in common in our views on motivation. One virtue of identifying arousal with drive is that it relates differing views (as well as bringing into the focus of attention data that may otherwise be neglected). The important thing is a clear distinction between cue function and arousal function, and the fact that at low levels an increase of drive intensity may be rewarding, whereas at high levels it is a decrease that rewards. Given this point of view and our assumptions about arousal mechanisms, we see that what Harlow has emphasized is the exteroceptively aroused, but still low-level, drive, with cue function of course directly provided for. In the concept of anxiety, Spence and Brown emphasize the higher-level drive state, especially where there is no guiding cue function that would enable the animal to escape threat. The feedback from cortical functioning makes intelligible Mowrer's (35) equating anxiety aroused by threat of pain, and anxiety aroused in some way by cognitive processes related to ideas of the self. Solomon and Wynne's (44) results with sympathectomy are also relevant, since we must not neglect the arousal effects of interoceptor activity; and so is clinical anxiety due to metabolic and nutritional disorders, as well as that due to some conflict of cognitive processes.

Obviously these are not explanations that are being discussed, but possible lines of future research; and there is one problem in particular that I would urge should not be forgotten. This is the cortical feedback to the arousal system, in physiological terms: or in psychological terms, the *immediate drive value of cognitive processes,* without intermediary. This is psychologically demonstrable, and *has* been demonstrated repeatedly.

Anyone who is going to talk about acquired drives, or secondary motivation, should first read an old paper by Valentine (48). He showed that with a young child you can easily condition fear of a caterpillar or a furry animal, but cannot condition fear of opera glasses, or a bottle; in other words, the fear of some objects, that seems to be learned, was there, latent, all the time. Miller (29) has noted this possibility but he does not seem to have regarded it very seriously, though he cited a confirmatory experiment by Bregman; for in the same passage he suggests that my own results with chimpanzee fears of certain objects, including strange people, may be dealt with by generalization. But this simply will not do, as Riesen and I noted (21). If you try to work this out, for the infant who is terrified on *first* contact with a stranger, an infant who has never shown such terror before, and who has always responded with eager affection to the only human beings he has made contact with up to this moment, you will find that this is a purely verbal solution.

Furthermore, as Valentine observed, you cannot postulate that the cause of such fear is simply the strange event, the thing that has never occurred before. For the chimpanzee reared in darkness, the first sight of a human being is of course a strange event, by definition; but fear of strangers does not occur until later, until the chimpanzee has had an opportunity to learn to recognize a few persons. The fear is not "innate" but depends on some sort of cognitive or cortical conflict of learned responses. This is clearest when the baby chimpanzee, who knows and welcomes attendant *A* and attendant *B,* is terrified when he sees *A* wearing *B*'s coat. The role of learning is inescapable in such a case.

The cognitive and learning element may be forgotten in other motivations, too. Even in the food drive, some sort of learning is fundamentally important: Ghent (15) has shown this, Sheffield and Campbell (41) seem in agreement, and so does the work of Miller and his associates (3, 32, 30) on the greater reinforcement value of food by mouth, compared to food by stomach tube. Beach (1) has shown the cortical-and-learning element in sex behavior. Melzack (28) has demonstrated recently that even pain responses involve learning. In Harlow's (16) results, of course, and Montgomery's (33), the cognitive element is obvious.

These cortical or cognitive components in motivation are clearest when we compare the behavior of higher and lower species. Application of a *genuine* comparative method is essential, in the field of motivation as well as of intellectual functions (22). Most disagreements between us have related to so-called "higher" motivations. But the evidence I have discussed today need not be handled in such a way as to maintain the illusion of a complete separation between our various approaches to the problem. It *is* an illusion, I am convinced; we still have many points of disagreement as to relative emphasis, and as to which of several alternative lines to explore first, but this does not imply fundamental and final opposition. As theorists, we have been steadily coming together in respect of ideational (or representative, or mediating, or cognitive) processes; I believe that the same thing can happen, and is happening, in the field of motivation.

REFERENCES

1. BEACH, F. A. The neural basis at innate behavior. III. Comparison of learning ability and instinctive behavior in the rat. *J. comp. Psychol.,* 1939, **28**, 225–262.
2. BERGMANN, G. Theoretical psychology. *Annu. Rev. Psychol.,* 1953, **4**, 435–458.
3. BERKUN, M. M., KESSEN, MARION L., & MILLER, N. E. Hunger-reducing effects of food by stomach fistula versus food by mouth measured by a consummatory response. *J. comp. physiol. Psychol.,* 1952, **45**, 550–554.
4. BERLYNE, D. E. Novelty and curiosity as determinants of exploratory behavior. *Brit. J. Psychol.,* 1950, **41**, 68–80.
5. BEXTON, W. H., HERON, W., & SCOTT, T. H. Effects of decreased variation in the sensory environment. *Canad. J. Psychol.,* 1954, **8**, 70–76.
6. BRINK, F. Excitation and conduction in the neuron. In S. S. Stevens (Ed.), *Handbook of experimental psychology.* New York: Wiley, 1951. Pp. 50–93.
7. BROWN, J. S. Problems presented by the concept of acquired drives. In *Current theory and research in motivation: a symposium.* Lincoln: Univer. of Nebraska Press, 1953. Pp. 1–21.
8. BROWN, J. S., & JACOBS, A. The role of fear in the motivation and acquisition of responses. *J. exp. Psychol.,* 1949, **39**, 747–759.
9. BURNS, B. D. The mechanism of afterbursts in cerebral cortex. *J. Physiol.,* 1955, **127**, 168–188.
10. BUTLER, R. A. Discrimination learning by rhesus monkeys to visual-exploration motivation. *J. comp. physiol. Psychol.,* 1953, **46**, 95–98.
11. CARPER, J. W., & POLLIARD, F. A. Comparison of the intake of glucose and saccharin solutions under conditions of caloric need. *Amer. J. Psychol.,* 1953, **66**, 479–482.
12. CLARE, M. H., & BISHOP, G. H. Properties of dendrites; apical dendrites of the cat cortex. *EEG clin. Neurophysiol.,* 1955, **7**, 85–98.
13. DUFFY, ELIZABETH. An explanation of "emotional" phenomena without the use of the concept "emotion." *J. gen. Psychol.,* 1941, **25**, 283–293.
14. ECCLES, J. C. *The neurophysiological basis of mind.* London: Oxford Univer. Press, 1953.
15. GHENT, LILA. The relation of experience to the development of hunger. *Canad. J. Psychol.,* 1951, **5**, 77–81.
16. HARLOW, H. F. Mice, monkeys, men, and motives. *Psychol. Rev.,* 1953, **60**, 23–32.
17. HARLOW, H. F., HARLOW, MARGARET K., & MEYER, D. R. Learning motivated by a manipulation drive. *J. exp. Psychol.,* 1950, **40**, 228–234.
18. HEBB, D. O. Elementary school methods. *Teach. Mag.* (Montreal), 1930, **12**, 23–26.
19. HEBB, D. O. *Organization of behavior.* New York: Wiley, 1949.
20. HEBB, D. O. On human thought. *Canad. J. Psychol.,* 1953, **7**, 99–110.
21. HEBB, D. O., & RIESEN, A. H. The genesis of irrational fears. *Bull. Canad. Psychol. Ass.,* 1943, **3**, 49–50.
22. HEBB, D. O., & THOMPSON, W. R. The social significance of animal studies. In G. Lindzey (Ed.), *Handbook of social psychology.* Cambridge, Mass.: Addison-Wesley, 1954. Pp. 532–561.
23. LI, CHOH-LUH, & JASPER, H. Microelectrode studies of the cerebral cortex in the cat. *J. Physiol.,* 1953, **121**, 117–140.
24. LINDSLEY, D. B. Emotion. In S. S. Stevens (Ed.), *Handbook of experimental psychology.* New York: Wiley, 1951. Pp. 473–516.
25. LLOYD, D. P. C. A direct central inhibitory action of dromically conducted impulses. *J. Neurophysiol.,* 1941, **4**, 184–190.
26. MACCORQUODALE, K., & MEEHL, P. E. A distinction between hypothetical constructs and intervening variables. *Psychol. Rev.,* 1948, **55**, 95–107.

27. MARSHALL, S. L. A. *Men against fire.* New York: Morrow, 1947.
28. MELZACK, R. The effects of early experience on the emotional responses to pain. Unpublished doctor's dissertation, McGill Univer., 1954.
29. MILLER, N. E. Learnable drives and rewards. In S. S. Stevens (Ed.), *Handbook of experimental psychology.* New York: Wiley, 1951. Pp. 435–472.
30. MILLER, N. E. Some studies of drive and drive reduction. Paper read at Amer. Psychol. Ass., Cleveland, September, 1953.
31. MILLER, N. E., BAILEY, C. J., & STEVENSON, J. A. F. Decreased "hunger" but increased food intake from hypothalamic lesions. *Science,* 1950, **112**, 256–259.
32. MILLER, N. E., & KESSEN, MARION L. Reward effects of food via stomach fistula compared with those via mouth. *J. comp. physiol. Psychol.,* 1952, **45**, 555–564.
33. MONTGOMERY, K. C. The effect of activity deprivation upon exploratory behavior. *J. comp. physiol. Psychol.,* 1953, **46**, 438–441.
34. MORUZZI, G., & MAGOUN, H. W. Brain stem reticular formation and activation of the EEG. *EEG clin. Neurophysiol.,* 1949, **1**, 455–473.
35. MOWRER, O. H. Motivation. *Annu. Rev. Psychol.,* 1952, **3**, 419–438.
36. NISSEN, H. W. Instinct as seen by a psychologist. *Psychol. Rev.,* 1953, **60**, 291–294.
37. OLDS, J., & MILNER, P. Positive reinforcement produced by electrical stimulation of septal area and other regions of rat brain. *J. comp. physiol. Psychol.,* 1954, **47**, 419–427.
38. OLSZEWSKI, J. The cytoarchitecture of the human reticular formation. In E. D. Adrian, F. Bremer, & H. H. Jasper (Eds.), *Brain mechanisms and consciousness.* Oxford: Blackwell, 1954.
39. SCHLOSBERG, H. Three dimensions of emotion. *Psychol. Rev.,* 1954, **61**, 81–88.
40. SHARPLESS, S. K. Role of the reticular formation in habituation. Unpublished doctor's dissertation, McGill Univer., 1954.
41. SHEFFIELD, F. D., & CAMPBELL, B. A. The role of experience in the "spontaneous" activity of hungry rats. *J. comp. physiol. Psychol.,* 1954, **47**, 97–100.
42. SHEFFIELD, F. D., & ROBY, T. B. Reward value of a non-nutritive sweet taste. *J. comp. physiol. Psychol.,* 1950, **43**, 471–481.
43. SHEFFIELD, F. D., WULFF, J. J., & BACKER, R. Reward value of copulation without sex drive reduction. *J. comp. physiol. Psychol.,* 1951, **44**, 3–8.
44. SOLOMON, R. L., & WYNNE, L. C. Avoidance conditioning in normal dogs and in dogs deprived of normal autonomic functioning. *Amer. Psychologist,* 1950, **5**, 264. (Abstract)
45. STELLAR, E. The physiology of motivation. *Psychol. Rev.,* 1954, **61**, 5–22.
46. THOMPSON, W. R., & SOLOMON, L. M. Spontaneous pattern discrimination in the rat. *J. comp. physiol. Psychol.,* 1954, **47**, 104–107.
47. TYHURST, J. S. Individual reactions to community disaster: the natural history of psychiatric phenomena. *Amer. J. Psychiat.,* 1951, **107**, 764–769.
48. VALENTINE, C. W. The innate bases of fear. *J. genet. Psychol.,* 1930, **37**, 394–419.
49. WHITING, J. W. M., & MOWRER, O. H. Habit progression and regression—a laboratory study of some factors relevant to human socialization. *J. comp. Psychol.,* 1943, **36**, 229–253.
50. WOODWORTH, R. S. *Psychology.* New York: Holt, 1921.
51. WOODWORTH, R. S., & MARQUIS, D. G. *Psychology.* (5th Ed.) New York: Holt, 1947.

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