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THE CONCEPT OF INTEGRATION IN PSYCHOLOGY

by

Christopher Nelson Ballantyne Robinson

**Thesis presented in partial fulfillment of the
requirements for the degree of**

MASTER OF ARTS

Graduate Programme in Psychology

Carleton University

Ottawa, Canada

April, 1993

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Abstract

Throughout psychology, the concept of integration is used to explain the formation of higher levels from lower ones. In perception theory, for example, the concept is used to explain the formation of perceptions from elemental sensations. Throughout the history of psychology, integration has been defined in many ways -- e.g., as summation, association, correlation, and connection, to name just a few. Underlying these varied definitions, however, has been a single hegemonic approach to the problem of integration. I call this approach **mechanical integration**, and define it as: The putting together of previously existing parts or elements by some external organizing entity or principle. In the first half of this thesis, I demonstrate: 1) the varied uses of the concept of mechanical integration in psychology; 2) the elementarist assumptions behind the concept of mechanical integration, and; 3) the problems with the concept of mechanical integration (false analogy, infinite regress, and the 'degrees of freedom' problem).

An alternative to the concept of mechanical integration can be found in biology and comparative psychology. This alternative is the concept of **integrative levels**. The integrative levels approach avoids reductionism because it views higher levels as irreducible to lower ones; it avoids holism, because it views higher levels as integrated with, and dependent upon, lower ones. This approach also views the emergence of new levels from a 'genetic' (i.e., origins) or evolutionary perspective. In the second half of this thesis, I provide:

1) a review of Integrative Levels Theory; 2) a new definition of integration, in terms of structural, functional, and historical (genetic) relations; and 3) examples of how the concept of integrative levels can be applied to psychology.

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Perceptual Integration Processes

COGNITIVE PSYCHOLOGY 23, 615-680 (1991)

Coordinating Cognitive Information: Task Effects and Individual Differences in Integrating Information from Several Sources

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Psychological Dimensions

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American Museum of Natural History
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Journal of Experimental Psychology: Learning, Memory, and Cognition 1992, Vol. 18, No. 1, 125-137

Episodic Integration

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Integration versus Interactive Activation: The Joint Influence of Stimulus and Context in Perception
MARK W. MASSARO AND MICHAEL M. COLLINS
University of California, Santa Cruz

Journal of Memory and Language 28, 444-461 (1989)

1: MECHANICAL INTEGRATION

As the collage on the opposite page suggests, the word 'integration' appears in numerous psychological articles. It is used in a multitude of areas, from the 'social integration' of ants, to 'integrative processes' in reading comprehension. Today, the concept of integration is most often used in cognitive psychology. What would cognitive theories of perception be without the idea of special mechanisms integrating sensory signals into representations? How could visual codes be organized into percepts; how could a collection of phonemes, or a bundle of semantic features, become a meaningful word? Indeed, all cognitive theories assume the existence of 'integrative processes' of some sort or another. It would appear, then, that the concept of integration was well defined, and that the existence of 'integrative processes' was well founded. Yet, I have been unable to find in the literature a single theoretical treatment of the concept of integration *itself*: its logic, its assumptions, and its evidence. Instead, the existence of 'integrative processes' is merely *assumed*.

For this reason, I believe that the only way to explore the assumptions behind the concept of integration is to trace it back to its origins in psychology -- to when the assumptions behind the concept were more explicit. After doing this, I shall define the concept and trace its historical use through to the present day.

*I Historical Background***A. William James**

Long before William James wrote his Principles of Psychology, Bishop Berkeley had already set the stage for all psychological theories that were to use the concept of integration. In 1709, Berkeley argued that sensory impressions were integrated through association with past impressions to form meaningful perceptions (Dember, 1964). This idea of beginning with basic elements that are then combined to form higher psychological functions would appear in many different forms throughout the history of psychology. J. S. Mill, Helmholtz, and Wundt all shared this same basic elementaristic-associationistic approach (Dember, 1964).

Given the elementarist tradition, the concept of integration was already a well accepted by the time William James wrote his Principles of Psychology. In James' time, integration was being used to help define major psychological categories, such as mind, consciousness, and perception. For example, one early psychologist, cited in James (1890), defined perception as a process by which the mind, "supplements a sense-impression by an accompaniment or escort of revived sensations, the whole aggregate of actual and revived sensations being solidified or 'integrated' into the form of a percept..." (Sully in James, 1890, Vol. II, p. 79)

Percepts were thought to be the resultant aggregates of some sort of integration process -- either summation, or association. This emphasis on 'summation' and 'aggregates' in the theories of perception meant that integration was viewed primarily as an additive process; indeed, the whole was seen as the simple sum of its parts. This definition of integration was also central in two theories of mind that were popular in James' time: the

Mind-Stuff theory, and the Master-Homunculus theory (James, 1890; see Flanagan, 1986 for a summary).

According to the mind-stuff theory, the mind was a result of the summation of lower states. As James (1890) described, mind-stuff theory attempted to "*explain the constitution of higher mental states by viewing them as identical with lower ones summed together*" (p. 162, cited in Flanagan, 1986, p. 41; original italics). In the mind-stuff theory, the role of integration was of "summation" or the forming of "aggregates".

According to James, the mind-stuff theory begs for an homunculus. Some external observer, argued James, is needed to comprehend the resultant sum -- to see the totality *as a sum*. In typical Jamesian style, he invites the reader to "Take a sentence of a dozen words, and take twelve men and tell to each one word. Then stand the men in a row or jam them in a bunch, and let each think of his word as intently as he will; nowhere will there be consciousness of the whole sentence" (Vol. 1, p. 160). The main problem, according to James, was that:

All the 'combinations' which we actually know are EFFECTS, wrought by the units said to be 'combined', UPON SOME ENTITY OTHER THAN THEMSELVES...

In other words, no possible number of entities (call them as you like whether forces, material particles, or mental elements) can sum *themselves* together. Each remains in a sum, what it always was; and the sum itself exists only for a *bystander* who happens to overlook the units and to apprehend the sum as such... (Vol. 1, p. 158-159; original italics, of course)

The master homunculus theory, then, was the same as mind-stuff theory, with the exception that it postulated the existence of some entity, an "homunculus", that performed the summation and acted as a by-stander.

James criticized the master-homunculus theory on empirical grounds, arguing that there does not exist any evidence for a special part of the brain that possesses consciousness. But, as Flanagan (1986) points out, the homunculus theory also results in an infinite regress: "...if we *always* need a comprehender who must exist in addition to, and outside of, what is being comprehended then we have an infinite regress on our hands" (p. 43; added italics). In the next chapter, I will provide more details of this argument.

Thus, the mind-stuff theory begs the master homunculus theory, and the master homunculus theory ends in an infinite regress. Both these theories are the result of the same elementarist, or reductionist, method -- one theory being a logical continuation of the other. This **elementarism** assumes that lower levels, or 'states', are more fundamental than higher ones, and that higher levels can ultimately be reduced to lower ones.

As we have seen in the mind-stuff and master-homunculus theories, elementarist theories always encounter a problem: Given that higher states exist, how does one account for them? This, essentially, is the **problem of integration**. The concept of integration addresses the problem of how higher levels (e.g., higher mental functions) are formed from lower levels (e.g., mental elements). Strictly reductionist theories may write-off higher functions as 'epiphenomena', but if we are to refer to such higher processes as 'perception', 'mind' and 'consciousness', then some way of accounting for the existence of these higher processes is needed. In other words, if psychology is to accept more levels than just 'brain phenomena' or 'mental elements' as real, then some process of 'getting from the lower to the higher' is needed. It is precisely this problem that the concept of

integration addresses. Hence, the concept of mechanical integration is the result of an awkward union between two opposing beliefs: that lower elements are primary, and that higher levels cannot be explained fully by appealing to the elements. As a result of this gap between that which explains (lower elements) and that which is explained (higher processes) some *additional explanation*, in the form of hypothetical integration processes, is required.

The need to integrate sensory elements into percepts is necessary only if one first believes that experience is broken up into sensory elements. But James did not believe in the primacy of elements: "The 'simple sensation' or 'simple idea', James always insisted, is not to be found in unanalyzed, unintellectualized experience; it is a product of abstraction, arrived at after long and sophisticated study" (Heidbreder, 1933, p. 161). James therefore had doubts about the elementarists' concept of integration.

...a higher state *is* not a lot of lower states; it is itself. When, however, a lot of lower states have come together, or when certain brain-conditions occur together, which, *if they occurred separately, would produce* a lot of lower states, we have not for a moment pretended that a higher state may not *emerge*. In fact it does emerge under those conditions...But such emergence is that of a new psychic entity, and is *toto coelo* different from such an 'integration' of lower states as the mind-stuff theory affirms. (1890, Vol. 1, p. 162; original italics)

Since James was against any kind of elementarism, he was also against any kind of summation process. Simply put, James argued, the problem of integration arises only when one has broken experience into artificial elements.

To summarize, a single hegemonic view of integration arose in the early stages of psychology. In theories of perception and of mind, integration was defined in elementarist terms. This view of integration I call **mechanical integration**, for it assumes the primacy of elements, or parts, over the whole, and assumes a mechanical process of putting elements together to form wholes. As we saw with the mind-stuff theory, elementarist theories beg for an homunculus to perform the operation of integration. In the light of all this, we can now define mechanical integration as: *The putting together of previously existing parts or elements by some external entity or force.* The central themes of my thesis will be to show that mechanical integration is ubiquitous in psychology, to argue that it is problematic, and to provide an alternative, non-mechanical definition of integration.

B. Gestalt Psychology

James was not alone in his mistrust of elementalism. The Gestaltists believed that what was 'immediately given' in experience were not elementary sensations, but wholes, or 'Gestalten' -- a belief expressed in the slogan 'the whole is more than the sum of its parts.' For example, a series of straight lines can form groups according to the 'principle of proximity'. Lines that appear together do so because of their relationship to each other, as in the following figure | | | |. Other 'organizing principles', such as 'closure' and 'praeganz', work in a similar fashion; it is the *totality* of relationships, not the elements themselves, that determines the experience. "The fact that two lines are seen as a pair cannot be explained by taking the lines singly nor by analysing them into parts. It is the disposition of the

total field that determines that they shall be seen in twos" (Heidbreder, 1933, p. 351). The two lines are not simply associated with each other, but are *organized* into a group.

One could change the grouping of the same lines as above by using the principle of 'closure', as in the following [] [] [. Groups are now formed not according to proximity, but according to the principle of closure. Notice that the original straight lines have not changed, neither has their proximity; yet, the experience of grouping *has* changed. What accounts for the shift in perception is the 'total field', not the individual elements. Gestalt psychology,

...flatly denies that elements pieced together, blended, fused or associated in any way give the perceptions we actually experience. A core of sensations, forming the centre of a cluster of images or of any other elementary processes, simply is not what we immediately perceive....The elements, they have asserted repeatedly, exist only as products of analysis....A direct examination of experience does not disclose elements at all -- in Koehler's words, "minute local things which nobody ever sees." (Heidbreder, 1933, p. 339)

In Gestalt psychology, we see a principled criticism of elementarist-associationist theories. The *end result* of 'perceptual organization,' or integration, is not merely a 'sum' held together by the 'glue' of associations. In the figure/ground pictures, for example, an experience can shift from seeing a vase to seeing two faces *without* change to the *elements* of the stimulus itself. What is immediately given in experience, then, are 'segregated wholes', not clusters of sensations.

Rather than being the primary source of experience, the elements of associationism are the products of trained abstraction. But once these

abstract elements are taken as primary, then some equally abstract means of integrating them is required:

The bonds of connection, like the elements themselves, are unreal....The error of elements has as a necessary consequence the error of associations. If experience is broken into artificial bits, some artificial means must be devised of putting it together again, and the bonds of associations are the result. (Heidbreder, 1933, p. 340)

It must be recognized that the Gestalt psychologists, being influenced by Kant, did not consider these organizing principles to be *qualities of the objects themselves* (Reed, 1988). In the phi phenomenon, the external lights are fixed and stationary; the perception of continuous motion is *imposed* on them. In other words, the organizing principles existed, for the Gestaltists, in the head, not in the external stimulation. The exact way in which the organizing occurred, however, was not clear. The principles which organize elements into wholes were said to be embedded in the structure of our nervous system: "Gestalt psychologists hypothesize that the mind is endowed with the capacity to organize patterns of stimulation...In other words, simple patterns of stimulation are organized by the innate structure of the nervous system" (Kendler, 1987, p. 203). (However, we shall see in the next chapter how Koehler objected to this view of the nervous system).

Despite the Gestalt psychologists' attempts to reject elementarism, however, the Gestalt laws of perceptual organization still fit the definition of mechanical integration: 'The putting together of previously independent parts or elements by some external force or entity'. There is still the assumption that sensations are not organized, and that there is a need to

organize them. Rather than homunculi organizing them, Gestalt psychology appeals to organizing principles. In other words, although the *end result* of integration maybe 'more than the sum of its parts', the integration process *itself* still implies that some external force or entity must do the organizing, for the parts are assumed to be primarily separate. Kendler (1987) is a typical example of how the Gestaltists' work has been interpreted: "Gestalt psychologists offered a variety of perceptual phenomena to demonstrate that we *integrate incoming stimulation into organized percepts*" (p. 201; added italics). Thus, except for its rejection of associationism, Gestalt theory is hardly distinguishable from the 19th Century mechanistic theories of perceptual integration; consider Sully's definition of perception as a process by which sensations are "...solidified or 'integrated' into the form of a percept..." (Sully, in James, 1890, Vol. 2, p. 79). Whether 'organizing principles' (learned or innate) or 'organizing agents', the logic is that of mechanical integration. The organizing principles of Gestalt psychology served the same function as the 'glue' of associations in elementarist theories.

C. Behaviourism

Integration played a lesser role in behaviourist theories, if only because such processes as 'perception' were often neglected. The existence of internal integrative processes is not necessarily needed in a theory that views perception as a response to a stimulus (e.g., Skinner, 1976). But a quick look at Behaviourism reveals. yet again, the concept of mechanical integration.

Elenor Gibson (1969) described behaviouristic theories of perceptual integration as 'motor copy' theories. In such a framework, it is the association of overt responses to stimuli that forms the basis of perceptual integration. Hence, integration really meant 'learning', or rather, the forming of associations between stimuli, and between stimuli and responses. The content of the motor copy theories may have been different from that of mentalistic theories, but the logic was identical: Instead of Berkeley's "sensory impressions" being associated with "past impressions", one has stimuli being associated with other stimuli and with responses.

D. Contemporary Psychology

Needless to say, Cognitive Psychology did not reject the idea that incoming stimulation needed to be integrated into a whole. In 'snap-shot' theories of visual perception, for example, a successive series of retinal snap-shots (e.g., iconic memory images, codes) present themselves to the brain where they must be integrated (Gibson, 1979). As Neisser (1967) put it: "There must be an integrative process that transforms a succession of fleeting and discontinuous retinal snap shots into a stable perceived world" (p. 138). Another, more recent, example of mechanical integration is Anne Treisman's Feature Integration Theory, with which she asserts that: "...focal attention provides the 'glue' which integrates the initially separable features into unitary objects [i.e., percepts --cr]" (1980, p. 98).

All of these theories are based on elementarist assumptions; they postulate the existence of hypothetical integrative processes only because they first have broken down their objects into artificial elements.

Sometimes the elementarist assumptions are made explicit. Hence, in

perception theory, Norma Graham (1989) admits: "...just as with all the kings' men and Humpty Dumpty, current visual science knows much more about the parts than about the subsequent computations that put them back together again" (p. i). In a similar vein, when outlining the assumptions behind the information processing approach, Palmer and Kimchi (1986) write: "Any complex...informational event at one level of description can be specified more fully at a lower level by *decomposing* it into (1) a number of components...and (2) the temporal ordering relations among them..." (p. 39; original italics). Of course, given this decomposition, a need is then created for some integration process to put Humpty Dumpty back together again.

Bower (1971) demonstrates how assuming that the momentary retinal image is the basis of visual perception ends up creating the need for various hypothetical processes:

At the heart of the argument there is a genuine scientific question: how to account for the discrepancy between the richness of perception and the poverty of its apparent cause -- the momentary retinal image. First of all, there is the problem of space perception. The world as perceived seems to have one more dimension -- the dimension of depth-- than the retinal image does. Then there are the spatial 'constancies,' the tendency of an object to retain its size regardless of changes in viewing distance (even though the size of the retinal image changes) and to retain its shape even when its orientation (and therefore its retinal image) is changed. In other words, perception seems faithful to the object rather than to its retinal image. (p.85)

Bower solves these problems by rejecting the original assumption that perception is based on the momentary retinal image. The surprising ability of infants to detect higher ordered information, and their inability

to detect 'lower-ordered' information (e.g., retinal similarity) led Bower to conclude: "Rather than being the most primitive kind of perceptual ability, it would seem, the ability to register the information in a retinal image may be a highly sophisticated attainment and may indeed have to be learned" (p. 93). Notice that if we reject the retinal image as the basis of visual perception, then there is no need to postulate the various processes described above; the problem of converting a two dimensional image into a three dimensional one is only a problem if visual perception is based on the two dimensional retinal image. In the final chapter, we shall see how James Gibson (1966, 1979) rejected the momentary retinal image in favour of what he called 'invariants' in the flow of stimulation.

Whether an appeal to explicit homunculi, or an appeal to vague integrative processes, the logic is the same -- mechanical integration. But the problem is not restricted to theories of perception.

Cognitive approaches to developmental psychology also display the same logic of mechanical integration. Jean Piaget believed that a child's initial understanding of the world was solipsistic (Flanagan, 1986); the child existed in "a universe without objects...a world of pictures each one of which can be known and analyzed but which disappear and reappear capriciously" (Piaget, 1954, cited in E. Gibson, 1983, p. 22). The problem was to put these fleeting pictures together. During the course of development, objects become unitary only through a process of *construction*, whereby "permanence and meaning had to be understood as a process of developing schemata, at first separate and eventually *assimilated* into one" (E. Gibson, 1983, p. 21; added italics). In other words,

development is the problem of how schemata come to be 'built up' from less complex schemata.

Piaget's theory did contain hints of a less mechanical understanding of integration. Higher stages of development were said to presuppose the lower ones; hence, lower stages are integrated into the higher ones during the process of development itself. This less mechanical and more developmental understanding of integration is closer to what I will propose in the third chapter. However, even here Piaget falls back onto mechanical integration, and the integration of the higher and lower stages becomes, for Piaget, an accomplishment of mental operations inside the child's head: "... the child who understands conservation does so by integrating the concept of object permanency that he acquired at an earlier stage with the concept of reversible operations that he has just acquired" (Flanagan, 1986, p.60).

Other theories of concept formation have their roots in the assumption that, as Balzano and McCabe (1986) put it, "objects are bundles of features and...concepts are sets of objects sharing common features." Recent 'prototype' theories, after the work of Elenor Rosch, share a similar view. Although there are many variations of 'prototype' theory, many view categories as "represented in the mind in terms of prototypes (that is, best examples)" (Lakoff, 1987). Incoming stimulus information about an object must be "matched" to the prototype in order for the object to be classified. Objects, then, range in degree of "goodness of fit" to the prototype. Schemas, scripts, prototypes, etc. -- all must be built up from simpler elements through some kind of integration process.

In social psychology, Norman Anderson's (1981) Information Integration Theory is based on the belief that "Virtually all thought and

behavior has multiple causes, being integrated resultants of multiple sources of stimulus information" (p. 80). The multiple causes, "each pushing in its own direction", are "combined" to produce a behaviour. Behaviour is a result of "general-purpose integration processes" that can be described as "obeying" a "cognitive algebra": "The human organism frequently appears to be averaging, subtracting, or multiplying the stimulus information to arrive at a response" (p. 3). That these algebraic integration rules *exist* is demonstrated empirically: "*Evidence for averaging processes, in particular, has been found in so many situations that it seems reasonable to consider averaging as a basic capacity of adaptive behavior*" (p. 87; added italics). (Anderson may not be claiming that the models reflect exactly how the cognitive algebra is supposed to work in real organisms, but that the cognitive algebra exists is not questioned by him). Of course, the belief in mechanical integration is not original to Anderson; "*A view of the organism as an integrator of stimulus information is time-honored in traditional perception and forms a basic paradigm of modern judgment theory*" (p. 3; added italics). But Anderson takes mechanical integration to new heights: "The more general theory of stimulus integration includes, among other results, an extended network of empirical studies that have *placed cognitive algebra on a solid empirical foundation*" (p. 4; added italics). In more recent years, Anderson has applied his theory to other fields, such as development (Anderson, 1991).

Homunculus-style theories of mechanical integration also abound. In motor control theories, 'central executives' integrate muscle movements into actions using an 'action-plan' or 'motor programme' (see Stelmach & Requin, 1980). In theories of memory, 'central processing units' retrieve

files of information and make matchings. In theories of instinctive behaviour, genes are thought to 'govern' behaviour in an homunculus-like fashion (e.g., Tinbergen, 1951/1961). In Donald MacKay's (1987) Node Structure Theory, 'nodes' function to integrate perception with action, and language with other cognitive skills. On and on the examples go; they do not even stop at the boundaries of psychology, for, as we shall see in the next chapter, mechanical integration exists throughout biological theory as well.

II Summary: The Logic of Mechanical Integration

Whether 'associating' sensory and past impressions into a percept, or 'correlating' prototypic features into a concept, the logic is the same: previously existing parts or elements are put together by some external organizing principle or agent. Only the complexity of the particulars and the diversity of the labels have changed over time: external elements are now called 'features', mental elements are now called 'codes', associations are now called 'connections', homunculi are now called 'central executives', and bundles...well, bundles are still called bundles.

Integration has remained a central concept in almost every area of psychology. The single most hegemonic view of integration, as it exists in psychological theory, is that of mechanical integration. Despite its hegemony, however, there has been no direct investigation into this mechanical definition of integration.

Gestalt psychologists revealed the mechanicism of early associationist theories, and, indirectly, the mechanicism of the concept of integration: "Gestalt psychology flatly denies that elements pieced together, blended,

fused or associated in any way give the perceptions we actually experience" (Heidbreder, 1933). Unfortunately, the Gestaltists were unable to recognize mechanical integration in their own thinking. As a result, mechanical integration did not die with the attacks on elementalism by the Gestalt psychologists. Instead, it retreated from being an explicit theory (e.g., as in Mind-Stuff Theory) to being an implicit assumption; so implicit and so entrenched in psychological thought that the Gestalt psychologists themselves did not recognize it in their own theories. This is exactly the form in which it exists today -- as an ubiquitous, unquestioned assumption. There is a need, therefore, to expose the concept of integration as it is used in psychology -- to define it and give it a name.

As I have said, the name is 'mechanical integration,' and it results from a tense combination of two opposing assumptions: a reductionist belief in the primacy of the lower levels, together with a non-reductionist belief that the higher levels exist and must be accounted for. What results from this awkward reasoning are theories that account for the higher levels in terms of the lower levels. We shall see this same pattern of reasoning in the examples throughout this thesis.

III Overview of Thesis

In this thesis, I shall provide arguments and examples, from psychology and biology, to demonstrate the various *forms* of mechanical integration, the *problems* with mechanical integration, the *alternative* to mechanical integration, and the *application* of the alternative to psychology.

In chapter two, I demonstrate three problems with mechanical integration: the false analogy problem; the infinite regress problem

(already mentioned); and the degrees of freedom problem. In addition, I provide a classification scheme of mechanical integration theories, ranging from simple homunculus theories to hierarchical systems theories.

In chapter three, I demonstrate why Integrative Levels Theory is the correct alternative to mechanical integration. I use Integrative Levels Theory to re-define integration in non-mechanical terms, and provide examples of how this definition works. In the examples, I show how integration can be described and explained without appeal to external organizing principles or agents.

In chapter four, I discuss alternatives to mechanical integration in psychology. In the first half, I consider and reject the Neural Network approach as a possible alternative to mechanical integration. In the second half, I provide three examples of how the concept of integrative levels has been applied (implicitly, if not explicitly) to psychology: 1) the comparative psychology of integrative levels theorists; 2) the Gibsonian ecological approach, and; 3) the Activity Theoretical approach. I conclude with an appeal to integrate all three schools of thought.

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2: PROBLEMS WITH MECHANICAL INTEGRATION

Mechanical integration is the assumption that parts are put together mechanically by some organizing principle or agent. In this chapter, I argue that there are three problems with mechanical integration; 1) it is based upon a false analogy, i.e., it is based upon a mechanical analogy; 2) it leads to an infinite regress, i.e., it explains one unknown by appealing to a second (logically identical) unknown; 3) it runs into the 'degrees of freedom problem', i.e., there are more variables that need to be controlled (degrees of freedom) than there are variables that control (constraints).

In chapter one, I tried to provide a taste for the extent to which mechanical integration is used in psychology. However, most current theories using mechanical integration are more complex and more difficult to identify than the historical examples I provided in chapter one. Often, a theory will combine 'organizing principles' with 'organizing agents' to create an organizing homunculus that follows 'rules' or 'plans.' Even more complex theories organize homunculi into 'hierarchies,' or distribute them into 'networks.' Given this complexity, it would help to have a taxonomy of mechanical integration theories to identify this very old assumption where it may be hidden, in amongst the latest buzzwords and jargon.

In the section on the degrees of freedom problem, I provide a tentative taxonomy of mechanical integration theories.

I False Analogy: The Argument from Design

It is difficult to determine the exact reasons why the concept of mechanical integration is so popular. After all, no one has acknowledged the existence of the concept until now; it has been assumed implicitly and without question throughout the history of psychology. But one reason why its use seems so natural to the western psyche has to do with the *argument from design*.

That the organization of Nature stood as evidence for the existence of a Creator has been popular since ancient times (Hick, 1990). However, it is William Paley (1743-1805) who holds the honours of presenting the argument in the most concise of ways. In Paley's argument, he imagined walking in a desert place and stumbling upon a watch. The watch, so intricate in design, must have been made by a skilled watchmaker. When met with such complexity of design, we must assume the existence of a creator, the watchmaker. So too, Paley reasoned, when we look around us at the infinitely more complex design of Nature, must we *also* assume the existence of a Creator. Hick (1990) summarizes:

Paley argues that the natural world is as complex a mechanism, and as manifestly designed, as any watch. The rotation of the planets in the solar system and, on earth, the regular procession of the seasons and the complex structure and mutual adaptation of the parts of a living organism, all suggest design....Can such complex and efficient mechanisms have come about by chance...? (p. 24)

Although Paley's argument seems intuitively compelling, most philosophers of religion have discarded it (Hicks, 1990). Hume made three arguments against Paley, one of which deserves our attention: *underlying Paley's argument is a false (or, at least, weak) analogy.*

The argument from design is based on a mechanical analogy; i.e., the universe is likened to a mechanical artifact, a watch. "Only if the world is shown to be strikingly analogous to a human artifact, which we know to be designed, is there any basis for the inference to an intelligent Designer" (Hick, 1990, p. 25). Given a mechanical analogy, it is difficult to imagine anything other than an intelligent Designer organizing the universe. But, as Hume pointed out, one could equally have used an organic analogy "such as a crustacean, or vegetable" (Hick, 1990, p. 25). Such animals and plants grow (and, we now know, evolve) without the need for an Organizer. (I am not convinced, however, that viewing the universe as analogous to a crustacean -- or any other organism -- does not create an equal number of its own problems).

One would think that a failed theological argument would have little to do with a modern day cognitive scientist, or with a geneticist. Yet, it is precisely the same logic as in the argument from design that compels, so naturally, the psychologist and biologist to use the concept of mechanical integration. The argument from design, as it exists in science, is as follows: *That the mere existence of complexity, or organization, is itself evidence for some Organizer, or some vague Organizing Principle.* Hence, when presented with

the complexity of cognition, some Organizer is hypothesized to explain it. As in the original argument from design, such reasoning is based on a **mechanical analogy**. But would it not be more logical to explain the *organic* integration of organisms with an *organic* analogy?

The apparent self-evidence of certain ideas often precludes their critical consideration. And so it is that the existence of integrative processes in perception, or central executives in memory, seems self-evident. But, as I shall demonstrate, such inferences are based on an analogy that is both mechanical and false. We begin with an example from biology.

In biology, vitalism replaced the need for a God -- substituting Him for a 'vital entity' or 'vital force' (Beckner, 1972). As Beckner (1972) defined it, vitalism was a belief in "the presence in living systems of a substantial entity [or force -- cr] that imparts to the system powers possessed by no inanimate body" (p. 254). Vitalism was also opposed to the mechanistic theories of the time. Hence, vitalism attempted both to replace the need for a god, on the one extreme, and to oppose the mechanicism in biology, on the other.

However, the argument for vitalism, as it turns out, was no different than the argument from design, for vitalism, too, was based on a mechanical analogy. Only if one *accepts* the mechanical analogy of organisms will vitalism seem plausible. The more purely mechanical the description of organisms, the more it will seem that something vital has been left out. The physiology of an animal, for example, may be explained fully in terms of moving parts and their

connections, and yet there will still be something missing in such an account. In this way, vitalism was *parasitic* upon mechanicism and needed mechanicism in order to seem plausible.

Vitalism was not plausible enough, however, to last long after the turn of this century. This was, in part, a result of the decline of mechanicism in biology and its replacement by Cybernetics. Kalmus (1966) wrote: "The antithesis between holism and atomism, which at the beginning of the century was confused by the ideological quarrel between vitalists and mechanists, can now be resolved by the science of *Cybernetics* -- the study of systems, which appear goal directed and simultaneously have a transparently causal structure..." (p. 4; original italics).

In the argument from design, a mechanical analogy of the universe implied the need for a Designer; in vitalism, a mechanical analogy of the organism implied the need for a vital entity. In both cases, the existence of complexity was taken as evidence for some organizing entity or force. The universe cannot be explained in purely mechanical terms any more than a watch can; something, the Maker, will always be missing in such an account. But, as Hume suggested so long ago, the mechanical analogy is a false one.

As mentioned above, mechanicism in biology began to be replaced with cybernetics, which views organisms as complex systems.

The method with-which cybernetics explains the 'control' and 'regulation' of biological systems originates with the study of systems in engineering (Wilkins, 1966). Wilkins (1966) demonstrated the

logic of regulation and control with a description of James Watt's steam engine.

An example of a regulator, and also one of the earliest examples of a control system in engineering, is the steam engine governor made by James Watt. He wanted his engine to run at a constant speed irrespective of the load it was driving, but was faced with the fact that with a constant steam supply the speed of the engine falls as the loading increases. The governor makes the steam supply increase if the speed starts to fall below the desired value, and decreases if the speed starts to rise above it, thus turning the engine into an almost constant speed machine. (p. 14)

The 'governor', it must be added, regulates the amount of energy according to a set of 'instructions': "...the ultimate function of the controller is to control the supply of energy to the controlled system, in accordance with the instructions" (p. 15). The basic underlying logic of cybernetic systems, Wilkins tells us, is outlined in this example of Watt's steam engine: An input box (e.g., power supply) is connected to a control box (e.g., the governor), which reads instructions to produce an output box (e.g., a controlled release of energy). The task in biology is to find structures analogous to Watt's governor -- as in the control of body temperature despite variations in the ambient temperature (Wilkins, 1966; see also Mercer, 1981).

Cybernetics was purported to resolve the debate between vitalism and mechanicism in biology. It focuses on complex systems and describes them in the terms of engineering. But as I have tried to show, the problem with vitalism and mechanicism was that they

both were based on a false analogy. Surely, cybernetics is based on a more complex version of the same mechanical analogy. One could argue that cybernetics 'resolved' the debate between vitalism and mechanicism by including more complex versions of both; e.g., cybernetics describes organisms ("biological systems") as complex machines, and yet appeals to complex 'governors', 'regulators', 'controllers' and other such entities to explain them. By viewing the organism as a machine, the need for some organizing entity arises. (We shall see below how the genes are often thought to 'govern' behaviour by 'issuing commands' in an homunculus-like manner).

A more appropriate solution to the vitalism/mechanicism debate would have been to reject the mechanical analogy, upon which both were based, in favour of an organic one. Oddly enough, this is what Kalmus (1966) implies cybernetics has done. Consider the following: "...it should be observed that the modern restricted use of the word regulation in biology derives from modern technology and sociology but not from physics. Events in the *inorganic* world -- to which man-made machines do *not* belong -- though 'governed by laws' are not *conspicuously* regulated..." (p. 4; added italics). The implication, strange as it seems, is that machines are 'organic'; i.e., if man-made machines are *not inorganic*, they must be organic. I will leave it to the reader to ponder the appropriateness of applying the term 'organic' to a steam engine. Suffice it to say, 'man-made' machines may not be part of the 'natural' physical world, but that does not make them organic. Another, equally strange, implication is that, if organisms are like machines and machines are

'conspicuously regulated', then organisms too are so regulated!

Once again, as in the argument from design, the mechanical analogy necessitates the need for some kind of entity.

Perhaps what is needed is a fuller understanding of what the terms 'organic' and 'mechanical' actually mean. I must confess that I have no systematic answer to these questions. To the question of what are organic systems, the interested reader may refer to readings in non-equilibrium thermodynamics and dissipative structures (e.g., Prigogine, 1980; also, Kugler & Turvey, 1987, for an application to psychology). But I am undecided. I am wary of the close connection between traditional cyberneticists and those biologists interested in dissipative structures. I am also unsettled by their occasional references to abstract organizing principles: "Dissipation is the driving force of the Universe's building-up or *integrative tendency*; nature's forces give it form in space and time" (Wicken, 1984, p. 92; italics added). What exactly is the difference between this 'integrative tendency' and the old Organicist notion of an 'organizing trend' (see Novikoff, 1945)?

To the question of what is mechanical, Wolfgang Koehler's (1929/1947) treatment is one of the most comprehensive in the psychological literature, and deserves our attention. Arguing from the Gestaltist perspective, Koehler argued that organization can arise through natural processes, without the need for machine-like interventions. Koehler begins his treatment with the traditional answer to the question of how order arises.

Since the early days of science, wrote Koehler (1929/1947), people have believed that the forces of nature, if left to themselves, can produce only chaos and destruction. "On the other hand, we know that chaos can be prevented, and order enforced, if proper controls are imposed upon acting factors from the outside. As soon as man [sic] begins to restrict the possibilities of his choice, he can compel the forces of nature to do orderly work" (p. 104). Without the "rigid arrangements" of the type found in machines, there can be no direction to the forces of nature. Thus, according to this mechanical view, order in the universe is a result of what Koehler called "rigid" or "topographical" arrangements, rather than "dynamic determinants" (p. 107). These arrangements rigidly constrain the natural forces, and direct them in an orderly fashion. Heidbreder (1933) put the mechanists' reasoning succinctly: "Reasoning on the analogy of their own machines, they assume that if natural forces are to produce any kind of order, they must be directed along fixed lines marked out by special arrangements" (p. 357).

It is this method, in which one seeks out rigid arrangements to explain order, that characterized mechanism in the psychology of Koehler's day. In this psychology, the nervous system was thought to have a rigid architecture, in which impulses flow along fixed pathways to definite, isolated conductors. "A system so conceived is obviously an affair of special arrangements; and as in man-made machines, the special arrangements -- particular fiber-tracts,

particular nerve-centers, and the like -- are the decisive factors" (Heidbreder, 1933, p. 358).

Under this scheme, the task is to discover what reactions (output), goes with what stimuli (input) -- the rest is just a matter of the proper anatomical connections:

In the machine theory of the nervous system, connections between cells in the brain and effector organs (such as muscles) are of the same type as the connections between points of sense organs and those cells. Under these circumstances, a thoroughly adequate formula for research in psychology will be as follows: we have to discover what reactions in the effector organs go with given stimuli. This is the well-known *stimulus-response formula* which has for long enjoyed considerable prestige in American psychology. (Koehler, 1929/1947, p. 114; original italics)

Vision, learning, and memory, Koehler (1929/1947) observed, were all explained in the early mechanical theories in terms of rigid arrangements in the nervous system. Visual perception is a particularly good example. The forming of orderly perceptions out of the chaotic flow of stimulation (the "blooming, buzzing confusion") was a matter of forming the proper tracks. These neural pathways constrain the flow in an orderly fashion. Hence, the order in perception "...is a matter of anatomy rather than of factors inherent in the flow" (p. 111). (In contrast, see the final chapter for Gibson's theory of 'invariants' in the flow of stimulation).

The tracks and their connections can and do change with experience, however.

...psychologists have said that in some parts of the nervous system, for instance between its visual and auditory sectors, pathways are not fixed once for all in early youth. According to this view, either no paths are at first ready for conduction, or else from one point of the tissue several paths conduct processes equally well in several directions, so that no particular order is prescribed. (p. 111)

Order is eventually worked out when certain of the paths and their connections are strengthened through association: "Where at first no conducting paths, or several paths of equal conductivity, were given, learning has singled out one path by making it better conducting than all the others" (p. 111). Eventually, as more learning occurs and as more paths are strengthened, a network of such paths begins to emerge.

The view of the nervous system presented above is a result of a strictly mechanical analogy: "...if the first tentative picturing of nervous functions uses analogies of only one kind, the machine type, then other analogies have probably never occurred to the theorists" (Koehler, 1929/1947, p. 116). But Koehler argued that this is not the only way to obtain order -- nor is it even the most common. Rather, the universe is full of examples in which order is achieved through "dynamical interactions". That is, order arises out of "the interaction of physical forces themselves" (Heidbreder, 1933, p. 356). The solar system, for example, achieves its order without any visible machinery: "here there are no rigid arrangements -- no 'crystal spheres' like those of Aristotelian astronomy, no structures at all, in fact -- to keep the planets in their courses" (Heidbreder,

1933, p. 357). The man-made machine, in contrast, is only a special case.

The Gestatists approached psychology from a dynamical perspective, insisting that order in the nervous system appears without special arrangements:

The Gestaltist does not think in terms of *systems of fibers connecting* particular sensory areas with particular motor organs through fixed pathways in the central nervous system. He believes that *no matter how elaborate and numerous the possibilities of connection become*, they are inadequate to explain experience as it is. He thinks rather in terms of patterns of shifting stresses and strains like those which make the hole in the soap-film a circle. The nervous system, he insists, or rather the nervous system and its appendages, must be taken as a whole. (Heidbreder, 1933, p. 358; italics added)

Unfortunately, when confronted with the choice of rejecting the mechanical analogy, Koehler (1929/1947) did not choose an organic analogy in its place. Instead, his choice of "dynamical interactions" ends up looking rather like a complex form of mechanicism. Koehler invites the reader to consider a drop in a current of water as an example of dynamical interaction. If water in a pipe is constrained to flow in one direction, Koehler tells his readers, then this is an example of topographical or rigid arrangements. However, suppose

...that there is no pipe, and that the drop now becomes part of a much larger volume of water. In its new environment the drop will probably also move. But in this situation it is exposed to many gradients of pressure, and its movement will be in the direction of the resultant gradient....In the new situation, any drop within the current takes its particular

course for *dynamic reasons*; it follows the resultant force at each moment and in all places. (p. 127; added italics)

The forces acting upon the drop may be more numerous, less rigid, and farther apart, but this complexity does not make the example any less mechanical. Describing organic processes in terms of "patterns of shifting stresses and strains" striving for a "balance" or "equilibrium", sounds more like cybernetics than a true alternative to mechanicism. Unfortunately, it is difficult to imagine "forces acting by themselves", and, as a result, Koehler's examples end up looking an awful lot like what he is trying to oppose -- i.e., the mechanical analogy. Koehler, like the cyberneticists that followed, did not reject the mechanical analogy -- he merely adopted a more complex one.

My suggestion is to drop the mechanical analogy altogether, and start anew with an organic analogy. But what form should this analogy take? And how far should the analogy be carried? Obviously, the application of an organic analogy to the universe (e.g., the Gaia Hypothesis and the Anthropic Principle) would be no more justified than the application of the mechanical analogy to organisms.

In the second half of chapter three, I shall provide 'organic' examples of integration from biology and psychology. The hope is to explain integration, using these examples, without appeal to external organizing agents or organizing principles.

Dynamic forces ought not to be conceptually separated from 'constraints,' as Koehler tried to do. Constraints are not just

structural -- like the 'rigid arrangements' of machines. Instead, there also exist functional and historical constraints, which are best understood not as 'forces acting by themselves' but as relations. Integration results from the formation of structural, functional, and historical relations that span many levels and cannot be reduced to lower levels. But this is getting ahead. I now turn to two other problems with mechanical integration: the infinite regress problem, and the degrees of freedom problem.

II Infinite Regress

In homunculus theory, the question as to how the brain is able to integrate information is answered by appealing to an homunculus, or its equivalent. But, surely, answering how one entity (the brain) integrates by appealing to a second entity (the homunculus) merely pushes the need for explanation one step further. To state that an hypothetical homunculus is able to read two types of 'codes', for example, and integrate them gets us no further than saying that the brain does so. Furthermore, if we *stick to the logic of mechanical integration*, and explain the first homunculus by appealing to yet another one, then we end up with an infinite regress. Flanagan (1986) demonstrates how the Master Homunculus theory (see chapter one) ends in an infinite regress:

This is easy to see: suppose I assert that the Master Homunculus is located in area *Omega* of my left cerebral hemisphere, or alternatively that it is located in my soul. I can now be asked how I comprehend or am conscious of either of these facts. *But according to the logic that gave rise to the postulation of a Master Homunculus in the first place* [italics

added -- cr] I cannot say that I comprehend that area *Omega* is the seat of CML [Conscious Mental Life -- cr] *with* area *Omega*, or that I comprehend that my CML is realized in my *soul with my soul*, because then there is no comprehender or bystander outside the system. The infinite regress is then generated by the requirement that we need a new bystander for each higher-level act of self-comprehension. (p. 43; original italics)

One possible solution to the infinite regress problem was proposed by Dennett (1978). Dennett opts for a 'nested' or 'hierarchical' model, in which the burden of processing is shifted from the homunculus to smaller and more stupid homunculi. Eventually, the task is 'distributed' throughout a system of homunculi so stupid, that they merely turn off and on -- like neurons.

If one can get a team or committee of relatively ignorant, narrow-minded, blind homunculi to produce the intelligent behaviour of the whole, this is progress. A flow chart is typically the organisation chart of a committee of homunculi...; each box specifies a homunculus by prescribing a function *without* saying how it is to be accomplished (one says in effect: put a little man in there to do the job). If we then look closer at the individual boxes we see that the function of each is accomplished by subdividing it via another flow chart into still smaller, more stupid homunculi. Eventually this nesting of boxes within boxes lands you with homunculi so stupid (all they have to do is to remember to say yes or no when asked) that they can be, as one says, "replaced by a machine". One *discharges* fancy homunculi from one's scheme by organising armies of idiots to do the work. (1978, p. 123; original italics)

But Dennett's is a philosopher's solution -- an unimaginative transfer of capitalistic Taylorism. In chapters three and four, I will

provide a more scientific solution. However, Dennett's 'solution' does introduce the next problem with homunculus theories -- the degrees of freedom problem. The problem is to shift the number of variables -- or 'degrees of freedom' -- that the homunculus must control onto other homunculus-like entities.

Despite the problems with homunculus theories, they still exist throughout psychology and biology. Although straightforward homunculus theories (e.g., the Master Homunculus theory) may be fewer in number, one could say that homunculi have returned better organized -- into 'committees' and 'hierarchies'. For this reason, it is important to take a look at these more complex homunculus theories, and at why they are problematic. In the next section, I provide a taxonomy of mechanical integration theories.

III Degrees of Freedom Problem and a Taxonomy of Mechanical Integration Theories

Michaels and Carello (1981) provide a concise summary of the degrees of freedom problem as it exists in theories of motor control. Essentially, the problem arises when there are too many variables (or degrees of freedom) for the controller (or homunculus) to handle. But Michaels and Carello's treatment also provides a useful taxonomy of the types of models that follow the mechanical integration logic. It is a classification scheme that begins with simple homunculus theories, and ends with the most complex, hierarchically arranged, homunculus theories. Their treatment, I believe, has implications far beyond motor control theories, to almost all areas of

psychology and biology. For this reason, I will use it as an outline, and add to it my own elaborations and generalizations.

Simple Homunculus Theories. According to Michaels and Carello (1981), the classical view of motor control claims that a motor executive writes an 'action plan' based on inputs that it has read. Although theories may differ on the details, they all share the same logic: "A plan must exist in order that an act can be executed" (p. 135). Michaels and Carello cite R. A. Schmidt's schema theory as a classic example:

It is also assumed that there are "generalized" motor programs formed in the central nervous system that contain stored muscle commands with all of the details necessary to carry out a movement. The program requires response specifications that determine how the program is to be carried out (e.g., rapidly, slowly, etc.). Given the response specifications, the program can be run off, with all of the details of the movement determined in advance. (Schmidt, 1976, p. 46, in Michaels & Carello, 1981, p. 135)

But such theories encounter an immediate problem. As the Soviet physiologist-mathematician N. A. Bernstein pointed out, an action involving many degrees of freedom (e.g., joints, muscles) could never take place simply by efferent motor commands (Luria, 1979). This is called the 'degrees of freedom problem', which "...arises because the number of things in need of regulation exceeds the number of things that can regulate. That is, the executive has fewer degrees of freedom than the mechanical degrees of freedom it must control" (Michaels & Carello, 1981, p. 137). The

computational load, produced by the enormous number of variables, is too great for a single homunculus to handle.

Simple Hierarchies. One solution is to partition the computational load in an hierarchy. This passes the burden of regulating some of the degrees of freedom onto subunits or subprogrammes: "If commands could be issued to these units which, in turn, would take care of the finer details of the act in some stereotyped fashion...[then]...the degrees of freedom that would have to be regulated would be reduced" (p. 138). This style of organization is sometimes called a "command hierarchy" (Muir, 1982).

It is important to note that command hierarchies do not necessarily imply 'top-down' causation. This is because command hierarchies do not always reflect levels of integration; rather, they reflect the *direction of control*. Thus 'hierarchy' is a metaphor for how a control system is organized into a group of homunculi. The commander atop the hierarchy could very well exist on a lower level of matter. In the Watson-Crick model of genetics, for example, DNA is the commander issuing the commands that will lead to the forming of proteins, cells, and eventually whole organisms. In this case, the command hierarchy is clearly 'bottom-up'. A command hierarchy, then, should not be confused with levels of integration.

One way subunits in a hierarchy can be formed is through physical linkages. Micheals and Carello (1981) provide an example of how mechanical linkages in an airplane reduce the degrees of freedom that a pilot must control.

Two ailerons and the rudder are (sometimes) mechanically linked into a 'collective' so that one command from the pilot affects all three mechanisms. The particulars of what the individual members of the collective do are standardized to the kinds of commands they receive (e.g., one possible command could move the left aileron up, right aileron down, and turn the rudder to the left). (p. 138)

Linking the rudder and the two ailerons reduces the degrees of freedom from three to only one. The same linking of muscles and joints into collectives, also reduces the degrees of freedom that need to be controlled. Although containing several variables (e.g., muscles and joints), each collective can be treated as a single degree of freedom. Similarly, command hierarchies seem to reduce the degrees of freedom by shifting the burden of control onto subunits.

There are two problems with simple command hierarchies. First, there is the problem of how the subunits come to be collected, or integrated. If the integration is explained, as it often is, by appealing to yet another 'integrator', then the homunculus problem is reintroduced. If, on the other hand, the hierarchical organization arises out of physical relations (i.e., without the need of an integrator), then why, in principle, can't the whole system arise in such a way? Why appeal to an homunculus at all, if we can already explain integration in terms of structural and functional relations? In short, the very process of collecting variables into standard units involves degrees of freedom that must be controlled for (Michaels & Carello, 1981).

The second problem, as Michaels and Carello explain, concerns the rigidity of command hierarchies. Co-ordinated

movement is much more flexible than the rigid arrangements of command hierarchies allow. Without 'feedback', for example, the action-plan cannot be corrected for variability in the environment. There are many sources of variability not accounted for by the simple command hierarchy (e.g., changes in body posture, and changes in the environment). For example, the constantly changing posture of the body and state of the muscles, ensures that a single command may have different results depending on the state of the body. "There cannot be a pat command for 'step over'; the plan must take into account current states in the motor machinery and precise conditions of the environment..." (p. 143). A standardized set of programmes would fail in the presence of such variability.

The command hierarchy model is not limited to motor control theories. It exists wherever there persists the logic of a controller using a plan or programme to send commands. This includes several areas of biology, including embryology, genetics, ethology, and evolution theory. As Longuet-Higgins observed back in 1969: "The problems of biology are all to do with *programs*. A program is a list of things to be done with regard to circumstances" (cited in Boden, 1984, p. 325; original italics).

In the classical 'instinct theories', instincts were supposed to consist of a set of behaviours, programmed by biology, that were 'released' or 'triggered' by environmental stimuli. In Tinbergen's (1951/1961) theory of 'Innate Response Mechanisms', for example, "each motor centre controls a configurational pattern of muscle contractions" (p. 108) which are set off by environmental stimuli.

The stimulus situations, according to Tinbergen, were "arranged in a hierarchical system, like the staff organization of an army or other human organization" (p. 108).

In entomology, the same logic can be seen in the sociobiological explanations of insect colonies. E. O. Wilson presented the problem as it he understood it:

The individual member of a large colony cannot possibly perceive the actions of more than a minute fraction of its nest mates; nor can it monitor the physiological condition of the colony as a whole. Yet somehow everything balances out, a fact that keeps drawing the mind back to Maeterlink's [1905] poetic question about the termite colony: "What is it that governs here, that issues orders, forseees the future...?" (Wilson, 1971, p. 226)

Wilson's solution (unlike Maeterlink's, who appealed to a 'super organism') was that the genes *programme* the insects with a repertoire of behaviours "...not in a way that permits them...to execute a series of predestined maneuvers like an elementary servo-mechanism, but rather in a way that causes them to respond with certain behaviors, in accord with certain probabilities..." (p. 226). To which Wilson adds "...that not only the acts themselves are programmed but also the frequency in which they occur" (p. 226). Again, this is an hierarchical model, where one level issues commands according to a plan that affects higher levels.

This line of thinking, in which an organizing entity issues commands resulting in some organizing process, underlies what Crick called the "central dogma" of the new biology: "that DNA makes RNA and RNA makes protein [and protein makes cells, etc.

-- cr], in a one-way flow of information, a unidirectional process of mechanical construction" (Gould, 1987, pp. 157-158). In other words, the complex organisation of organisms is "...explained, ultimately, as the translated products of an inherited program coded by a simple mechanism..."(p. 157). This is a one-way command hierarchy, where-by the organism is 'governed' by the genes. But like the motor control theories that share the same logic, the one-way logic of the 'central dogma' also has been challenged. More recent findings in genetics, notably the work of Barbara McClintock on 'transposable elements', show that there is indeed a feedback loop. The DNA molecules actually respond to 'environmental insults' (Ho, 1988). This violates "the central dogma, as well as Weismann's barrier, which specifically 'forbid' information passing from body (soma) to germline" (Ho, 1988, p. 103).

In short, a set of new themes -- mobility, rearrangement, regulation, and interaction -- has transformed our view of genomes from stable and linear arrays, altered piece by piece and shielded from any interaction with their products, to fluid systems with potential for rapid reorganization and extensive feedback from their own products and other sources of RNA. The implications for embryology and evolution are profound, and largely unexplored. (Gould, 1987, p. 159)

To summarize, command hierarchies are models in which control is distributed to subunits that follow a sub-programme, or stereotyped behaviour. By distributing the task of control onto other subunits, command hierarchies reduce the burden of control, regulation, or computation for the controller. Unfortunately, the

one-way causal logic of command hierarchies makes them inflexible, slow and unrealistic.

Hierarchies with feedback. A style of organization that is more successful with the degrees of freedom problem is the 'hierarchy with feedback' -- also called a 'control hierarchy' (Mercer, 1981). This is basically a command hierarchy that allows for feedback loops to pass information, correcting any mistake in commands. This helps solve the problem of unpredictable context-variation: "Because it conveys information about what the variables have done...feedback can presumably be used to adjust the action plan to factors unforeseen when the plan was written (e.g., as when a hidden rock causes one to stumble)" (Michaels & Carello, 1981, p. 142). In addition, subunits are not rigidly restricted to a single standardized function: "Unlike a strict hierarchy, the whole system need not be informed. No one variable is solely responsible for a particular function; a number of subsystems could be *functionally equivalent*" (p. 142; added italics).

The Soviet neuro-psychologist A. R. Luria (1979) had a very similar view, which he borrowed from Bernstein:

...movement is in principle uncontrollable simply by efferent impulses. For a movement to take place, there must be constant correction of the initiated movement by afferent impulses, which give information about the position of the moving limb in space and the change in muscle tone. This complex structure of locomotion is required to satisfy the fundamental conditions preserving the invariance of the task and its performance by variable means. (p. 127)

In other words, afferent impulses can help control for variation. In such a scheme, there must be a constant flow of checking or correcting responses that help co-ordinate movement. (In the next chapter, we will see how Luria was able to expand his ideas beyond the 'hierarchy with feedback' model). This feedback seems to be essential for the brain's control of movement. But the feedback hierarchy model is not restricted to motor control theories.

Recall that recent interest in transposable genes is usually couched in the terms of the hierarchy with feedback model. For example, 'homeotic mutations' -- developing normal parts in the wrong places, such as wings where antennae should be -- are often explained using the classic control hierarchy. This is explicit in the following quotation from Gould (1984):

...some master regulator must be responsible for turning on the homeotics [i.e., homeotic genes] at the right time and in the right place, for we know that many homeotic mutations are mistakes in placement and timing. Perhaps this master regulator is no more than a gradient of some substance running from the front to the back end of a larval fly; perhaps the homeotic regulators can "read" this gradient and turn on in the right place by assessing its concentration. In any case, we have three hierarchical levels of control: the structural genes that build different parts in each segment, the homeotic regulators that switch on the blocks of structural genes, and the higher regulators that turn on the homeotic regulators in the right place and at the right time. (1984, pp. 195-196)

Many biologists (e.g., Eldredge, 1988; Salthe, 1985; Gould, 1984; Mercer, 1981) are now proposing the hierarchical systems model as a new way of thinking. Theorists stimulated by the model

have applied it to the domains of genetics, embryology, and evolution theory.

Returning to Michaels and Carello, we begin to see why the hierarchical systems model is problematic. Motor control theories tend to restrict the information from feedback loops to the *consequences* of an action. This means that feedback is reactive, and that the information fed back is often too late.

If feedback indicates an error, the plan is modified. But feedback in this strict sense is essentially reactive; the act is carried out and *then* checked. Such feedback could inform the organism only of conditions that no longer exist...But the organism does not need to know that it has tripped; that is unalterable and no longer relevant. The real need is for information about how to recover without falling - feed *forward*, so to speak (Greene, 1972; Turvey, 1977).

Registering error or even direction of error is not enough. It simply says that some or perhaps all of the elements are wrong. It does not carry information about which elements to fix or how much to fix them. Indeed, the same error signal could be caused by any number of combinations of elements all doing the wrong thing to varying degrees. (1981, p. 143; original italics)

But if the controller computes the where and how of correcting a mistake, then the computational load increases again and the degrees of freedom problem is reintroduced. "Unless such surprises are somehow anticipated in the design of the act, feed back cannot control for them" (Michaels and Carello, 1981, p. 143). It would seem that the model would need either an infinite number of anticipatory plans, or an infinite number of feedback loops -- either way, the computational burden is too large.

Going beyond motor control theories, there are more general problems with hierarchical models. The triangular shape of hierarchies imply a narrowing to one all-important, controlling level -- the 'seat' of the homunculus. Whether a 'top-down' or a 'bottom-up' hierarchy, one level is always considered to be more fundamental than the others; one cannot attribute an equal status to each in an hierarchical model. Models that include both top-down and bottom-up loops (e.g., feedback hierarchies) do not really resolve the top-down/bottom-up dichotomy. These models still require that the influence of one causal direction (say, top-down) be fundamental (i.e., controlling), while the influence of the other causal direction (say, bottom-up) be subordinate.

That one level is the most fundamental -- the end point -- implies that hierarchical systems are *closed systems*. In fact, the feedback loop is sometimes said to be a "closed loop" (Wilkins, 1966). One cannot go beyond this endpoint, then, if the model is that of an hierarchy. Consequently, all the degrees of freedom must be controlled *within* the hierarchy. With such a computational burden on the uppermost level, it is difficult to imagine anything but an intelligent entity occupying its space.

And so it appears that even the most complex of hierarchy theories do not avoid the degrees of freedom problem. In an hierarchy, there always exists a seat on the top, reserved for a central executive to sit and issue commands. This executive has a big job, since no other source of control, or 'constraints', is allowed outside the hierarchical system itself, even though many sources of

variation, or 'degrees of freedom', constantly impinge from the outside. Once again, as I tried to show in chapter one, homunculi theories are adopted when-ever and where-ever integration is defined mechanically -- as the putting together of previously existing parts by some external agent or principle. In the next chapter, I shall provide a non-mechanistic definition of integration, and some examples of how the definition might work.

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3: INTEGRATIVE LEVELS THEORY

In chapter two, I showed the problems with mechanical integration and how widespread the problems are. In this chapter, I want to show that there is a solution to the problems; there is an alternative to mechanical integration that avoids the problems of both reductionism and holism. I shall also try to make 'integrative levels theory' much clearer than it is in the existing literature. First, I discuss the origins of the integrative levels concept, and describe its main features. Second, I try to formulate a new definition of integration -- one that does not include an external organizing force or agent. Third, I describe integration in terms of structural, functional, and historical relations, and describe each kind of relation in turn, providing examples from psychology and biology.

I The Integrative Levels Approach

According to Tobach and Greenberg (1984), biologists such as Woodger (1929) used the term "integrative levels" to refer to a structural hierarchical arrangement of matter. In psychology, such emergentists as C. Lloyd Morgan (1923) and Jan Smuts (1926) were also referring to "levels of integration" and "levels of organization". Smuts even went so far as to argue against the traditional (metaphysical) emergentist notion that new levels were 'predestined', or 'foreshadowed' prior to their actual historical

emergence (Salthe, 1988). By 1937, Joseph Needham, a biochemist interested in evolutionary theory, had formulated most of the basic premises of Integrative Levels Theory (Salthe, 1988; Tobach, 1991). However, Needham apparently had not formulated the concept clearly enough to prevent it from continually being used in several different ways (Aronson, 1984). The first clear statement on the integrative levels approach came with Novikoff's (1945) publication in Science (Tobach, 1991).

Novikoff, also a biochemist, used the concept of integrative levels to criticize both the reductionistic and the holistic approaches of the time. The levels approach "...neither reduces phenomena of a higher level to those of a lower, as in mechanism, nor describes the higher in vague non-material terms..." (p. 209).

Integrative Levels theory is not an extreme holism. Phenomena existing on the lower levels cannot be explained fully by the laws governing the higher levels. While Novikoff (1945) praised some of the holists of his time -- called 'organicists' -- for resisting the pitfalls of reductionism, he also criticized them for allowing non-material concepts to enter their theory: "They [i.e., organicists] try to describe the behavior of the organism solely in terms of the higher level, the whole. As a result the impression is created that no material basis exists for the part-whole relation" (p. 210). The organicists appealed to such mystical terms as "super organisms" to explain insect colonies, or "organizing trends" to explain progress in evolution; "Progress in organic evolution occurs without a *plan* or direction..." (Novikoff, 1945, p. 214; added italics).

At the same time, Integrative Levels Theory is not reductionist either. Each level is qualitatively distinct, has its own set of laws, and requires its own methods of study: "Although some may have validity for the higher level, laws of a lower level are inadequate to describe the higher level. The laws unique to the higher level can be discovered by approaches appropriate to the particular level; to do otherwise is invalid scientifically..." (Novikoff, 1945, pp. 214-215).

Levins and Lewontin (1985) describe how holism and reductionism are opposite sides of the same style of thought. After pointing out that holistic notions, such as the "Clementsian super organism", are idealistic, Levins and Lewontin write:

Idealism [here, holism -- cr] and reductionism in ecology share a common fault: they see "true causes" as arising at one level only...Clementsian idealism sees the community as the only causal reality, with the behaviors of individual species populations as the direct consequence of the community's mysterious organizing forces. One might *describe* the community for some purpose by giving a list of species abundances, but that description is of eiphenomena only. Reductionism, on the other hand, sees the individual species, or ultimately the individual (or cells, or molecules, for there is no clear stopping place in the reductionist program), as the only "real" objects, while higher levels are again descriptions of convenience without causal reality. A proper materialism, however, accepts neither of these doctrinaire positions but looks for the actual material relationships among entities at all levels. (1985, pp. 135-136; original italics)

This "proper materialism", of course, is Integrative Levels Theory, which sees "the various levels of organization as partly autonomous and reciprocally interacting" (Levins & Lewontin, 1985, p. 288).

The concept of integrative levels should not be confused with 'interactionism' (Lewontin et al, 1984; Levins & Lewontin, 1985; Tobach, 1991). Lewontin, Rose, and Kamin (1984) explain how Roger Sperry's use of interactionism in neuro-psychology is problematic:

It is this integration which is misunderstood by interactionism.... Consider Sperry's argument, referred to above, for "downward causation." If all he is saying is that, within a complex whole, the degrees of freedom available to the component parts are differently determined than if the parts were isolated monads, he is obviously right. But it is clear he means more than just this. He means that there are two types of *radically incommensurable* causes determining the behavior of the parts of any system. Some run "up" as when the interdigitation of muscle proteins "cause" the muscle to contract. Others run "down" as when the instruction "jump" causes the contraction. Presumably causes pass one another as they cross levels, like commuters going respectively up and down on parallel escalators. The image conveys the paradox which is always present at the heart of such dualism, for how can ontologically different types of causation each produce an identical set of results? Perhaps it is for this reason that when faced with real methodological challenge Sperry-type holism collapses so easily into hard-nosed reductionism. (pp. 281-281; italics added)

I will summarize the integrative levels approach by giving the final word to Novikoff (1945):

The concept of integrative levels stresses the need to study living organisms at all levels -- cells, tissues organs, organ systems, organisms, and populations. It is not "organistic"; always the reciprocal relationship of elementary units to each other and to the unit system as a whole must be studied. It is not mechanistic; the detailed methods of study at higher levels will include not only some used at lower levels but new methods peculiar to the higher levels..." (p. 211)

II What 'integration' means

Before we can define integration, we first must look at how wholes and parts emerge. The integrative levels approach is distinguished from reductionism, because it does not view parts as having primacy over wholes. Conversely, the approach is also distinguished from holism, because it does not view wholes as having primacy over parts. Rather, integration is best described as a dialectical process: It is the formation of parts into wholes -- and, simultaneously, the formation of wholes into parts. How does this work?

A distinguishing feature of the integrative levels approach is that it views the part-whole relation as reciprocal: "What were wholes on one level become parts on a higher one" (1945, p. 209). Neither parts, nor wholes, can be viewed in isolation -- as reductionism and holism, respectively, do. This is different from saying that the whole is 'more than the sum of its parts'. Such statements give the impression that 'the whole' is something *in addition to* and other than the parts. The reciprocal nature of the part/whole relation is clear in the treatment from Levins and Lewontin's The Dialectical Biologist (1985), which I quote here at length:

...in general, the properties of parts have *no prior* alienated existence but are acquired by being parts of a particular whole. In the alienated world [of Cartesian reductionism - cr] the intrinsic properties of the alienated parts confer properties on the whole, which may in addition take on new properties that are not characteristic of the parts: the whole may be more than the sum of its parts. But the ancient debate on emergence, whether indeed wholes may have properties not

intrinsic to the parts, is beside the point. The fact is that the parts have properties that are characteristic of them only as they are parts of wholes; the properties come into existence in the interaction that makes the whole. A person cannot fly by flapping her arms, no matter how much she tries, nor can a group of people fly by all flapping their arms simultaneously. But people do fly, as a consequence of the social organization that has created airplanes, pilots, and fuel. *It is not society that flies* [i.e. 'the whole' --cr], however, but individuals in society, who have acquired a property they do not have outside society. The limitations of individual physical beings are negated by social interactions. The whole, thus, is not simply the *object* of interaction of the parts but is the *subject* of action on the parts. (p. 273; added italics).

Given that parts have no prior existence *as parts*, integration is not so much the making of parts into wholes, but *the making of wholes into parts to form a new whole*. Parts do not exist before wholes; they exist as wholes and *become* parts of a greater whole when they enter into a definite set of relations. We can now formulate a working definition of integration that avoids the 'homunculi' of reductionism, and the 'organizing principles' of holism: *Two things become 'integrated' when they enter into a set of relations, such that they act or function together as parts of a single system.*

I argue that this 'entering into a set of relations' is the most defining feature of integration. To support this argument, though, and the new definition of integration, it is important to say what is meant by 'relations'. I now turn to this problem, and provide examples of three kinds of relations: structural, functional, and historical.

III What 'in relation' means

In the previous section, I tried to show how 'entering into a set of relations' is a defining feature of the concept of integration. But what are 'relations', and how should we understand them? In this section, I classify three kinds of relationships: structural, functional, and historical. In the following, I provide examples of the three types of relations, and discuss how they explain the emergence of integration. In the examples, integration is explained without any reference what-so-ever to an external organizing principle or agent.

Structural relations. Structural relations are easier to visualize than functional and historical relations, and consist of such things as arrangement, shape, and size. The Gestaltists' Laws of Organization, for example, were really descriptions of structural relations. The so-called principle of *Praegnanz*, or 'good form', is a description of how arranging elements, such as dots or lines, can form 'perceptual groups'. For example, four dots arranged in a particular way can form a square. What is important, here, is that structural relations do not exist on the level of the elements themselves. The dots "cannot be said to add up to a square because if the locations of the dots are changed the perception of squareness is destroyed" (Kendler, 1987, p. 204). In the next chapter, we shall see why these relations are not 'in the head', and are not the result of organizing principles. For now, consider Gibson's (1979) interpretation of the Ganzfeld effect.

Metzger's Ganzfeld, or "total field", effect (Gibson, 1979; Reed, 1988) demonstrates the importance of levels of structure for

the *integrity* of perceptual information. This effect results from looking at a homogeneous field of light -- in this case, either a dimly lighted plaster wall or diffusely translucent eye-caps (Gibson 1979; Reed, 1988). According to James Gibson (1979), by using diffusely translucent eye-caps:

The structure of the entering light, the optical texture, can thus be eliminated at any level of intensity. What my observers and I saw under these conditions could better be described as "nothing" in the sense of "no thing."....There was no surface and no object at any distance. Depth was not present in the experience but missing from it. What the observer saw, as I would now put it, was an empty *medium*. (p. 151; original italics)

In the above experiment all the receptors were stimulated, yet no true perception took place. No information, except perhaps 'illumination', is contained within a homogeneous field of light. What the observers experienced may be described as a kind of blindness: "The observer is blind, not to light, for the photoreceptors are still stimulated, but to the environment, for the ocular system is inactivated; its adjustments are frustrated. The observer cannot *look at or look around...*" (Gibson, 1979, p. 152; original italics).

This raises the question of what *is* needed for perception? Gibson (1979) writes, "Metzger's experiment points to the distinction between an optic array with structure and a nonarray without structure. To the extent that the array has a *structure* it specifies an environment" (p. 151; added italics). But the structure does not exist on the level of the photoreceptors. The traditional theory of perception was distorting because it restricted its methods to the

lowest level. Note that, if the stimulation has *integrity* on the higher level, then there is no need for an *integrating* process.

What can be said of perception, can also be said of living organisms. The integrity of living organisms also relies on structural relations. The relationship between size and shape is one of the most important in biology. The relative strength of certain forces, such as gravity, varies with the size of an animal, and its shape must vary accordingly (Gould, 1977). For example, the shape of a leg bone "of a large animal must thicken disproportionately to provide the same relative strength as the slender bone of a small creature" (p. 173). Many functions that depend on surface area also require particular size-shape relationships. Small mammals, for example, do not have villi in their small intestines, since the surface area to volume ratio is sufficient already to absorb nutrients. Simply put, the organization and function of organisms vary with size.

To summarize, structural relations are important for the integration, or rather integrity, of an object. Structural relations do not exist on the level of 'elements'. For example, the integrity of perceptual information relies on structural relations, but this structure does not exist on the level of receptor units. Structural relations are also important in biology for describing the integrity of organisms, where the shape and function vary with size.

Functional relations. The integrity of living organisms does not rely on structural relations alone. There exists complex functional relations, without which integration would be impossible.

Functional relations are not as easily visualized as structural relations, but they are no less real. Like the physical linkages among muscles, bones, and joints that together make up coalitions, *functional linkages* also make up groups. For example, the wrists, although not physically connected, become functionally linked in the act of drumming (von Holst, cited in Michaels & Carello, 1981). Groups of such functional linkages are called **functional systems** (e.g., see Luria, 1973, 1979; von Holst, 1956/1973).

The concept of a functional system requires a different meaning of the term 'function' than is commonly used. Luria (1973) made this point clear in his writings on functional systems in the brain. "Those investigators who have examined the problem of the cortical 'localization' of elementary functions by stimulating or excluding local brain areas have understood the term 'function' to mean the *function of a particular tissue*" (p. 27; original italics). Such an interpretation is often reasonable (e.g., the 'function' of the liver), but as Luria tells us, "this definition does not meet every use of the term 'function'" (p. 27). The 'function of digestion', for example, requires a whole series of components with which food is transported, broken down, and absorbed. The 'function of respiration', too, involves a great many components working together, including the lungs, the diaphragm, and the intercostal muscles. Luria uses the example of respiration to illustrate what he means by a functional system:

It is obvious that the whole of this process is carried out, not as a simple 'function', but as a *complete functional system*, embodying many components belonging to different levels of

the secretory, motor and nervous apparatus. Such a functional system (the term introduced and developed by Anokhin, [in 1935]...), differs not only in the complexity of its structure, but also in the *mobility of its component parts*. The original task (restoration of the disturbed homeostasis) and the final result...obviously remain unaltered in every case...However, the way in which this task is performed may vary considerably. For instance, if the principle group of muscle working during respiration (the diaphragm) ceases to act, the intercostal muscles are brought into play, but if for some reason or other they are impaired, the muscles of the larynx are mobilized and the animal or human begins to swallow air, which thus reaches the alveoli of the lungs by a completely different route. (pp. 27-28; original italics)

Luria (1973, 1979) was interested in applying the concept of functional systems to the 'localization' of brain functions. Mental functions are not a result of specific tissue-centres or 'modules' in the brain, but of many concertly working zones 'connected' not just structurally, but functionally. Like the functional link between the two wrists in the act of drumming, a working brain has a functional organization consisting of links among many disperately located structures, each acting as a part.

...mental functions, as complex functional systems, cannot be localized in narrow zones of cortex or in isolated cell groups, but must be *organized in systems of concertly working zones, each of which performs its role [as a part -- cr] in a complex functional system*, and which may be located in completely different and often far distant areas of the brain. (p. 31; original italics)

When the integration of a system changes, so too do the functional relationships. New functional systems form throughout development, and can form during rehabilitation after brain damage. "For example, a lesion at the visual sensory areas of the cortex in

early childhood results in an underdevelopment of cognition and thought, while in an adult an identical lesion can be compensated for by the influence of the already developed higher functional systems" (Luria, 1979, p. 127). So the functional organization of the brain actually changes.

Although often neglected or misunderstood, functional relations are just as important to the integrity of an organism as are structural relations. An organism is made up not only of physical linkages, but functional linkages among its parts. The most developed idea of functional relations is the concept of 'functional systems'. Luria (1973; 1979) applied the concept of functional systems to the study of the brain and showed how functional links organize the brain into a set of concertly working zones.

Historical relations. An explanation of the integrity of organisms would not be complete without consideration of historical relations. In fact, from the Integrative Levels approach, it is impossible to explain fully any phenomenon without explaining how it came into being -- or evolved. This 'genetic method' (shared by activity theorists, see chapter four) is what I want to emphasize in this section. By 'historical relations', I mean to include all levels of development: ontogenesis, history, and evolution.

We saw in the previous section how the functional integration of the brain can change in development. Luria (1973) also recognized that the functional organization of the brain changes as a result of historically formed practices. The use of 'external aids',

for example, helps change the functional organization of the brain.

As Luria put it:

...it becomes perfectly clear that these external aids or historically formed devices are *essential elements in the establishment of functional connections between individual parts of the brain*, and by their aid, areas of the brain which previously were independent become the *components of a single functional system*. (1973, p. 31; original italics)

Historically formed external aids, such as tools and mnemonic devices, form, in their use, functional links between different parts of the brain. Hence, the functional organization of the brain does not only change through development, it changes through history. Furthermore, these functional links *extent beyond the confines of the skull*, to the external aids used in activity. Notice that this characteristic of the brain is lost in a purely structural description of the nervous system (e.g., 'rigid arrangements').

Another example of the relation between external aids and functional relations is the evolution of the human hand.

The anatomy of the human hand cannot be understood fully except in the context of its evolution. But the human hand evolved in response to tool-use (Leakey, 1979). Through the use of stone tools, it has come to form the capability of a precision grip. Once the first, crude stone tools became a part of the environment, the early hominids evolved in response to this (culturally produced) change in their environment and way of life. That is, by being a part of the effective environment, a cultural artifact helped shape the *biological evolution* of hominids. (The same could be said of 'language centres' in the brain). Human biology cannot be understood fully when

abstracted from culture, since the biological structure of humans is *fused with the cultural evolution* that helped shape it. Put another way, culture and biology do not 'interact' like separate billiard balls; they are *integrated*.

Another important idea when discussing historical relations is that of 'historical contingency'. This idea, championed by Stephen Jay Gould in his many essays on natural history (e.g., 1977; 1980; 1984; 1987; 1992), helps explain the evolution of biological organization.

The organization of an organism is historically contingent upon the organization of its ancestors. Darwin demonstrated that orchids developed elaborate contraptions for attracting insects "from common components of ordinary flowers, parts usually fitted for very different functions" (Gould, 1980). Through evolution, these 'available parts' entered into a different set of structural and functional relationships, changing the integration of the flower.

An evolutionary explanation for the integration of orchids does not require appeal to an external force or entity. On the contrary, as Gould tells us, that the orchids have historical relations showing decent from common flowers is evidence against the intervention of an organizing entity:

Our textbooks like to illustrate evolution with examples of optimal design -- nearly perfect mimicry of a dead leaf by a butterfly or of a poisonous species by a palatable relative. But ideal design is a lousy argument for evolution, for it mimics the postulated action of an omnipotent creator. Odd arrangements and funny solutions are the proof of evolution -- paths that a sensible God would never tread but that a natural

process, constrained by history, follows perforce. (Gould, 1980, pp. 20-21)

The historical perspective helps explain why an organism has the integration it does, and not some other (possibly more ideal) one. In fact, that organisms are historically contingent is itself evidence against mechanical integration, in that the integration is better explained by natural processes and not by an external organizing agent.

Parts of organisms, such as the parts of orchids, change their structural and functional relationships in an historically contingent manner. What this means is that the *relations between levels are transitional*: structural and functional relationships, making up the integration of an object, *change* through time. It is impossible, therefore, to explain fully the integration of an object viewed outside the context of its development.

I will end this section with another example from Gould, which nicely illustrates how all three types of relations -- structural, functional, and historical -- together can explain the phenomenon of integration. Gould (1980) illustrates the concept of historical contingency with a review of Davis' (1977) explanation of how the thumb of panda bears evolved. This example shows how a structure (a wrist bone), originally used for a completely different function, can become functionally integrated with other parts (the true digits) to form a new structure.

Panda bears have flexible thumbs, with which they strip off the leaves of bamboo stalks. This opposable "thumb", however, is actually a sixth digit constructed from an enlarged wrist bone. The

Panda's true thumb -- like the fifth digits of all members of the order Carnivora -- is far too specialized as part of a claw to function as a thumb. "So the panda must use parts on hand and settle for an enlarged wrist bone and a somewhat clumsy, but quite workable, solution" (p. 24).

In pandas, muscles that, in other bears, normally connect to true digits, instead connect to the enlarged radial sesamoid bone of the wrist, enabling the pandas to contract their 'thumbs'. How does this happen?

During fetal development, the radial sesamoid enlarges, completely changing the structural and functional relationships among the bones and muscles.

The sesamoid thumb of pandas is a complex structure formed by marked enlargement of a bone and an *extensive rearrangement of musculature*. Yet Davis argues that the entire apparatus arose as a mechanical response to growth of the radial sesamoid itself. Muscles shifted because the enlarged bone blocked them short of their original sites [the true digits -- cr]. Moreover, Davis postulates that the enlarged radial sesamoid may have been fashioned by a simple genetic change, perhaps a single mutation affecting the timing and rate of growth. (p. 23; added italics)

Through evolutionary change, the bones and muscles of the panda paw entered into a new set of structural and functional relations. But the form of these new relations were constrained by the panda's carnivorous history: "Like Darwin's orchids, they are familiar bits of anatomy remolded for a new function" (p. 22). A single change in one structure -- enlargement of the sesamoid bone -- created an extensive rearrangement, and a new set of functional

relations. It would be impossible to explain fully how this happened without looking at the fetal development.

Evolutionary and developmental explanations are examples of explaining the integration of organisms without appeal to organizing entities, or mystical organizing forces. Organisms do not evolve as a result of an intervening variable piecing parts together; that's how machines are made. On the other hand, as long as organisms are viewed as complex machines, the need for mechanical integration will persist.

In this section, I discussed how historical relations influence the integration of organisms, and how understanding these historical relations makes our understanding of structural and functional relations more concrete. In fact, according to the integrative levels approach, one must view something in its development, or formation, to understand it fully.

IV Summary

Novikoff used the concept of integrative levels of organization as a general description of the evolution of matter. As matter evolves, new and higher levels of complexity emerge from lower levels. There is a continuity between levels of matter, in that they are integrated and in that, historically, higher levels evolved from lower ones. There is also a discontinuity between levels, in that each level is qualitatively distinct, having its own set of laws, and requiring its own methods of study. Higher levels are dependent upon lower levels, and knowledge of the laws of lower levels is

important to understand them; yet, higher levels cannot be reduced to lower levels, and cannot be predicted from the laws of the lower levels. "The different levels of matter, while distinct, are not completely delimited from each other" (1945, p. 209). Novikoff's concept of integration, then, *avoids both reductionism and holism*.

Integration is best described as a dialectical process: It is the formation of parts into wholes -- and, simultaneously, the formation of wholes into parts. Two things that are unrelated may become related and form an entirely new structure; what were wholes once can become parts. Or, as in the case of the panda's thumb, parts that were already related -- such as the bones of the wrist and the digits of a claw -- may change their relationship and, again, form an entirely new structure. Hence, things -- either independent wholes or two already related parts -- become 'integrated' when they *enter into a set of relations* such that they act or function together as parts of a unitary system.

The integrity of organisms can be explained historically without appeal to an external organizing agent or principle. Integration is a result of structural and functional relations that change through time. In this way, the relations between levels are transitional and historically contingent. Defined in terms of structural, functional, and historical relations, the concept of integration *avoids appeal to an external organizing agent or principle*.

In the next chapter we will see how this new definition can be applied in psychology.

4: ALTERNATIVES TO MECHANICAL INTEGRATION IN PSYCHOLOGY

In the second chapter, we saw why the concept of mechanical integration is problematic. In the third chapter, we saw how integrative levels theory provides a truly alternative definition of integration -- one that does not fall into the problems of mechanical integration. It would seem, then, that applying the concept of integrative levels to psychology would be the best way to create an alternative to mechanical integration. However, recent trends in cognitive science have given rise to a new hope in associationism. It is necessary to deal with this new issue first, before going on to an application of the integrative levels concept to psychology.

A possible alternative to mechanical integration that traditional psychology has to offer is the Neural Network approach. This approach is spoken of as a new paradigm for psychology, which has no need for homunculus-style explanations. If this were true, then the Neural Network approach would not fit the definition of a mechanical integration theory, and would therefore provide an alternative to mechanical integration. We shall see, however, that the Neural Network approach does, after all, fit the definition of a mechanical integration theory, and that the homunculus concept is 'smuggled in through the back door'.

In the second half of this chapter, I turn to what I consider to be three true alternatives to mechanical integration in psychology: integrative levels theory in comparative psychology; Gibsonian ecological approach; and Activity Theory. All three are instances of applying the concept of integrative levels to the domain of psychology. In the first case, the application of the integrative levels concept is explicit. In the other two cases, however, the application is implicit, and must be made more explicit. In the final section, I shall speculate on how to combine all three perspectives.

I The Neural Network Approach

In the first chapter, we saw Gestalt psychology's criticisms of elementarist-associationist theories. These theories led to abstract and unreal concepts: "If experience is broken down into artificial bits, some artificial means must be devised of putting it together again..." (Heidbreder, 1933, p. 340). In the same chapter, we saw how elementarist theories require an integrating entity (homunculus). It would appear then, that any elementarist-associationist theory necessarily uses the concept of mechanical integration and requires an integrating entity. In recent years, however, a 'neo- associationistic' (Martindale, 1991) approach has emerged, the proponents of which claim to have overcome the problems of classical associationism. In other words, they claim not to use unreal, arbitrary models, and not to appeal to homunculus-style explanations. This new approach is the Neural-Network approach.

Neural Network theories are based on the assumption that the brain, ultimately, is a network of neurons. Because of this assumption, neural network theorists claim to be using the brain, rather than the computer, as a model of mental activity (e.g., Rumelhart, 1991). Neural network models are composed of neuron-like 'units' and their 'connections'. These units, called 'nodes', become activated and produce an output when a weighted sum of their inputs reaches above a threshold. The connections "provide the medium whereby the units interact with each other" (McClelland, 1989, p. 9). Thus, the models represent units that interact through physical connections with each other.

Humphreys (1989) summarizes some of the successes of the neural network approach:

They [the models] can solve the XOR problem [the 'exclusive OR', necessary in logic -- CR] (Rumelhart, Hinton, and Williams 1986); they can learn the past tenses of English verbs (Rumelhart and McClelland 1986); they can learn to associate spelling patterns with associated distinctive phonemic features (Seidenberg and McClelland, in press), and so on. (p. 66)

That these theories are 'neurologically inspired' (Norman, 1986, cited in Boden, 1991) has great appeal: "Because mental activity takes place inside the brain, this model seems undeniably reasonable" (Martindale, 1991, p. 8). No longer can the critics of associationism -- or, in this case, 'neo-associationism' -- argue that "...the bonds of connection, like the elements themselves, are unreal" (Heidbreder, 1933, p. 340). The Gestaltists argued that the associationist theories of the time were abstract and unreal. The

fundamental 'elements' of the associationist theories were mere products of abstraction. But neurons and their connections are real enough. Thus, by claiming to model real neural networks in the brain, these theorists seem to have avoided the criticism that associationism is based on abstract, unreal concepts.

Neural network models also appear to avoid the homunculus problem. No organizing entity is required to integrate if individual neurons are already connected together. If we view mental representations as composed of patterns of neural activity, then representations (patterns) can "...give rise to further processing activity directly, without any need for a central processor..." (McClelland, 1989, p. 11). In other words, mental processing leads to more processing simply as a result of the spread of activation. There is no need for an homunculus who examines the representations, and who passes them along to the next homunculus in the chain. Furthermore, neural network theorists argue that the brain does not always process information serially -- in a step-by-step fashion. (Serial processing would require an integrator to put the temporal series together). Instead, the brain, they argue, acts as a 'parallel processor'; the processing units function in parallel, or at the same time. That the units are connected and function in parallel are celebrated in the terms Connectionism and Parallel Distributed Processing (PDP). (From here on I shall use the term 'connectionism').

It would appear, then, that the neural network or connectionist approach avoids two of the problems of mechanical integration.

First, connectionism contradicts the claim that associationism's elements and connections are unreal. Connectionism is modeled after the brain, and its neural units and connections. Second, connectionism contradicts the claim that any associationist approach leads to the need for an integrating entity. Connectionism appears to explain integration in physical terms, without appeal to an external entity. "The brain has no CPU [central processing unit]," asserts Martindale (1991) "to say that the brain has a CPU would be equivalent to saying that the brain has a brain" (p. 13). But, despite the appearances, can a reductionist-neo-associationistic approach really avoid 'mechanical integration'? The answer to this question requires a closer look.

Connectionists claim that their networks are modeled after the brain. The extent to which this is true is debatable. First, it is debatable that the brain is merely a network of identical neural units. In reality, there exists a differentiation of cells and tissue (Kalat, 1984), and a complex distribution of function (Luria, 1973). To reduce the structures of the brain to networks of identical neural units is to ignore these qualitative differences. Second, it is debatable whether nodes are anything like real neurons. These 'nodes' are said to be "...similar to neurons, but not nearly as complicated" (Martindale, 1991, p. 2). Neurons are complex living cells, not passive conduits. Once real neurons were found not to live up to the associationists' standards (i.e., were too complicated for an associationistic theory), the real neurons were rejected in favour of the artificial 'nodes'.

The biologist Francis Crick (1989) points out that some of the more complex neural network models are "unrealistic in almost every respect" (p. 130), and that they usually violate certain rules of neuro-physiology. He claims that many of the models are arbitrary with respects to the actual brain, and that many of the psychologists working in this area "have little or no interest in the actual brain, or at least what goes on inside it" (p. 131). In the same vain, although rather extreme, one neuro-physiologist has been quoted as saying: "The only thing neural networks have in common with the human brain is the word 'neural'" (Poggio, 1988; in Morris, 1989, p. 203).

It appears as though the connectionists' approach has not, in fact, overcome the first problem with associationist theories. The artificially simplistic elements, nodes, and their arbitrary connections, networks, are not any more real than those of classical associationism. (I should mention, however, that a few neuro-physiologists [e.g., Morris, 1989] have begun to work with the connectionist approach in the hopes of making models that are more realistic.)

What of the homunculus problem? Do the connectionist models, although abstract and arbitrary, nevertheless avoid the need for an integrating agent? My understanding is that the more complex, 'multi-layered' network models do not, in fact, avoid the need for an organizing agent.

Simple nets have been successful in modeling memory without the need for an homunculus (Crick, 1989). (Actually, even this claim is generous; what the connectionists do not tell you is that the model

has no meaning except to an external observer -- i.e., the psychologist. This is, essentially, the point William James was making). In such models, memory is said to be distributed over many synaptic connections in the network. These connections are 'weighted' and the thresholds of the nodes are 'adjusted', so that information is eventually stored in the form of connection strengths (Newell, 1990). While several synapses may be involved in a single memory, a single synapse may be involved in several memories; memories are not fixed to particular nodes, or connections, but are embedded in a pattern of weighted connections. Such memory networks can be modeled using a single layer of units if the net is able to feedback on itself (Crick, 1989). These simple nets are said to be 'unsupervised' - - i.e., they do not require an external agent acting as an homunculus.

Such nets are usually simple, having a single layer of units. Moreover, they are usually unsupervised. That is, there is no teacher to tell the net how to adjust its output to make it resemble the desired one. The net learns by using an algorithm based on an idea of Hebb. This is a local algorithm *as it depends only on the activity* near that particular synapse: roughly, the synapse is strengthened if it receives an input signal... (Crick, 1989, p. 129; added italics)

These algorithms are 'local' or intrinsic to the activity of the network itself. Simple nets, then, seem not require an external agent to follow rules.

Unfortunately, these simple, homunculus-free networks are severely limited (Longuet-Higgins, 1989; Crick, 1989). In order to model such psychological phenomena as language and categorization,

connectionists use more complex, multi-layered networks. These complex nets are "governed by externally imposed learning algorithms" (Lycan, 1991, p. 268) such as the 'back propagation of errors' (Crick, 1989). Such algorithms are said to 'supervise' the learning process by 'correcting' the net until it has built up the correct pattern of weighted connections. In such cases, it is the computer and not the network itself that runs the algorithm (Lycan, 1991; Crick, 1989). This contradicts the claim that "PDP models do not have explicit rules" (Humphreys, 1989, p. 68). Such networks are dependent upon external rules imposed upon them by a *computer acting as an homunculus*. Even the *logic* of the 'back prop' model is the same as the earliest of homunculus theories:

...a network undergoes a training period during which it is presented with a series of inputs and allowed to produce an output for each presentation. A comparison is made [by the computer - cr] between the actual output and a target output for each presentation, resulting in an error signal [meaningful only to the computer -- cr]. This signal is subsequently propagated back through the network [by the computer -- cr], adjusting weights in accordance with the learning algorithm [read by the computer -- cr]. (Ramsey & Stich, 1989, p. 299)

Presumably, there is something in the human brain that functions analogously to the computer. Even Francis Crick (1989) has been taken in by the seeming need for an homunculus. However, unlike many psychologists, at least he admits to it when he sees it:

...the theorists working on the subject are so remote from actual neurons that they have been cavalier in omitting one type of unit altogether. Obviously there should be a unit to compare the output of each output neuron with the signals from the teacher, in order to calculate the error (in back-prop

nets this is done by the computer). Such a set of neurons, if they exist, should have novel properties and would be worth looking for, but there is no sign of back-prop advocates clamouring at the doors of neuro-scientists, begging them to search for such neurons. (p. 131)

For psychologists to actually beg for the time-worn "pontifical cell" (James, 1890) would, of course, be a humiliating admission of failure.

II A Solution for Connectionism?

Simple neural networks have provided insights into how the brain works, apparently without need for an homunculus (again, with the exception that an external observer is needed). More complex models, however, are required to model more complex mental phenomena. These complex networks have also provided insights, but they require an homunculus to work, and they violate many neuro-physiological laws -- e.g., synaptic connections that switch from excitatory to inhibitory (Morris, 1989). Attempts to overcome these problems result in even more contrived models (Crick, 1989). Connectionists, then, are stuck in a dilemma: If they try to build realistic models, the models don't do much; if they try to build models that do much, the models aren't realistic.

The dilemma is not a problem with the current state of the art. Better computers, better networks, and better neuro-physiological data, although useful, will not overcome the essence of the problem. Rather, there are certain, unnecessary assumptions that are forcing connectionists into this dilemma. In fact, the problem may be a

surprise to most connectionists, for I believe that the very method of trying to rid themselves of homunculi is the very cause of their needing them!

The assumptions stem from the fact that connectionism is a 'bottom-up' approach (Newell, 1990; Morris, 1989). In that it tries to explain the 'top' in terms of the 'bottom', it is also a reductionist approach. But if one tries to build models of the lower levels that explain fully the higher levels, then one is making the implicit assumption that the models be 'self-contained'. This is evidenced in the fact that connectionists talk of ultimately obtaining 'fully automated networks'. They think that the way to rid themselves of the need for external agents, imposing *external* rules, is to achieve completely automated, or self-contained systems -- *internal* relations without external ones. This runs into an immediate problem: Real neural networks are not self-contained, self-regulated closed systems (Luria, 1973). In fact, living systems *in general* are open (Prigogine, 1976; Kugler & Turvey, 1987), and require both internal and external relations (Levins & Lewontin, 1985). This is the essence of the connectionist's problem, for *if we accept that real networks are not self-regulated, and yet still insist that they are regulated on the lower level, then some hypothetical agent* (existing on the lower level, and external to the system itself) *will be required to do the job of regulating*. Hence, by confining our explanation of regulation to the lower levels, we are forced into postulating a regulating agent. As a consequence of this confinement,

connectionist models can *never* be both realistic and self-regulated at the same time; they will always require an homunculus.

The solution to this problem is to rid ourselves of the idea that the lower levels be self-contained, and embrace the idea that neural networks are *open to the constraints of many higher levels*. As we saw with the concept of functional systems, systems may *span several levels*, forming functional links (not just 'physical connections' making up rigid arrangements) both internally and externally. There is no need to shift the regulation (or 'degrees of freedom') problem onto hypothetical agents existing on the lower levels, when, instead, we can recognize the real constraints of higher levels. If connectionism can embrace the idea that regulation occurs across levels, then it may have the potential to rid itself of the homunculus problem once and for all.

III The Concept of Integrative Levels in Psychology

Given that integration is a problem in psychology, and given that there is a solution to be found in biology, can this solution be applied to psychology? In this half of the chapter, I show how the concept of integrative levels can be, and already has been, applied to areas in psychology. These examples are: the integrative levels approach in comparative psychology, the Gibsonian Ecological approach, and Activity Theory.

A. Comparative Psychology

Comparative psychologists study the development and evolution of all living organisms, making contrasts and comparisons. Most comparative psychologists are interested in evolutionary theory and its relevance to behaviour. It is not surprising, then, that the idea of integrative levels has been adopted by a number of comparative psychologists. In fact, T. C. Schneirla (1940) was already using the concept in his studies of ant colonies even before the publication of Novikoff's (1945) article. In his work on army ants, Schneirla showed how social organization constitutes an emergent level and how the behaviour of the individual ant has no pattern, and is non-adaptive, outside the social sphere.

Recall from chapter three that levels are integrated but distinct. Each level requires its own methods of study. As Ethel Tobach (1991), a student and colleague of Schneirla, writes: "One would not study hormonal titers of the organism by a paper and pencil questionnaire..." (p. 18). Recall also, that the concept of integrative levels is not to be confused with similar sounding concepts:

The concept of integrative levels...is not to be confused with the terms "levels of organization", "hierarchy" or "stages". Organizational levels frequently emphasize structure; hierarchy connotes a predetermined relationship of greater or lesser status, as in the societal institutions of religion, the military or the government; and stages seem more useful in defining developmental processes within categories and are definable in terms of integrative levels. (Tobach, 1991, p. 18)

The integration of organisms changes through evolution and through ontogenesis. In evolution, structures appear and disappear, or (as in the case of the Panda's thumb) they become re-organized, creating new structural and functional relationships. In ontogenesis, too, "all the levels of intra-organismic organization are differently integrated at each stage of development as the individual grows, develops, and matures" (Tobach, 1981, p. 46). The individual embryo, has a different integration than it will have as a fetus, infant, or adult. Thus, in order to understand a particular behaviour, it is essential to explain its integration in the organism, and how this integration developed and evolved. This 'genetic approach' (as 'genesis', not 'genes') is essential to the integrative levels approach in comparative psychology.

This line of thinking was expressed long ago in comparative psychology by Daniel Lehrman (1953/1961) in his critique of Konrad Lorenz. Lehrman argued that behaviours do not develop as unitary and autonomous things. Pecking in the chicken, for example, *when viewed in its development*, turns out not to be a single unitary 'instinct', but a result of a complex integration of several lines of development: "Pecking in the chick is also an emergent -- an integration of head, bill, and throat components, each of which has its own developmental history" (1953/1961, p. 159). Integration of this kind is a result of many lines of development entering into new structural and functional relations. For this reason, Lehrman also criticized the traditional interpretation of 'isolation experiments';

i.e., if a behaviour develops in the absence of 'learning' it is said to be the result of an 'innate instinct'.

It is obvious that by the criteria used by Lorenz and other instinct theorists, pecking in the chick and nest-building and retrieving in the rat are not 'learned' behavior. They fulfill all criteria of 'innateness,' i.e., of behavior which develops without opportunity for practice or imitation. Yet, in each case, *analysis of the developmental process* involved shows that the behavior patterns concerned are *not unitary, autonomously developing things*, but rather that they emerge ontogenetically in complex ways from the *previously* developed organization of the organism in a given setting. (pp. 157-158; italics added)

Lehrman (1953/1961) summarized the 'genetic approach' (although he did not call it that) in the following passage:

The "instinct" is obviously not present in the zygote. Just as obviously, it is present in the behavior of the animal after the appropriate age....The problem of development is the problem of the development of new structures and activity patterns from the resolution of the interaction of *existing* structures and patterns, within the organism and its internal environment, and between the organism and its outer environment. At any stage of development, the new features emerge from the interactions within the *current* stage and between the *current* stage and the environment. The interaction out of which the organism develops is *not* one, as is so often said, between heredity and environment. It is between *organism* and environment! And the organism is different at each different stage of its development. (pp. 162-163; original italics)

In the classical 'instinct theories', instincts were supposed to consist of a set of behaviours, programmed by biology, that were 'released' or 'triggered' by environmental stimuli. These instinct theories followed the logic of mechanical integration, because they

required a controller, such as the genes, to govern the behaviour according to a plan of behaviour. But the development of behaviour, like the development of body parts, is not the straight-line result of a 'plan', or 'programme' (Lehrman, 1953/1961; Gould, 1984; Ho, 1988). There do not exist genes 'for' a particular behaviour or body part (Gould, 1984); genes act in the context of many levels, and act differently at different stages of development.

Again, the approach to integration that I am proposing in this thesis, so clearly illustrated by Lehrman, is not mechanical and does not appeal to an organizing agent. Instead, when the concept of integrated levels and the 'genetic approach' are combined, we can avoid the problems of mechanical integration. We have seen how a number of comparative psychologists have made explicit use of the concept of integrative levels, and have illustrated how this concept can be applied to psychology as a true alternative to mechanical integration. In the next two sections, we will see how other schools of psychological thought have made implicit use of the concept.

B. The Gibsonian Ecological Approach

Although I cannot say whether James Gibson (1904- 1979) was ever aware of Integrative Levels Theory, per se, I can say that his approach shares many of the same basic methods and assumptions. James Gibson used the concept of integrated levels, combined with a serious interest in evolution, to make an 'ecologically valid' theory of perception.

The senses evolved as parts of integrated organisms that actively live in their environments. It is within the life activity of the organism, and within the integrated body of the organism, that the senses function normally. But if this is true, then there are other, more 'ecologically valid', forms of perception that traditional psychology has ignored. Gibson wrote:

The headrest of the laboratory prevents the observer from turning his [sic] head and looking around, which provides what I will call *ambient vision*. It also, of course, prevents him from getting up and walking around, which provides *ambulatory vision*. Are these forms of vision? I suggest they are; in fact, they are the kind of vision we need in life.... The crux of the matter is whether or not natural vision is compounded of units like the snapshot. I very much doubt that vision is simplest when the experimenter tries to make the eye work as if it were a photographic camera... (1979, pp. 1-2; original italics)

Accordingly, Gibson (1966) had a corresponding view of the senses. Although he used the term 'hierarchy' (see chapter 2) rather than 'integrated levels', Gibson argued that the senses must be considered, not as channels for sensations, but as perceptual systems spanning many levels and consisting of many disparate, but related components. The visual system, for instance, is not limited to the eye, optic nerve, and visual cortex; rather, the visual system *extends* to the muscles around the eye, the head, and even the body -- all working together as a system. This 'hierarchy' is what Gibson (1966, 1979) termed a **perceptual system**. "Such a system is never simply stimulated [as receptors are] but instead can go into activity in the presence of stimulus information" (1979, p. 53).

In such a perceptual system, various kinds of movement are possible -- enabling the animal to pick up *higher ordered* information, called 'invariants'. Gibson argued that invariants, not sensations, are the structural units of information in perception (1973/1982, p. 286). Many of these invariants only arise through an observer's motion or through the motion of an object in the observer's environment. "What exploration does is to isolate the invariants. The sensory system can separate the permanence from the change only if there is change" (1963/1982, p. 369). Hochberg (1978) comments on the importance of motion to the pick up of information:

When the study of perception started with pure sensations and attempted to build up from these to an understanding of our perceptions of the physical world, there seemed to be no way to gain unambiguous information about the world of objects, space, and motion by means of our eyes alone...[However] if we consider the higher order variables which result from the observer's own motions, the stimulation at the eye is almost completely unambiguous... (p. 126)

That higher order variables do not have to be calculated reduces the burden for the brain. But notice that these higher order variables cannot be picked up if the perceptual system is restricted, as it is in the laboratory. Gibson (1979) wrote:

Looking around and getting around do not fit into the standard idea of what visual perception is. But note that if an animal has eyes at all it swivels its head around and it goes from place to place. The single, frozen field of view [produced in the lab -- cr] provides only *impoverished* information about the world. The visual system did not evolve for this. The evidence suggests that visual

awareness is in fact panoramic and does in fact persist during long acts of locomotion. (p. 2; italics added)

Given the assumption of impoverished sensations, some kind of cognitive process (either learned or innate) must be hypothesized in order to explain the richness of perception. But if the assumption of impoverished sensations is a consequence of reductionism -- and if the confirming laboratory results are a consequence of unnaturally restricting the perceptual system -- then we can rid ourselves of the assumption by replacing the concept of mechanical integration with the concept of integrative levels.

Gibson's theory states that perception is an activity, involving a perceptual system, which entails the 'pick up of information over time' (1966, 1979). Given that perception is information-based not sensation-based (Gibson, 1976/1982), what is the nature of 'information'? Gibson (1976/1982) wrote,

My notion of information consists of invariants underlying change. It does not consist of stimuli, nor patterns of stimuli, nor of sequences of stimuli. A perceptual system does not respond to stimuli (although a receptor does) but extracts invariants. This notion is radical and unfamiliar but why should it be so difficult to comprehend? Gradients, transients, derivatives, ratios, and rates of flow in a flowing array...are actually much more plausible than patterns of stimuli that go on and off [in an ordinal sequence --cr] at a mosaic of receptors. (p. 40⁹)

Let us consider an example of invariants in the flow of stimulation. According to classical theories of perception, the age old question of how one sees a rectangular table top, given that all

our eye receives are two dimensional projections of irregular trapezoids, is answered traditionally with the distinction between sensation and perception. Sensations, we are told, are impoverished and, thus, some kind of 'enrichment' process must take place. Enrichment of sensations can take the form of inference, supplemental feed-back, the attaching of responses to stimuli, or organizing processes (Gibson & Gibson 1955/1982). The question in this traditional formulation is : Given that the information for 'rectangularity' cannot possibly be in the impoverished flow of stimulation, what sort of mental processes can we hypothesize to account for the discrepancy?

In contrast to the traditional approach, Gibson (1979) asks us to reformulate the question of the table top to ask,

What are the invariants underlying the transforming perspectives in the array from the table top?...Although the changing angles and proportions of the set of trapezoidal projections are a fact, the unchanging relations among the four angles and the invariant proportions over the set are another fact, equally important, and they uniquely specify the rectangular surface. (p. 74)

Thus, Gibson's approach assumes no need for enrichment of any kind, asserting instead that the stimulus flux (caused by motion) contains much more information than is often assumed. (Compare this theory of order in the flow of stimulation with the similar discussion by Wolfgang Koehler, see chapter two).

Gibson's ecological approach uses the concept of levels to show how the senses are organized into perceptual systems, spanning many levels. It also uses the concept of integrative levels to show

how higher ordered information is available for perception -- variables that do not need to be computed. This approach to perception makes more evolutionary sense than the traditional approach. After all, it is difficult to imagine how an organism could have evolved that picked up only impoverished information about its surroundings. According to evolution theory, such an organism would soon die-out.

C. Activity Theory

Activity Theory finds its origins in the work of Lev Vygotsky and the cultural-historical approach. Together with Luria and Leont'ev, the three formed the 'troika' of Soviet psychology (Wertsch, 1985). Like the integrative levels approach, the cultural historical school adopted a 'genetic method', in which behaviour is explained by showing its development. Also like the integrative levels approach, Vygotsky and Luria argued that there were different levels of development:

Our task was to trace three basic lines in the development of behavior -- the evolutionary, historical, and ontogenetic lines -- and to show that the behavior of acculturated humans is the product of all three lines of development, to show that behavior can be understood and explained scientifically only with the help of three different paths from which the history of human behavior takes shape. (Vygotsky & Luria, 1930, cited in Wertsch, 1985, p. 27)

Activity theory, as outlined by Leont'ev (1981a; 1981b), also uses the concept of levels. Activity is analysed into three levels: activity, actions, and operations. Activity exists on a societal, or

'social institutional' (Wertsch, 1985), level. Actions are performed on a lower level, either individually or jointly, and is the level at which our conscious goals are achieved. Operations are performed on the lowest level, and can be performed 'automatically', without conscious thought. Although distinct, the three levels of activity are not completely delimited from each other. What was once an action can grow into an entire life activity. Alternatively, what was once an action, requiring conscious thought, may become an automatically performed operation.

The levels of activity are not an hierarchy, closed off at the top from the rest of the world. Activity is said to be 'object related' and 'essentially social' (Leont'ev, 1981a). Leont'ev wrote:

human psychology is concerned with the activity of concrete individuals, which takes place either in a collective -- that is, jointly with other people -- or in a situation in which the subject deals directly with the surrounding world of objects - for example, at the potter's wheel or the writer's desk. However, if we removed human activity from the system of [societal -cr] relationships and social life, it would not exist and would have no structure. With all its varied forms, the human individual's activity is a system in the system of [societal -- cr] relations. It does not exist without these relations. The specific form in which it exists is determined by the forms and means of material and mental social interaction that are created by the development of production and that cannot be realized in any way other than in the activity of concrete people. It turns out that the activity of separate individuals depends on their place in society, on the conditions that fall to their lot, and on idiosyncratic, individual factors. (1981a, pp. 46-47)

The concept of activity is richer and more developed as a unit of psychological analysis than are the units of traditional psychology

(e.g., consciousness, behaviour, cognition). Behaviour is too abstract, since "entities as diverse as liver cells and stock markets are spoken of as behaving" (Tolman, 1987, p. 198). As a consequence, qualitative differences, resulting from evolution, are lost or ignored in behaviourism. "For example, Skinner (1956) displays three cumulative curves side by side, and then informs us that '...one of them was made by a pigeon..., one was made by a rat..., and the third was made by a monkey....Pigeon, rat, monkey, which is which? It doesn't matter'." (Tolman, 1987, p. 198)

Cognitive psychology fares not much better than behaviourism, for it restricts study almost exclusively to the level of operations (Wertsch, 1981).

They tend to ignore issues that arise at the levels of analysis concerned with actions and activities. This does not mean, however, that it is a simple matter to make a direct comparison between Soviet [Activity Theory -- cr] and Western [Cognitive -- cr] research at the level of operations; the facts that this level *fits into a larger framework* in Soviet psychology, and that other features of the theory of activity influence Soviets' views of the nature of an operation, complicate matters. (pp. 18-19; added italics)

It is not merely a case of cognitive psychology focusing on one level, and activity theory focusing on three levels; rather, cognitive psychology distorts the lower level by isolating it from the other levels -- from the structural, functional, and historical relations it has with the other levels. For example, by approaching cognitive development as something happening inside the head of an

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individual, 'constructivist' approaches have created what is called the 'learning paradox' (Engestrom, 1987) or the 'constructivist's fallacy' (Flanagan, 1984).

If development works by the child constructing an hypothesis based on a concept, such as reversible operations, and by the child testing this hypothesis, then the question naturally arises: How could the child ever have formed the hypothesis in the first place, unless she already possesses the cognitive abilities? "But if the child is testing an hypothesis with the concept of reversible operations in it, then she is not learning the concept of reversible operations, she is just testing to see if the concept, which she *already* 'knows', applies" (Flanagan, 1984, p. 139; added italics). In a nutshell, what is the source of the hypothesis, if not some innate idea already in the head?

Reviewing an article by Bereiter, Engestroem (1987) formulates the 'learning paradox':

Metatheoretically, the problem is "how can a structure generate another structure more complex than itself?" Theoretically, the problem is "how can the development of complex mental structures be accounted for by mechanisms that are not themselves highly intelligent or richly endowed with knowledge?" In other words, how is progress toward higher levels of complexity possible without there "already being some ladder or rope to climb on" (Bereiter 1985, pp. 204-205). (Engestroem, 1987, pp. 29-30)

At the heart of the problem is the assumption that cognitive development is a private and individual phenomenon, happening only inside of heads. But children rarely learn complex concepts on their own -- closed off from the social-historical context in which they

live. "Learning does, indeed, depend on the prior experience of more complex cognitive structures, but these complex structures are situated in culture, not in the child" (Bereiter, 1985; in Engestroem 1987, p. 30). Children learn in joint action, with adults and more capable peers (Vygotsky, 1978). These joint actions, in turn, are part of a larger activity -- education. And education, in turn, has developed through cultural evolution (see Engestroem [1987] for a fuller development of the concept of activity)

By restricting study to what is 'in the head', and isolating what is 'in the head' from many higher levels, developmental psychologists have been led to believe in hypothetical innate constructive processes. (One is tempted to add, here, that the assumption of impoverished sense data only aggravates the problem). By closing off processes on the lower level (mental operations) from other levels, they have ended up completely distorting those lower processes. Entire theories have been based on this fallacy (e.g., Fodor, 1980).

Activity Theory provides a more concrete framework for psychology, in that it does not abstract out one level for isolated study. Instead, Activity Theory recognizes that human activity consists of at least three levels, and is open to the societal and historical levels of which it is a part. By emphasizing development and levels, Activity Theory avoids the hegemony of mechanical integration theories, which try to explain complexity in terms of hypothetical integrative processes existing on the lower levels.

IV Conclusion: Integrating the Three Approaches

We have seen examples of how the concept of integrative levels can be applied to psychology. Such an application produces a more concrete and less abstract understanding of the subject matter. But are these examples separate instances, or is there a more fundamental relationship between them?

Given the similarities between integrative levels theory, Gibsonian ecological psychology, and activity theory, it would make sense to combine the three approaches. Ethel Tobach (1991), and, indirectly, Charles Tolman (1987a; 1987b) have already argued in favour of integrating activity theory with integrative levels theory. In addition, Paul Robinson (1990) has argued for integrating activity theory with the Gibsonian ecological approach, and has also outlined some of the implications of such an integration for the theoretical unification of psychology.

All three perspectives, once combined, can learn from each other. Activity theory, once combined with the ecological approach, can avoid the mechanical integration that it sometimes slips into. The ecological approach, once combined with activity theory, can avoid its biological bias. Finally, once comparative psychology is combined with the other two approaches, the activity theoretical and Gibsonian ecological approaches can form a solid evolutionary background.

Activity theorists often fall into mechanical integration because they do not have a developed theory of direct perception. Elenor Gibson (1969, pp. 53-57) has described Soviet theories of perception

as 'motor copy theories', in which association is the mechanism of perceptual integration. Luria (1973, 1979), too, fell into mechanical integration, despite his brilliant work on 'functional systems'. For example, he describes a patient with a lesion to the secondary visual zones as unable to combine sensory features into forms (1973, p. 116). The implication is that normal people with intact brains do "combine these individual features into complete forms" (p. 116). In short, activity theory has adopted the representationalism of western psychology.

However, according to Lektorsky (1984), representationalism is inconsistent with the underlying philosophy of Activity Theory (i.e., Dialectical Materialism). Although he does not recognize representationalism existing in Activity Theory, Lektorsky does criticize the western theories of perception as, ultimately, being subjectivistic. This is certainly true, but without an *alternative* theory of perception, it is difficult to imagine Lektorsky's warning being heeded. Activity theorists would do better to adopt the Gibsonian ecological approach than to continue with representationalism.

The ecological approach would, of course, be changed in the process of being absorbed by Activity Theory. The main problem with the ecological approach is that it is 'ecological' -- i.e., it says little about the social and societal levels of human activity.

Finally, comparative psychology can strengthen both the ecological approach and the activity theoretical approach by providing a solid evolutionary background. Ethel Tobach (1991) has

outlined, from the integrative levels approach, some of the issues concerning the evolution of activity. Charles Tolman (1987) has reviewed some of the literature on human evolution, and has interpreted it using both activity theory and integrative levels theory. More recently, I have used concepts from all three perspectives (the ecological, integrative levels, and activity theoretical approaches) to speculate on the evolution of meaning (Robinson, in press).

It would be difficult to justify my call for studying integrative levels in psychology, if I did not also believe in the integration of the psychological sciences. It is precisely because levels are integrated that psychologists studying different levels should also integrate their science. Integration among the three discussed approaches will be a major step toward such a unification of the psychological sciences.

Such an 'integration,' however, does not imply that psychologists cannot specify in particular areas: "The data for all levels need not be gathered in one laboratory..." (Tobach, in press). Dialectically speaking, the 'differentiation' between psychologists should reflect the 'integration' between them. Without differentiation, it becomes too difficult to study all the domains of psychology; without integration, it becomes too easy to distort a particular domain through isolating it from its context. In this chapter, I hope to have provided examples of how many levels of integration can be studied without the task becoming too overwhelming. The advantages of following this methodology, I believe, are as numerous as the disadvantages of not.

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