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Functional Organization, Functional Analogy, and Functional Inference

Preface

The main body of this article was written in May of 1970, and in-cluded in my (1971) dissertation. It builds upon my analysis of statements of function (WIMSATT 1972). For the most part, it approaches problems of functional organization and functional analogy from a eye" "God's or "LAPLACEAN demon" point of view, assuming that "DARWIN's demon" (WIMSATT 1980a) will see any functional effect, no matter how small, and incorporate it in the functional architecture:

"It may be said that natural selection is daily and hourly scrutinizing,

throughout the world every variation, even the slightest, rejecting that which is bad, preserving and adding up all that is good; silently and insensibly working, whenever and wherever opportunity offers, at the improvement of each organic being in relation to its organic and inorganic conditions of life." (DARWIN, 1859, p84).

Assumptions of omniscience, computational omnipotence, and the like have a time-honored tradition in philosophy, and a close connection with conceptual or 'in principle' arguments. I have attacked them systematically throughout my professional career (see WIMSATT 1998.) They are fine for delineating the conceptual structure of functional organization, but I wanted to go further—to characterize functional organization in a way which would be useful to and capture ways it was described

Abstract

Functional organization is the architecture of the phenotype, the realm of arguments of adaptive design, and crucial to the application of selection theories from biology through software engineering to the design of legal codes. An analysis of functional attributions yields the conceptual structure of functional hierarchies. This ideal contrasts with heuristic tools used by practicing functional analysts. Heuristics and adaptations both have a functional structure. Analyzing heuristics addresses the relation between theory and practice, helps us to understand the strengths of various reasoning patterns. This alerts us to situations where they are likely to show bias or fail, leading us to functional localization fallacies-ways in which functional inference can go wrong.

Key words

Functional organization, functional equivalence, functional analogy, functional localization, heuristics, biases, adaptive design, means-end, phenotypic organization, optimization. and worked with by realworld practitioners. There were in the original some moves in that direction, but I felt that this analysis was still frustratingly far from this goal, so I delayed publishing it (even though it naturally complemented my 1972) until I could do better.

More recently Werner CALLEBAUT encouraged me to publish it, making connections with *Evolution and Cognition*, which suddenly had space available which had been saved for an article then under revision by Donald CAMPBELL at his untimely death. This version contains additional interstitial remarks, expansions of sections 12 and 19, and brand new

sections (numbers 22-25) at the end on adaptations, heuristics, and their biases, which further this goal. The first of the final sections discusses problem solving heuristics, and features they share with biological adaptations. Both are teleological tools having a functional structure, and the components of functional organization everywhere. The analysis of heuristics addresses the relation between theory and practice, points us in directions where we can understand the strengths of various reasoning patterns, and gives us guidance about the conditions under which they are likely to break down. This leads us in the final section to a discussion of functional localization fallaciesways in which *functional* inference can go wrong. Most of the newer references are in these sections. I have generally not added references elsewhere, save to other papers (WIMSATT 1972 and 1974) which came

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out of my dissertation, and were written at the same time. KAUFFMAN (1971) was available in preprint.

I have particularly not tried to bring this paper up to date to address all of the now enormous literature on the teleological (or "selectionist") concept of function. I analyzed this at length in my (1972), which was the first (and is probably still the fullest) systematic treatment of this idea. Nonetheless, it is better known to most philosophers through the work of WRIGHT (1973) and MILLIKAN (1984). The 1972 paper is like more recent attempts in having more moderate versions than WRIGHT or MILLIKAN provide of the etiological connection between function and past selection. It also has more structure for dealing with the diversity of uses of functional inference than any of these analyses. WRIGHT treats a past selection history as a necessary condition of present functionality, and this is the source of the vast majority of counterexamples to his analysis. My analysis avoids almost all of these, and provides many more resources for treating a variety of issues arising particularly in the comparative uses of functional analyses.¹

1. Introduction

Until recently (e.g., BECKNER 1969)² the philosophical literature on function has been primarily concerned to determine whether functional analyses and explanations are reducible to causal analyses and explanations and whether (in either case) they are scientifically acceptable. While these may be interesting questions to the philosopher, there are questions relating to the concept of function which are of far greater interest and import to the biologist. Two such questions are the following:

- 1. How is biological organization to be characterized?
- 2. What are the grounds for judgments of homology and functional analogy?

I will offer an analysis of the structure of "functional hierarchies" which bears on both of these questions. While this analysis is addressed primarily to biology, it should apply in any area where one may appropriately use functional analyses in the sense to be explicated below (See WIMSATT 1972). I suspect that this includes (1) ordinary explanations of human action—especially in cases where deliberation, strategies, or planning are involved; (2) explanations connected with learning phenomena, problem-solving behavior, and perhaps in other areas in psychology; and (3) many aspects of the social sciences where cultural evolution is held to be an influential factor.

Part I: The Structure of Functional Hierarchies

2. The Sense of "Function" Intended

(1) There seem to be at least three distinguishable senses of "function" in use by biologists (See WIM-SATT 1972 for more):

(a) To say that an entity is functional is to say that its presence contributes to the self-regulation of some entity of which it is a part (see, e.g., NAGEL 1961 and BECKNER 1959, 1968, 1969).

(b) To say that an entity if functional is to say that under at least *some* conditions it plays a (presumably causal) role in the operation of some system of which it is a part (see, e.g., BECKNER 1959, FODOR 1965, 1968, and KAUFFMAN 1971)

(c)To say that an entity is functional is to say that it is being selected for or maintained by natural selection (and presumably, in the overwhelming majority of cases, that it owes its presence and form to the operation of natural selection).

Of these three senses, physiologists and biologists strongly influenced by cybernetics are most prone to understand "function" in the first sense. (So are most social scientists, of a classical functionalist bent.) Biochemists, molecular biologists and biophysicists (to the extent that they use functional language at all) tend to the second sense—as do "intervening-variable" psychologists and those influenced by computer science. The third sense is commonly used throughout evolutionary biology, including particularly by systematists, paleontologists, geneticists, ecologists, and ethologists, though it also is used by a sprinkling of people in the other areas mentioned.

(2) I will use "function" in this essay in the third sense for biology³ and in a manner generically related to the third sense in more general applications⁴. This is for several reasons: (1) Biological organisms are products of an evolutionary process, and reflect this genesis at all levels of organization. Thus, this sense of function should apply at all levels and to a much broader range of biological phenomena than in the first sense, which is connected solely with self-regulatory phenomena. (2) Conversely, any theory of biological organization should reflect the fact that biological entities are evolved and evolving systems. It is thus plausible to suppose that if any sense of "function" is relevant to the characterization of biological organization, it should be conceptually related to the operation of evolutionary processes. (3) The second

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sense lacks the specificity necessary to generate interesting properties for the hierarchy which result from the third sense.⁵ (4) The first two senses of function can be treated, in all interesting applications, as special cases of the third sense, or (in nonbiological contexts) of its generalization. (5) For conceptual reasons, the third sense of "function" has the strongest reasons of any of the three to be called a "teleological" sense of "function." [See WIMSATT 1972].

So in summary, only the third sense has the requisite generality (in the range of biological phenomena and entities to which it applies), relevance (in its explicit connections with evolutionary considerations), and specificity (in terms of logical constraints) to generate an interesting logical theory of biological organization.

3. The "Function Statement" Schema

In what follows, I assume a "normal form" for attributions of function (in the third sense). The schema (as explicated and derived in WIMSATT 1972) is as follows: "According to causal theories, T, the function of behavior B of item i in system S in environment E relative to purpose P is to do (or bring about) causal consequence C." This schema will generally be represented by the logical functional equation:

F[B(i), S, E, P, T] =to do C

As example of a function statement fitting this schema might be: According to current physiological theories (T), the function of the expansion and contractions of the peripheral capillaries (B of i), in the thermoregulatory systems of a mammal (S) in environments which are appreciably hotter or colder that the normal bodily temperature of that organism (E), relative to maximizing the probability of survival to time t in the future of the appropriate evolutionary unit of which the organism is a part (P),⁶ is to change the rate of heat exchange between organism and environment in such a way as to decrease the difference between the organism's bodily temperature.

Note that most attributions of function are not this completely spelled out, and that biologists frequently attribute functions to recognizable parts of organisms, as well as to the behaviors of these parts. In advancing this schema, I am *not* suggesting that all functional attributions must take this form, but only that the more telescoped descriptions implicitly involve reference to all of these variables and that attributions of function to behaviors is conceptually prior to attributions of functions to physical objects.

The latter claim is roughly that we attribute functions to objects because and only because they exhibit certain behaviors under the appropriate conditions. This claim is supported in part by the character of the concept of "functional equivalence": Two objects are functionally equivalent (or analogous) if they do the same (or similar) things in the same (or in similar) systems in the same (or in similar) environments, etc. The key is the emphasis on the word "do." No other features of the objects are relevant other than the fact that they do the same things under certain conditions-which is to say that it is their behavior which is important. This is why (i) is not a separate variable—the variable is the behavior of an item. And indeed, it is one of the strengths of this analysis that it permits such a natural treatment of functional equivalence. [See WIMSATT 1972, and below.]

I will not defend including all of these variables in the function statement schema, (see WIMSATT 1972). The key to my argument is that changes in any of these variables may result in a change in what is chosen as the functional consequence (*C*), and that these variables are in fact independent. Thus each of them is required in the most general case.

A third feature of this schema is worth noting: this is indicated by the use of the phrase "the function" in the verbal formulation and the implication that the logical equation is single-valued—it is a function in the formal mathematical or logical sense. The claim (hereafter called the "uniqueness claim") is that when determinate values for the variables B(i), S, E, P, and T are plugged into the schema, they determine at most one value of the functional consequence C. (There may be cases in which for the values of S, E, P and T specified, B(i) has only disfunctional and/or nonfunctional consequences—in which case there are no functional consequences relative to the conditions specified and C has no value (again, in the mathematical sense of "value"). I argue below that this "uniqueness claim" cannot be maintained in general, but it plays a pragmatic and pedagogical role in elaborating the structure of functional organization and in generating the most useful representation of that structure. It increases the resolution of differentiated functional structure by determining any changes in conditions under which functional consequences are realized, and demonstrations of single or multiple functions all make heuristic use of this structure.

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Figure 1a: Simple tree structure (unipathic)

4. The Structure of Functional Organization: a Preliminary Attempt

It seems intuitive that a functional hierarchy should be a tree. I will show that this intuition is ultimately incorrect. It is still worthwhile, however, to analyze this structure in such a way as to make it as close to that of a tree as possible.

The intuition that functional hierarchies have a tree-structure probably is suggested by the inclusion relationships between the various systems and subsystems of a functionally organized entity. Parts of a system perform functions in that system, which may in turn be just one of several systems which perform functions in a still larger system. And each of the original parts may itself constitute a functional system, with subsidiary parts having functions in it. All of this suggests a tree-structure.

The functions of the various systems and parts in a given functional hierarchy are related. The function of a system determines the functions of its parts, which in turn determine the functions of the parts of the parts, and so on. Conversely, the fulfillment of function of the parts of a system contributes to (but does not entail) the fulfillment of function of that system and so on up the line to the most inclusive system in the hierarchy.

If the systems and parts are represented by nodes and the functional contribution a part makes to a system is represented by a directed arrow from the part-



Figure 1b: Multipathicity with nodes as physical object

node to the system-node, it appears that a simple treestructure, as represented in Figure 1a, is generated.

But this simple picture is incorrect if the systems and parts are construed as physical objects, as is common practice. Thus, for example, the circulatory system in mammals would include as parts the heart, arteries, veins, and peripheral capillaries. The thermoregulatory system in mammals would include parts of the endocrine and nervous systems, bodily hair, certain muscles (involved in shivering), skin pores (involved in perspiration), and the peripheral capillaries. Notice that the peripheral capillaries are parts of two distinct functional systems if these systems are regarded as systems of physical objects. Presumably, there is also a functional system at a higher level (perhaps, e.g., the organism itself) which contains both the thermoregulatory and circulatory systems as parts. This means that there would be (at least) two paths from the node representing the peripheral capillaries to the node representing the organism, one via the thermoregulatory system and one via the circulatory system. This situation is depicted in Figure 1b.

But a defining characteristic of a tree-structure⁷ is that it is "unipathic"—that there is *exactly one* path connecting any two points in that structure. It can readily be seen that this condition is not met in the above case.

The result is inexorable as long as the nodes—the systems and parts—are interpreted as physical *objects*. Thus BECKNER's characterization of function and of functional organization proceeds in terms of the functions of physical objects. Partially as a result, he speaks of the "net-like organization " of functional systems rather than seeing anything like a tree-structure (BECKNER 1969). But arbitrary multi-

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ply-connected nets do not possess very many logical properties which are usable in this context, so it is desirable to try another tack.

5. Nodes as behaviors and systems of behavior

I suggested above that attribution of functions to behaviors of objects had conceptual priority over attributions of functions to the objects or items themselves. If this suggestion is taken seriously, it seems natural to interpret the nodes as behaviors and systems of behavior. Indeed, on this interpretation, the case of the peripheral capillaries no longer blocks interpreting the functional hierarchy as forming tree structure. So there are formal as well as conceptual reasons for this move.

The peripheral capillaries have distinct behaviors in the thermoregulatory and circulatory systems. Their functional behavior in the thermoregulatory system is expansion and contraction (thereby changing the rate of heat exchange between organism and environment). Their functional behavior in the circulatory system involves (a) providing a closed loop for the return of blood through the veins, (b) allowing diffusion of nutrient materials (food and oxygen) and waste materials through their walls to and from the cells. Since these behaviors are distinct, they may be assigned separate nodes in the hierarchy, no one of which is contained both in the thermoregulatory behavior system and in the circulatory behavior system. Thus, the functional hierarchy for objects (Figure 1b) which does not have a tree structure is transformed into the functional hierarchy for behaviors (Figure 1c) which appears to.

6. The "Uniqueness Claim" and Its Significance

Interpreting nodes in the hierarchy as behaviors or systems of behavior is certainly necessary for giving the functional hierarchy a tree-structure, but is it sufficient? I will argue shortly that it is not—and that in general, the functional hierarchy *cannot* be analyzed into a tree-structure.

The "uniqueness claim" associated with the function statement schema is both necessary and sufficient for the functional hierarchy to have a tree structure. This is the claim that there is *at most one* functional consequence *C* for given values of the variables B(i), *S*, *E*, *P*, and *T*.

By definition, the functional hierarchy would contain only nodes representing behaviors which are functional, and not those which are either nonfunctional (selectively neutral) or disfunctional (se-



Figure 1c: Restoration of unipathicity with nodes as behaviors of physical objects.

lected against).⁷ Every node in the hierarchy will be directly connected to at least one other node in the hierarchy (the functional system in which that functional behavior is included) via the arrow representing its functional consequence (for that system).⁸ If nodes are individuated for each set of values of the variables B(i), S, E, P and T, for which there *is* a functional consequence, C, then the uniqueness claim says that for any given node there will be *exactly one* arrow from that node to a higher node—for there is exactly one functional consequence relative to those values of the variables. Situations like that of Figure 1b (which involve two or more arrows from a given node) are ruled out *in principle* if the uniqueness claim is accepted.

The uniqueness claim cannot be maintained in general, since there are cases in which the hierarchy does not (and cannot) have a tree-structure. But this does not end its usefulness. Functional analysts often act as if the uniqueness claim were true That is, when faced with a situation in which for given values of the variables B(i), S, E, P, and T, there appear to be two functional consequences, there is a tendency to redefine the values of one of the first three (and sometimes of the fourth) variables to individuate two or more values for one of these variables so as to maintain the uniqueness claim if possible. Functional hierarchy are commonly elaborated in this way, producing increases in structural detail. BECK-NER (1959) emphasized the extent to which biological systems and subsystems are defined in functional terms. Seen in this light, acting in accordance with the uniqueness claim (functional analysts surely do

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not *consciously* try to preserve it from falsification) can be seen as one of the major forces leading to the redefinition of biological systems and sub-systems and the relations between them. Thus it would seem to be a *good* practice to redefine functional behaviors, system, and environments in such a way as to maintain the uniqueness claim where possible. (This might be regarded as an analogy in the functional realm to applying Mill's methods for the discovery of causal relations.).

7. Closed Functional Loops and Failure of the Uniqueness Claim

Given the number of degrees of freedom in variables of the function statement schema, together with the possibility of *redefining* values of B(i), S, E, and P, why can't we always change, individuate, and redefine values of these variables so as to hold the uniqueness claim true come what may? There are two reasons why we cannot, and one of them appears to be inescapable.

Is it always possible to redefine values for these variables in a non-trivial way-in a manner which does not appear to be *ad hoc*—so as to maintain the uniqueness claim? It should not be fair, for example, to include the same behavior in two different systems under two different names, though different aspects of the behavior could well be fairly individuated. But determining when this does or does not occur presupposes prior decision upon criteria for individuating behaviors. This is not trivial. Note also that behaviors are generally individuated at least partially on functional criteria. If they were defined wholly in terms of functional criteria, no problems for the uniqueness claim could arise from this source. Nodes at different points in the functional hierarchy represent behaviors with different functional roles-behaviors which are by definition different behaviors if behaviors are individuated solely on functional criteria.

But behaviors are also identified and individuated on other grounds, including the identity of the behaving object, a variety of kinds of similarity, spatio-temporal location, and possibly other things. If any criteria other than purely functional ones are used, there is a possibility that they will conflict with the functional criteria and thus that the uniqueness claim may have to be given up.

The second problem for the uniqueness claim is less equivocal. This is the possibility that a functionally organized entity contains closed func-



Figure 2: Closed functional loops and cyclical multipathicity.

tional loops, as depicted in Figure 2. It is necessary but not sufficient for the existence of closed functional loops that a system contain closed causal loops. This is because a closed functional loop is composed of functional consequences, and a functional consequence is simply a causal consequence which is also functional. But not all of the causal interrelations in the operation of a functional system are functional, so the presence of a closed *causal* loop does not imply the existence of a closed functional loop.

An example of a closed functional loop (due to Allan GIBBARD) is as follows: the heart's pumping blood contributes through the circulatory system to the maintenance of structural integrity of all parts of the organism and thus to the maintenance of structural integrity of the ribs. But the maintenance of structural integrity of the ribs functions to protect the inner organs from injuries that would prevent or impair their functioning, and thus contributes to the heart's pumping blood.

Similarly, maintenance of homeostasis (of temperature, ionic balance, or whatever) contributes to the organism's ability to find food, and finding food con-

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tributes to the organism's ability to maintain homeostasis. More generally, any regulatory mechanism whose operation is functional should contain closed functional loops among its parts—though there may be closed functional loops which may be partially or even wholly outside of what we normally would think of as a regulatory mechanism—e.g., any closed functional positive feedback loop.

The presence of functional loops in the hierarchy defeats the uniqueness claim if none of the nodes in a loop is the topmost node in the hierarchy⁹. If none of the nodes in a loop is the topmost node in the hierarchy, an arrow must go from at least one of the nodes in the loop to a node higher in the hierarchy. But each node in the loop also has an arrow to some other node in that loop. Thus at least one node in the loop has two arrows leaving it—one to a node in the loop and one to a node outside of the loop. The uniqueness claim fails for this node. The claim may of course fail for several, and even for every node in the loop. It is reasonable to suggest, for example, that *each* node in a regulatory system has both internal and external arrows.

But could one save the uniqueness claim even here? Thus, one might argue that it always takes time for the functional effects at a given node to circulate around the loop and return to that node, and that the environment (E) had changed in the meantime. (It always does!) But this would be to trivialize the enterprise: The environment often has not changed in any relevant respects (e.g., if the system is at or near a regulatory equilibrium in a constant external environment). This *ad hoc* move would compromise any usable degree of generality in talk about the functional organization of different systems, or even the same system at different times or under different conditions.

In any case, this would not work if the external and internal functional consequences of a node in a loop are produced at the same time. Individuating different nodes for the same behavior at different times removes closed loops from the hierarchy but does not do away with the occurrence of multiple arrows from the same node.

8. The Elimination of P and T from Node Specifications

It was suggested above (section 6) that individual nodes be included in the hierarchy for each set of values of the variables, B(i), S, E, P, and T. But clearly this is more than required. If this is a general schema for function statements, such a move would include

every function statement made about any entity in the *same* functional hierarchy! As it turns out, *the values of* P *and* T *may be regarded as constant within a given functional hierarchy* without destroying the uniqueness claim for any nodes not already included in closed functional loops.

Three kinds of cases could lead to a change in the *T* variable:

(1) Different theories apply to different function statements because they refer to entirely different realms of phenomena such that one realm is not held to be a description of the other at a different level of organization or theoretical level. In this case, it is likely that one is talking about two distinct functional hierarchies. Thus, decision theory and learning theory might be held to apply to function statements about human action and evolutionary theory to the structure and behavior of organisms, but the functional hierarchy appropriate to the choice and explanation of human actions would generally be held to be distinct from the functional hierarchy appropriate to the selection and explanation of the biological behavior of organisms.¹⁰

(2) One theory of the operation of a system is replaced by another theory On his physiological theories, Aristotle thought that the function of the brain was to cool the blood. On modern theories, this is not even a candidate. Relevant functional hierarchies have been represented in different ways with the changes in physiological theories, often with one functional hierarchy gradually transforming into another, but sometimes with quite major changes if a major functional assessment is changed, because that often forces reassessment of many other functional assignments. [This is a product of what I have more recently called generative entrenchment. See RIEDL (1978), WIMSATT (1986) and SCHANK/ WIMSATT, (1988).] But in any case, no one thinks that the functional hierarchy of a biological entity should now contain any functions attributed on the basis of past rejected theories. It is reasonable to suggest that at any given state of theoretical knowledge of a system, the T variable is constant within a functional hierarchy. Of course, T must sometimes be taken to represent a set of mutually compatible theories, {T_i}, rather than a single theory. This leads naturally to the next kind of case:

(3) The level of investigation and description of a system is changed, and this change in level carries with it a change in the theories used. Biologists frequently apply different theories at different levels simultaneously in describing, analyzing, and explaining the organization of living things. Thus, in

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a chapter on hemoglobins, JUKES (1966)¹¹ discusses the amino-acid substitutions in the hemoglobin molecule characteristic of sickle-cell anemia victims, their effects upon the tertiary structure of that molecule under certain biochemical conditions, speculates on the cellular effects in an attempt to analyze the characteristic breakdown of the erythrocytas (red blood cells) at low concentrations of oxygen in the bloodstream and discusses the resultant pathological effects on the organism. He also appeals to population genetics to explain why the genes which cause sickle-cell anemia are maintained in a population in spite of their disfunctional traits.¹¹

But though different theories are applied at different levels, here, these are different *functional* levels of organization. These different theories will characteristically add nodes and arrows (functional behaviors and consequences) at different levels of the functional hierarchy. But the uniqueness claim could not be invalidated by different theories unless they added functional consequences at the *same* node—and thus, trivially, at the same level in the hierarchy. The net effect of using such theories at different levels will be to force interpretation of the *T* variable as referring to the set of such theories used, rather than to regard them as competing theories having competing claims on the structure of the functional hierarchy.

It thus seems that in the three types of case discussed, changes in the *T* variable cannot disturb the structure of a given functional hierarchy. While a set of theories may be characteristic and determinative of the structure of a given hierarchy, changes in the *T* variable need not be considered as a potential individuator of nodes *within* that hierarchy.

Changes in the "purpose" or P variable are simpler to discuss. Purpose-attainment is promoted by the operation of the top-most system in the functional hierarchy and determines the functions of the nodes all of the way down through the various levels of that hierarchy. A variation in P which produces another functional consequence can be ignored because the other functional consequence is *not contained* in the given hierarchy. Changes in *P* values change the hierarchy under consideration.¹²

But defense of the uniqueness claim requires that *P* be a "simple" purpose—one whose attainment can be characterized without using two or more logically independent criteria (WIMSATT 1972). Purposes which require two or more logically independent criteria for the description of their attainment-states may be described as complex. But many purposes are complex in this sense, and it may be worthwhile to

speak of a single functional hierarchy for a given complex purpose. In this case, it would be necessary to regard changes in the value of the P-variable as individuators of nodes. But also in such a case, it is natural to regard such a complex hierarchy as a set of "simple" hierarchies (with a constant value for P within each) which are "strung together" only at the top to yield the hierarchy for the complex purpose.

So in sum, it is sufficient to maintain the uniqueness claim within a given hierarchy (without cycles) that a separate node is individuated for each set of values of the variables B(i), S, and E. Variations of Pand T can be ignored within a given hierarchy because they are constant within it.

9. The Characterization of the Environment

It might be a good idea to treat changes in the environment in the same way as changes in the P variable, individuating different functional hierarchies for each environment, and perhaps "stringing them together" at the topmost nodes to get the total hierarchy under different environmental conditions. But it is not possible to separate characterizations of the environment from the individuation of nodes within a given hierarchy and still organize the hierarchy in a nice manner. This is due to practical constraints on how the environment is to be characterized in studying such systems: just as the systems are characterized and analyzed at different levels in the hierarchy, it is fruitful to characterize and analyze the environment in the same way, so that each system or subsystem can be studied in its own environment—systems and environments must complement each other all the way down.

Different values of *P* may plausibly involve different functional hierarchies, but not so for different values of E. Even if the nodes are interpreted as behaviors, the hierarchy gives the functional organization of a complex *object* which behaves. We apply spatio-temporal criteria in identifying objects, and this involves persistence of this object in a variety of environments. Each organism must face environmental changes, both within individual life cycles and in successive generations. Selection operates upon the differential ability of different evolutionary units (organisms, groups, etc.) to survive under such environmental variations, and the functional hierarchy of an evolutionary unit is the product. It would be extremely artificial to say that this hierarchy is "really" a composite of separate hierarchies for distinct environments. (Logical separability does not imply

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the conceptual or theoretical fruitfulness of performing the separation.)

Suppose that individual subhierarchies were individuated for each value of E. Does this mean that E will be constant within a given subhierarchy? That depends upon how *E* is characterized. If each environmental subhierarchy is individuated solely on the basis of changes in the environment of the system at the topmost node of the subhierarchy, the environments of subsystems of those systems will not be totally specified. There will be conditions internal to any system which are external to any one of its subsystems (such as the states of its other subsystems). Changes in these conditions will not be included in the environmental specifications of higher nodes, and thus may act as individuators of nodes at each level in the hierarchy.

One could always say that the environment of the topmost system includes not only its environment, but also its internal state description as well. Then there would be no branching with respect to environmental differences within a given environmental subhierarchy, but this move has little else to recommend it, and violates common sense. On this alternative, each subhierarchy would require "apocalyptic" specification of the environment at all levels. One couldn't investigate one subportion of a hierarchy at a time in local fashion (as is the usual practice), and the difficulties in filling in all of this information would be insurmountable.

The first alternative reflects and facilitates the manner of comparative investigations of the behavior of a system and its subsystems in different environments in that the environment can be "cut up" by levels and conceived of as varying one level at a time, and subparts of the system may be bounded and studied locally. Any node represents a system in its given environment. The nodes below it represent its subsystems in variations of their environments which are internal to that system and its environment. The results of a comparative investigation at some level will tend to fill in information concerning immediately connected nodes on the first mode of representation, but would fill in nodes in widely separated "corresponding subbranches" of different environmental subhierarchies on the "apocalyptic" second. The first proposal, but not the second, captures the intuition that a given investigation is performed at a certain level for a certain range of conditions holding at that level by localizing the information discovered in a well-defined compact sub-region of the hierarchy.

Finally, the first proposal has a substantial advantage over the second if the tree is simplified by merging nodes for functionally equivalent environments. (Roughly, functional equivalence for two environments with respect to a set of systems and subsystems is the isomorphism of the arrangements of the nodes and arrows assigned to those systems and subsystems in the two environments.) But if an environment is an entire state description of the topmost node and its environment, two environments are functionally equivalent if and only if the entire hierarchy of systems and parts is isomorphic. If the environment is characterized by levels, two environments isomorphic only at certain levels of the hierarchy can be grouped together at those levels.

To summarize the results of the last few sections, the structure of the functional hierarchy is as follows: With no closed functional loops, the hierarchy can be viewed as a composite tree composed as follows (see Figure 3):

- 1. If the hierarchy is for a complex purpose, it consists of a number of "simple purpose" hierarchies "strung together" at the top.
- 2. Each "simple purpose" hierarchy in turn consists of a number of hierarchies, one for each set of functionally equivalent environments of the topmost system, strung together to get the functional hierarchy for that system in the range of environments considered.
- 3. The hierarchy for each set of functionally equivalent environments for the topmost system node may be viewed as a composite of trees which branch solely with respect to the system-subsystem relation and the environment-subenvironment relation for those systems, resulting in a single tree which branches simultaneously with respect to both relations.
- 4. Closed functional loops are added as required (a) connecting all of the parts in a functional regulatory or amplification system, and (b) wherever every link in a closed causal chain is a functional consequence.

10. Non-Functional and Disfunctional Causal Relations and the Functional Hierarchy

Although a complete functional hierarchy gives a complete characterization of the *functional* organization of a system, it would fall short of giving a complete description of the causal organization of the system.¹³ By definition, any causal interactions which are either functionally neutral (nonfunctional) or disadvantageous (disfunctional)

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Figure 3: Different elements of a schematic biological functional hierarchy. *Notes*: (1) If a social unit is a functional unit, it should be represented as a single node rather than as it is here. (This node would be above that for organisms and below that for breeding populations.) (2) Selection acts as feedback which changes the structure of the hierarchy. It can operate at different levels with different units and can affect traits at other levels. Since selection changes genes and gene frequencies, thereby affecting the behavior of genes in their genetic, somatic and environmental milieu, changes induced by selection are represented as acting downwards from the relevant unit of selection whose functions are served to a gene or genes as represented through their behaviors, and then propagating upwards from the bottom-most nodes of the hierarchy. (3) The functional hierarchy does not noramlly include non- and disfunctional behaviors and interactions, though they may be respon-sible for the presence of many functional sub-systems which are included and influence the form and architecture in myraid ways. They are represented here to indicate that they can connect different sub-branches of the hierarchy. (4) Systems individuated on non-functional criteria (e.g., genes, anatomical organs, and physiological systems) generally map into a number of different nodes in the functional hierarchy.

would not appear in the functional hierarchy. But functionally neutral and disadvantageous causal interactions cannot be ignored because they are instrumental in determining the structure of the functional hierarchy.

In organisms, not all functional systems make direct positive contributions to purpose-attainment.¹⁴ Some clearly have the function of preventing or minimizing the effects of certain disfunctional occurrences or interactions. Although these various disfunctional interactions are not included in the functional hierarchy, their effects are certainly manifest in its structure: the "preventive" functional systems would not have been selected for or be contained in the hierarchy were it not for the relatively frequent occurrence of such disfunctional interactions. Indeed, large sections of the hierarchy and features of its organization would be incomprehensible without taking them into account.

A prime example of such "preventative" or "ameliorative" functional systems are immunological mechanisms. It is not functional that viruses invade

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the body and convert our protein producing mechanisms to the production of viral proteins. Nor is it functional that bacteria enter the system, producing toxins which kill cells. But given the frequency and importance of such occurrences,¹⁵ it is functional to have a system—whose degree of complication roughly measures its importance—for neutralizing the presence of or disposing of viruses and bacteria. Similar remarks apply also for various healing mechanisms.

It is not just the disfunctional operation of foreign agents within the system which is left out of the functional hierarchy. There may be many functionally neutral traits of biological organisms.¹⁶ There are many cases where the interaction of functional systems are either outright disfunctional or are at least antagonistic. None of these occurrences, traits, or interactions are included in the functional hierarchy.

In certain circumstances, the immunological mechanisms of the body can become sensitized, not only to foreign proteins, but also to the body's own proteins. In this case, the immunological mechanisms destroy the body rather than protecting it, and are interacting with the other bodily functional systems in a disfunctional manner.

In cases of extreme heat loss in mammals, when the thermoregulatory system cannot keep the temperature of the whole body at an acceptable level, circulation to the extremities decreases radically, and their temperature decreases. But the capacity of the thermoregulatory system to resist temperature decreases in the crucial organs is thereby increased. So normal functioning (under normal conditions) of the thermoregulatory system is impaired under extreme conditions in order to increase the ultimate chances of survival. (A man with frozen extremities has a better chance than a man with a frozen brain.)

It would of course be better (other things being equal) if the thermoregulatory system had sufficient heat production capacity to meet all emergencies without restricting circulation to the extremities, but any physical system has physical limitations. These limitations make it radically improbable that all causal interactions in a system are functional under all conditions. Those causal interactions which are non-functional and disfunctional, and the conditions under which they are non- and disfunctional, place constraints on the structure of the functional hierarchy even though they never appear there explicitly. Decisions whether things which appear to be locally disadvantageous might in fact be globally advantageous—or the reverse—are among the most difficult issues to resolve in complex functionally organized systems.¹⁷

Indeed, we need to be methodologically attuned to the analysis of disfunctional effects even when we are seeking only the functional organization of a system: the study of how the operation of a functional system changes "when things go wrong" is a very powerful way of determining its functional structure, but must be done with some care and with detailed consideration of all three types of causal interactions in the system.¹⁸

It is not necessarily a disadvantage of the functional hierarchy that it does not include all of the causal interactions of a functional system. Any theory of the behavior of systems picks out certain key features of the system and ignores others. For evolutionary or selectionist theories, the structure and nature of the functional hierarchy is of prime importance, and other features of the system in question are of theoretical importance only insofar as they affect this structure. The fact that functional hierarchies do not capture all of the causal interactions in the systems they apply to is appropriate to the nature and modes of generalization of these theories.

Part II: Pragmatic Features of the Elaboration of Functional Hierarchies and the Logic of Functional Analogy

11."Real" vs. "Ideal" Functional Hierarchies

If "function" is conceptually more closely connected with behavior than with objects (section 3) and if functional hierarchies can be given a logically more desirable structure if they are constructed in that way (sections 4 and 5), why do writers tend to attribute functions most frequently to physical objects and to think of functional systems primarily as systems of physical objects?¹⁹ The heart of this puzzling anomaly lies in practical considerations involved in the process of elaborating and investigating functional hierarchies.

The conceptual priority of behavior over objects in the analysis of function does not give behavior an observational priority. Behavior is always behavior of an object, and behavioral interactions are interactions between objects. It is objects and changes in objects which are characteristically observed—not behaviors. Given that functions can (derivatively) be attributed to physical objects and systems, it is not too surprising that people tend to do so.

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Most investigations of functional organization begin with an investigation of the static physical structural properties of the functionally organized system. The "statics" of a system is almost always easier to analyze than its dynamical interactions. It is no accident that physics students are taught statics before dynamics, and that VESALIUS conducted detailed anatomical investigations at a stage when physiology seemed still a jumble. As a rough maxim, it is reasonable to suggest that substantial knowledge of the structure of a system is not only more easily acquired than, but also must precede even relatively primitive analyses of, its dynamical interactions. (This doesn't mean that knowledge of structure must always stay "ahead" of knowledge of dynamical interactions, or that dynamical investigations cannot contribute anything to knowledge of structure, but only that in the beginning of an investigation of a system, some minimum knowledge of structure must come first.) So it is not surprising that the ontology of a system's static structure-individual physical objects and their spatial relations-should be applied also to its dynamic structure, though with somewhat less success.

The structure of a functionally organized system is partially determined by its functional organization, and its structure thus affords clues as to the form of this organization. Since static physical structure is characterized by the individuation and interrelation of physical objects, the simplest hypothesis concerning the relation of structure and function is that functions are in one-one correspondence with these objects. This hypothesis seems to be confirmed in some cases, and especially at early stages in an investigation, but frequently turns out to be wrong. Yet even after investigations have progressed to the stage that mutually exclusive functional systems are shown not to correspond to mutually exclusive systems of physical objects, the tendency persists to regard functional systems as systems of physical objects-but with "something added."20

To suggest that functional hierarchies should be regarded as functionally interrelated behaviors and systems of behavior does *not* mean that functional analysts can afford to follow this recommendation in practice. One would expect just the opposite! While an ideal completely elucidated functional hierarchy might contain only behaviors of physical objects and no direct reference to the objects themselves, functional analysis must, especially in the early stages, proceed in terms of systems of objects. As progress is made in analyzing the functional organization of a system, it is found more often that individual functions are performed jointly in a series or independently in parallel by more than one physical object in the system, and that individual objects in that system may perform a variety of functions.²¹ It then becomes increasingly conceptually profitable to construe functional systems as systems of behavior rather than as systems of objects. This will generally be done piecemeal in various branches and subbranches of the functional hierarchy. Thus, at any intermediate stage in the elaboration of a functional hierarchy, one could expect to find some nodes interpreted as behaviors and other nodes interpreted as physical objects. But even after the analysis is finished, physical objects are not completely dispensable: It would be impossible to verify that a given system exemplified a given functional hierarchy without analyzing the static and dynamical relations between various physical parts of that system.

12. Pragmatic Criteria for the Attribution of Functions

I have so far discussed two extremes in the elaboration of functional hierarchies. In early stages, with comparatively scanty information concerning static and dynamic characteristics of a system, most or all of the nodes will be interpreted as physical objects, and will be assigned functions on some basis or other. At the other extreme, individual *behaviors* in well-defined *systems of behavior* in given environments characterized down to the level of that behavior will be assigned nodes in a given hierarchy if their occurrence contributes under those conditions to the attainment of the purpose at the top of that hierarchy.

But how are functions assigned to physical objects at the first stage, and how are they assigned and individuated in the long course of successive analyses that lead to the completed "ideal" functional hierarchy? Roughly, functional organizations are kinds of machines, whose articulated parts contribute to the ends specified by a selection process-whether internal or external and whether of natural or artificial origin. For details of how these intuitions are played out-the various articulated conditions which the parts must meet, and how they are to be evaluated, see WIMSATT 1972. Roughly, each functional item must produce or contribute to its functional consequence in the appropriate circumstances, and that must-percolating up through the functional hierarchy-

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contribute to purpose attainment, again under appropriate circumstances. Explicating this brings in all of the variables of the function statement—B(i), *S*, *E*, *P*, *T*, and *C*. This is the primary criterion. But there are a variety of other heuristic ones. They don't guarantee results, but they can be very useful. These are, as it were, existential criteria. They don't identify a function. They serve to make it likely that something has a function, or (in the case of (3)), place some constraints on what can be identified as the function. Thus:

1. 1. the complexity of a system is assumed to correlate with the complexity either of the task it is to perform, or the complexity of the way and conditions under which it is to be employed.

And, since it takes some effort to maintain complex systems,

2. the elaboration and persistence of a complex system indicates a magnitude of its importance comparable to the complexity of maintaining it.

And, since we assume that any complex system will be constructed piecemeal,

3. the story told for the function of the system must be consistent either with its piecemeal elaboration for that function, or its elaboration and successive co-option—as a repeated exaptation through a succession of other functions.

Note that on this analysis (unlike that of WRIGHT (1973) or MILLIKAN (1984)), these are heuristic and pragmatic requirements—not logical ones.

We frequently also attribute functions on the basis of analogy in the broadest sense, which includes both homology and analogy as those terms are commonly used by functional morphologists, and also simple physical similarity. These are more pointedly useful, as they suggest specific functions on the basis of knowledge of other systems that we may have. I want to consider these criteria further, because they are particularly important for cross-species and cross phylum, or even broader comparisons of functional similarity. More explicitly, two pragmatic criteria for the attribution of functions are:

- 4. Similarity of the given object, set of objects, behavior, or set of behaviors with another object, set of objects, behavior, or set of behaviors with respect to *physical* features. Where the function of the second entity is known, the same or a similar function is attributed to the first.
- 5. Similarity of the given object, etc., with another object, etc., with respect to *functional* features. Where the function of the second entity is known, the same or a similar function is attributed to the first entity.

For the most part, these criteria raise more questions than they answer. These two criteria are at least partially independent and can conflict. There is a species of orchid in which an interior part of the flower resembles in appearance the female reproductive apparatus of the species of bee responsible for its pollination (DARWIN, 1876). In his abortive attempt to mate with the flower, the male of the species picks up pollen which is carried to the site of his next deception. The biologist disregards the physical similarity of parts of the flower and female bee on what are ultimately functional grounds. (Bee-flower matings do not produce fertile offspring.) A functional analogy, the fact that this adaptation attracts bees (as other flowers do with nectar or special color markings) is employed to place this adaptation in the orchid's functional hierarchy, rather than that of the bee, and to identify it as connected with the function of pollen dispersal. The visual similarity of this part of the flower with anything else to be found in nature gives only misleading clues as to its function.

Criteria of physical similarity might conceivably overwhelm *some* functional analogies in classifying the function of a given entity, but here presumably the physical similarity in question indicate a deeper or more fundamental functional analogy along different lines. It thus ultimately seems to be a deeper functional analogy which overwhelms a more superficial functional analogy.

I have nothing more to say about physical similarity, but more should be said about functional similarity—especially of the practical variety employed at stages where the functional hierarchies of the various systems being considered are relatively incompletely elaborated.

13. Varieties of Functional Similarity

Various writers talk freely abut functional identity, equivalence, isomorphism, correspondence, and analogy, and appear to use these terms almost interchangeably. But a much larger number of distinguishable concepts of functional similarity can be defined in terms of functional hierarchies. Without giving all possible combinations, I will outline the kind of considerations which can act as distinguishing dimensions of these mods of comparison. I will then discuss more fully the weaker forms of functional similarity which seem to be applied in practical cases.²²

In general, the entities which are said to exhibit functional similarities are entities whose functional

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structure is represented by *parts* of the functional hierarchy. A *part* of the functional hierarchy is a subset, (not necessarily a *proper* subset), of the nodes in the hierarchy together with some or all of the arrows connecting these nodes such that sufficient arrows remain in the part to connect all of the nodes in that part.

These "parts" might correspond to behaviors, systems of behavior, physical parts or systems of physical parts, functional consequences and sets of functional consequences, or non-homogeneous combinations of these. They will show at last some integration, because of the connectivity condition. It is possible to consider environments and sets of environments as well, for these also correspond to "parts" in this sense, even though they might not generally be considered to be parts of the functionally organized system on spatial criteria.

All varieties of functional similarity as defined here share one thing in common: they are at least partially defined in terms of isomorphisms—between the parts in question, between parts of the parts, between other parts of the hierarchy of which the parts are parts, or between other hierarchies or parts of hierarchies of which the parts could be parts. By *isomorphism* I mean that the nodes and arrows of the two structures said to be isomorphic can be placed in 1-1 correspondence such that each has the topological properties of the other when the arrows are interpreted as *directed* arrows.

There are four major kinds of considerations involved in talking about types of functional similarity.²³ These might be called, respectively:

- 1. conditions of isomorphism;
- 2. conditions on similarity of purpose;
- 3. conditions on the scope of isomorphism;
- 4. conditions on quantitative degree of functional similarity.

These are four *classes* of conditions. One condition in each class must be met for each type of functional similarity. Furthermore, with the possible exception of some of the conditions in class 3, all conditions are mutually exclusive, and except as noted below, all are logically independent. To the extent that they are, *x*, *y*, *z*, and *w* conditions in classes 1, 2, 3, and 4, respectively, would produce $(x \cdot y \cdot z \cdot w)$ possible distinguishable types of functional similarity. On *a priori* grounds, this results in very many types of functional similarity. Fortunately, the conditions themselves are of more interest than a botanizing lexicon of their combinations, reducing the task to a discussion of (x + y + z + w) conditions.

14. Conditions of Isomorphism

Isomorphisms can be studied under two kinds of conditions. The first and stronger condition is that the two parts in question be *intersubstitutable* in a set of hierarchies, a given hierarchy, or part of a given hierarchy without disturbing the structure of that set, hierarchy, or part. This can be regarded as the isomorphism of a set of hierarchies, a hierarchy, or a part of a hierarchy with itself before and after the substitution. This is the kind of functional equivalence appropriate when considering spare or replacement parts in engineering contexts, or in cases where there is internal redundancy of organs or genes, or functional equivalence of different alternative courses of behavior.

Alternatively, one can ask whether two sets of hierarchies, two hierarchies, or two parts of the same or of different hierarchies are isomorphic to each other. This might be called *comparative* isomorphism, and is the sense of functional equivalence or similarity appropriate in most evolutionary contexts, and essentially all cases of "comparative functional morphology."

Two entities can be comparatively isomorphic without being isomorphic under substitution, but not conversely. Thus, consider two computers of identical logical structure. This logical structure in the first is realized via electronic switching elements and in the second by hydraulic "fluid logic" components.²⁴ Presumably, the functional hierarchies of these two computers would be comparatively isomorphic. But one could not thereby simply replace a worn out part in one computer by the corresponding part in the other computer and expect the new part to function in the same way as the part it "replaced." One could of course include special "translating" devices-electrical/hydraulic and hydraulic/electrical "transformers" and add the relevant power supplies, but (as anyone with an artificial heart or kidney knows, this would require a substantial change in the supporting functional organization of the whole installation, and thus also destroys full isomorphism under substitution for that case.)

If two entities are functionally intersubstitutable under a set of conditions, they are also comparatively isomorphic in corresponding systems. Mass-produced parts are designed to be intersubstitutable and two physical systems of the same type produced on an assembly line with the same corresponding intersubstitutable parts will clearly be comparatively isomorphic.

15. Conditions On the Similarity of Purposes

An examination and comparison of structure within a hierarchy places no explicit constraints on the nature of the purpose associated with the hierarchy.²⁵ Yet the nature of the purpose associated with a functional hierarchy clearly has some relevance in comparative judgments of functional similarity. Among possible constraints on two purposes being compared, three come readily to mind:

- 1. The purposes at the top of the hierarchies being compared are *identical*.
- 2. The purposes at the top of the hierarchies being compared are *similar in respects* P_i
- 3. The purposes at the top of the hierarchies being compared are *dissimilar in all respects*.

These three conditions are arranged in order of decreasing functional similarity, but the apparent precision of this list is misleading. I have not discussed general criteria for the identity of purposes, or for their similarity, and do so here only in rough terms, illustrated with a few examples.

Purposes might conceivably be characterized either extensionally (by listing the set of states which count as purpose-attainment states) or intensionally (by listing the criteria for purpose-attainment states.) For various reasons the second way is preferable. (It is not even clearly possible to define purposes in the first.) It seems reasonable to take as the criterion for the identity of two purposes that their criteria for purpose attainment states are identical in meaning.²⁶ For scientific applications, it may be appropriate to weaken this requirement somewhat. Thus, one might say that two purposes are identical if and only if their criteria for purpose-attainment mutually entail each other according to all of the relevant scientific theories for all ranges of conditions within the scope of these theories. (This is an approximation to saying that the two purposes have the same "scientific meaning.")

Some examples may be useful. Suppose that two persons are playing a game that only one of them can win. It might be tempting to say that they both have the *same* purpose—each wants to win the game. But it seems plausible to say that two purposes cannot be identical if they can conflict. Thus they both have the same purpose only if they both want the *same* one of them to win the game, since if each one wants himself to win the game, their purposes conflict.

Similarly, two evolutionary units, X and Y, may have "the" purpose of maximizing their probability of survival, but these are two distinct purposes. When unpacked further, this statement says that one evolutionary unit has the purpose of maximizing its (X's) probability of survival, while the other evolutionary unit's purpose is to maximize Y's probability of survival. These purposes all involve disguised idexical references, and the conditions of identity for such purposes must include identity of the index.

There are situations in which purposes appear without an implied indexical reference, however, and these might be called general purposes. Thus, when someone says that the purpose of *a* car is to provide transportation, of a lathe is to machine metals in axially symmetric ways, of *a* game player is to win the game, or of an evolutionary unit is to maximize its probability of survival, this purpose is held to be the same for different cars, lathes, game players, and evolutionary units. Such general purposes may be regarded as referring to what is common to a class of indexical purposes defined by the characteristic that if they had the same indices they would be the same indexical purpose. General purposes are really types of purposes which are characteristic of certain kinds of objects.27

There are a number of possible ways in which purposes can be similar. For indexical purposes, one important way would seem to be that they are both indexical specializations of the same purpose-type. Other modes of similarity (which would apply both to indexical and to general purposes) might include:

(a) for complex purposes (see section 8 above), having some of the criteria for purpose attainment in common. Thus, "seeking advancement and power" and "seeking advancement and pleasure" have "seeking advancement" in common.

(b) for either complex or simple purposes, the criteria for attainment of two purposes may share certain key concepts. Thus, e.g., "the survival of the organism", "the survival of an evolutionary unit" and "the survival of the state" all make use of the concept of survival. "Homeostasis", "adaptation", and "utility" represent other key "portable" concepts.

Note that the last type of similarity might be regarded as a higher level generalization of general purposes, where the types of systems are no longer just "organisms", or "evolutionary units" or "states" but something like, e.g., "definable units which tend to organize themselves in such a way as to increase their probability of survival." This is on a narrow interpretation of (b) however, for the key concepts need not occur in parallel ways in the different purposes. For example, "maximizing homeostasis", "minimizing homeostasis", and "maintaining the degree of ho-

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meostasis at a constant level" are all possible purposes which make use of the concept of homeostasis.

This list of ways in which purposes may be similar is meant to be suggestive, rather than exhaustive. It seems quite likely that a closer examination of anthropomorphic and anthropocentric reasoning would turn up other ways in which purposes can be similar. (This is not to say anything about legitimacy, for that does not affect whether one might learn something about criteria for similarity of purposes by looking at such work.)

16. Conditions On the Scope of Isomorphisms

One obvious way of generating a number of different concepts of functional similarity is to individuate a number of different requirements for the scope of the isomorphisms which must be present. In some of these requirements, the definitions of scope will be treated as if the functional hierarchy had a tree structure. Where appropriate, the modifications necessary to generalize the definitions to take account of the presence of functional loops will be indicated.

The first six requirements of scope (three basic requirements, which apply both to isomorphism under substitution and to comparative isomorphism) are as follows:

(S.1) *Absolute substitutional isomorphism*: Two parts are intersubstitutable in any hierarchy in which *either could* occur without changing the structure of that hierarchy.

(S.2) *Absolute substitutional homomorphism*: One of the parts is intersubstitutable in any hierarchy in which the *other could* occur without changing the structure of the hierarchy.

(S.3) *Substitutional isomorphism relative to hierarchy* H_i : Two parts are intersubstitutable in hierarchy H_i without changing the structure of that hierarchy.

(S.4) *Absolute comparative isomorphism*: For each hierarchy in which *either* part could occur, there is a corresponding hierarchy which is isomorphic with the first and which the other part occurs as a corresponding part.

(S.5) *Absolute comparative homomorphism*: For each hierarchy in which *one* of the parts could occur, there is a corresponding hierarchy which is isomorphic with the first and in which the other part occurs as a corresponding part.

(S.6) *Comparative isomorphism relative to hierarchies H_i and H_j*: Hierarchies H_i and H_j are isomorphic, and the two parts are corresponding parts of these hierarchies. While possibly of formal interest, these requirements of scope are so strong that most are of little use in practice,²⁸ or met only in certain limiting cases²⁹. I have included here formulations for both isomorphism under substitution and for comparative isomorphism so that their differences could be seen. In the future I will discuss only isomorphism under substitution, as the definitions have a simpler form. In each case, however, a corresponding sense of isomorphism under comparison applies and could be given, and this will be indicated by "double numbering" of each definition.

17. Varieties of Partial Similarity

Assume for the time being that the functional hierarchy has a tree structure. (Roughly, if whole cycles are treated as nodes, hierarchies with cycles assume a tree-structure, though if there are many cycles at all, or any very important ones, there is substantial danger that most of the functional complexity will be collapsed to a single (very complex) node.) This fact allows defining structural properties of hierarchies with cycles relatively simply in ways suggested by definitions of corresponding structural properties of trees.

The necessary concepts are that of a path between two nodes, the distance between two nodes, the topmost node, above, below, and betweenness for nodes, the branch and branch-complement of a node, and a superbranch, superbranch-complement, sub-branch and sub-branch complement of a node. All of these concepts are informally characterized below. (These, and the following may be more formally defined using graph theory. See, e.g., HARARY et al. 1965, or the definitions in WIMSATT 1971.)

A *path* is an ordered sequence of connected alternating nodes and arrows traversed in the direction indicated by the arrows. A node or arrow is *between* two other nodes or arrows if there exists a path from one of these latter nodes or arrows to the other which includes the given node or arrow. The (or a) *topmost node* of the hierarchy is the (a) node which has no paths leaving it. A node or arrow is *below* a second node or arrow if a path from the first node or arrow to the (a) topmost node contains the second node or arrow. The second node or arrow is then *above* the first.

The *distance* between two nodes or arrows is the number of arrows in the shortest path between them. The *length* of a path is the number of arrows in that path. The *level* of a node is the number of arrows in the path from that node to its topmost node.

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The *branch* of a node is that node together with all nodes and arrows which are below it. The branch of a node is a *superbranch* of the branch of any node below it, and the latter branch is a *sub-branch* of the first. The *branch-complement* of a node is what remains of the hierarchy when that node's branch is removed. The *order* of a sub-branch or superbranch is the distance between the branch-node and the superor sub-branch node. A *superbranch* (*or sub-branch*) *complement of order n* of a node is what remains of the superbranch of order *n* (or branch) when that branch (or sub-branch of order *n* (or branch) when that branch (or sub-branch of order *n*) is removed.

18. Internal, External, and Total Functional Similarity

One natural way of classifying isomorphisms in the same hierarchy (under substitution) or in different hierarchies (under comparison) is to ask where the isomorphisms occur relative to a certain node, or set of nodes. Since a given node in the hierarchy corresponds to a functional system in a class of functionally equivalent environments, and all of the nodes below it correspond to its parts at various levels of functional analysis, it can readily be seen that isomorphisms in the branch of that node refer to internal functional features of that system, and isomorphisms elsewhere in the hierarchy or hierarchies refer to external functional features. Isomorphisms of internal functional features of systems imply similarities in the manner in which they produce their functional consequences. Isomorphisms of external functional features imply similarities in the role their functional consequences play in larger systems of which they are parts.

A physical part will in general correspond to a set of nodes in a functional hierarchy, with one distinct node for each of its functional behaviors. Examples of internal and external functional similarity for physical parts can be drawn from an examination of the design of different internal combustion engines. Two and four-cycle reciprocating engines, WANKEL (rotary) engines, turbojet, ramjet, and pulsejet engines, and rocket and diesel engines can all be designed in such a way as to use spark-plugs for ignition. These spark-plugs could all be internally functionally similar (they "operate" in the same way) but they would not be externally functionally isomorphic in these different types of engines. On the other hand, any of these different types of engines could be designed to use surface electrode or gap electrode spark plugs or "glow" plugs. These three different types of plugs are not internally functionally similar (they operate in different ways) but

would play the same external functional role in any given type of internal combustion engine.

If two entities are both internally and externally functionally similar, they will be said to be *totally* functionally similar. This obviously corresponds to (S.3) for substitutional isomorphism and to (S.6) for comparative isomorphism.

To put these definitions in more explicit form:

(S.7,8) *Internal functional similarity*: A hierarchy is *internally* functionally isomorphic under substitution with respect to a node or set of nodes if it is self-isomorphic before and after the substitution in all of the branches of that node or set of nodes.

(S.9,10) *External functional similarity*: A hierarchy is *externally* functionally isomorphic under substitution with respect to a node or set of nodes if it is self-isomorphic before and after the substitution in the branch-complement of that node or in the intersection of the branch-complements of that set of nodes.

There are at least two essential ways in which it may be important to consider a set of nodes in these judgments of functional similarity. The first is that we may want to talk about functional similarities of two physical parts in a given environment or set of functionally equivalent environments. Because this part may exhibit several functional behaviors in different functional systems, it is necessary to consider the nodes corresponding to these behaviors. Secondly, it might be desirable to consider the isomorphisms of two given functional behaviors over a range of functionally different environments. This again brings in a set of nodes. These two investigations may also be combined, of course, as when one investigates the functional similarities of two physical parts over a range of functionally different environments.

Internal and external functional similarity as defined above are still extremely strong requirements, but various weaker forms of internal and external functional similarity can be defined.

19. Relative Internal and External Functional Similarity

Using the concepts of a sub- and superbranch, suband superbranch complementation, and the order of a sub- or superbranch of sub- or superbranch complement, it is possible to define various forms of relative internal and external functional similarity within a given hierarchy.³⁰ Thus:

(S.11,12) Internal functional similarity relative to a sub-branch complement of order m: A hierarchy is in-

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ternally functionally isomorphic under substitution relative to a node and a sub-branch complement of that node of order *m* if it is self-isomorphic before and after the substitution in that sub-branch complement.

(S.13,14) External functional similarity relative to a superbranch of order m: A hierarchy is externally functionally isomorphic under substitution relative to a node and a sub-branch of that node of order m if it is self-isomorphic before and after the substitution in that sub-branch.

(S.15,16) Internal functional similarity relative to a sub-branch of order m: A hierarchy is internally functionally isomorphic under substitution relative to a node and a sub-branch of that node of order m if it is self-isomorphic before and after the substitution in that sub-branch.

(S.17,18) External functional similarity relative to a superbranch complement of order m: A hierarchy is externally functionally isomorphic under substitution relative to a node and the superbranch-complement of order m of that node if it is self-isomorphic before and after the substitution in that superbranch complement.

These types of functional similarity can all be viewed as ways of ignoring the whole functional hierarchy and looking at functional similarities at places defined in terms of their location from the node in question. Thus, the two senses of relative internal functional similarity can be viewed as the reapplication of external and internal functional similarity respectively, within the branch of a node (instead of within the whole hierarchy) relative to a sub-branch of that node (instead of relative to the branch of that node). Similarly, the two senses of external functional similarity amount to applying total and external functional similarity, respectively, within a superbranch of that node instead of within the whole hierarchy.³¹

Intuitively, functional similarity refers to the form of the branch-complement of a node—external functional similarity, or similarity of functional role. Functionally equivalent things are things which can be plugged in at a node and make no external difference. If the similarity is "higher level" or "less detailed" or "more abstract", the similarities may be in the form of a superbranch-complement of the node. Each superbranch has a sub-branch which with it covers the whole hierarchy, so a higher level functional abstraction is ignoring functional detail in a larger number of branches or functional subsystems of the hierarchy.

Multiple realizeability in this context is the recognition that there may be functional equivalents. And intuitively, functional equivalents may realize their functions differently, as long as they interface with the higher-level functional systems in the same way. So their internal functional organizations may differ. But functional equivalence is almost always approximate, or only along limited dimensions in the real world. So-called "open architecture" computer systems may have functional equivalents at a variety of levels-or fail to be quite "open" [or have components which are not quite functionally equivalent] because of failures that may occur at a variety of levels-exploring the depths of sub-branch complements of a node. Computer magazines commonly test new operating systems by running a variety of applications written to run with the older operating system, putting each through its paces to determine whether it breaks down, and the conditions under which it does so. What results is a variety of coevolutionary "patches" or fixes-published alike by applications hardware and software manufacturers, and by the computer manufacturer in later "bug fixes" for that release of the operating system. And there are some bugs-transients-which never recur reproduceably enough to find, or to analyze. Happily, most of these are also infrequent enough that they can be ignored.

The point of all of this is to note that although we act as if there are frequent cases of functional equivalence, and that when it happens the external functional structure places no constraints on the internal functional structure (so that we can mix and match, and are constrained only at the interface), this is true—even approximately—only in simple cases. A branch is a functional sub-system, and the operation of separating a system into a branch and its complement is naturally decomposing a system along functional lines at one particular functional joint. But remember that in complex systems, functional organization rarely corresponds to the organization of physical components, and parts of physical components tend to interact relatively strongly with one another. So, the more complex the functional organization, the more interactions tie internal and external functional structure together. (See BECHTEL and RI-CHARDSON 1992, on complex localization.) Functional equivalence exists for parts in complex systems too, but when it is found it is usually a product of detailed fine-tuning and co-evolution. Everyone trying to configure a new computer system must wrestle with functional equivalence,

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near-equivalence, or non-equivalence at a variety of levels. The development of "compatibility", "transportability", and "transparency" as applied to computer software and hardware are some of the finest engineering examples of this available today—and a closer look shows just how difficult this ease is to generate.

20. Other and Weaker Forms of Functional Similarity

In general, biologists doing comparative studies of organisms or anthropologists comparing societies would hardly ever have any chances to apply concepts of functional similarity stronger than the various relative senses of internal and external functional similarity, and even then, these concepts will usually still be too strong in several respects: (1) In virtually all real cases, judgments of functional similarity take place in situations where the various functional hierarchies and branches have not been completely elaborated. (2) Even if they were, such judgments often seem to ignore the structure of whole portions of the tree in ways which do not seem to correspond to the distinctions made above. In what ways can one afford to ignore portions of the tree in making these judgments, and is there any systematic way of describing the "pruning" of nodes and branches implicit in making these comparisons? There appear to be at least three patterns which emerge. These might be called "path similarity", "level similarity", and "opportunistic pruning."

(1) "Path similarity": I suspect that most cases of purported functional similarity in biology (where based upon functional analogies, and not directly upon physical similarity) involve nothing more than isomorphism of the *paths* (or parts of the paths) from the node in question to the topmost node in the hierarchy or from a lower node to that node, or both. Such criteria are often, in effect, strengthened by rough judgments or presuppositions that several or many of the paths in the corresponding hierarchies are isomorphic. They are weakened however by "opportunistic pruning" (see below) to make the paths isomorphic.

(2) "Level similarity": One could also require isomorphism (before or after opportunistic pruning) of the hierarchies only at, above, or below a certain *level* of the hierarchy, where level could be defined in terms of level in the hierarchy or in terms of levels above or below the node in question.³² This would presuppose, however, that the tree is relatively well elaborated in order that the levels of various nodes could be defined.

There are however other interesting criteria for assigning levels in a hierarchy which do not stem directly from its formal organization, and which seem frequently to correspond to biological practice. [I have discussed the notion of a level of organization much more fully and satisfactorily, involving relative frequency of interaction as well as size scale, in my 1994.]³³ The first of these criteria associates different levels with the various distance scales characteristically associated with a set of phenomena. Thus, successive levels might be indicated by a sodium ion, inorganic molecules, various small protein molecules (such as insulin), myoglobin and hemoglobin molecules, sub-cellular parts, such as ribosomes, GOLGI apparatus, and mitochondria, and so on up through cells, tissues, organs, system of organs, organisms, populations, and the like.

The different levels are characterized by changes of scale of an order of magnitude or more, though not all parts and effects at a given level involve the same distances, and the "levels" deal with continuously and overlappingly distributed objects and effects. Thus cell sizes may vary over several orders of magnitude. These inconsistencies arise partly because biological parts are often classified according to type on functional criteria, with no intrinsic reference to physical size. Frequently too, they are classified according to physical inclusion in physical systems. Thus, all cells are regarded as being at the same level, as are all sub-cellular parts, such as ribosomes and mitochondria. The first presumably reflects the influence of functional criteria and the second those of physical inclusion.

This manner of talk about levels is also most appropriate for and prejudicial to talking about functional systems as physical objects, since sizes are more readily associated with objects than with behaviors. (Of course one can associate sizes with behaviors by considering their "ranges of effect", as when we talk about "close interaction forces" or "contact adhesion" or, less obviously, heat- and stretch- sensitivity or phototaxis.) This mode of classification by levels is also useful at relatively early stages in a functional investigation because it involves comparatively little by way of theoretical commitments, and one cannot have theoretical commitments before one has theories!

It might seem that a methodologically more sophisticated method of assigning levels to phenomena could be arrived at by classifying these phenomena according to the level of theory most

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appropriate to their description. Thus, for example, sodium ions are probably most appropriately studied at classical chemical and the lower biochemical levels, hemoglobin and DNA molecules at levels involving more complex interactions, and tissues at the more classical anatomical, histological, and physiological levels. For reasons elaborated in my 1994, I think that this approach will not work: There is here also interpenetration of levels, and it is not always possible to speak of the various theories (where one can even say what the theories are) as being in well-defined relations in terms of level. A rough criterion in the back of most writers' minds would be that if one theory is *reducible* to another, then the theory which is reduced is at a higher level than the reducing theory. But no one has yet even attempted a formal reduction of one theory to another in this realm and there is still a wide diversity of opinion as to what is to be meant by reduction. In effect, talk about levels of organization in terms of this criterion often is accompanied by an actual use of criteria of relative size, functional similarity, or physical inclusion.

While this seldom results in a classification by levels, phenomena can also be classified by a combination of theoretical, observational, and operational criteria according to the limitations of a technique. Thus, for example, starch or gel electrophoresis classifies proteins according to their "electrophoretic mobility", where this is, to a first approximation, a function only of their size and net charge. To be "visible" in an electron microscope, (of the transmission type), an object must be within a certain size range, of no greater than a certain thickness (which is a function of the material involved), and exhibit a certain minimum stability when bombarded by electrons of the energy and intensity used, and when treated with the various heavy metals used in "shadowing" electron microscope specimens.

(3) "Opportunistic pruning": In spite of all of the degrees of freedom inherent in these various forms of functional analogy, I suspect that, by intention or in effect, many judgments of functional analogy cannot be analyzed in terms of any single *a priori* systematic rule for pruning nodes or branches or for considering just various sub-portions of the hierarchy. In many cases, functional hierarchies or parts of them are *not* carefully compared on a point-forpoint basis, but there is just a "feeling" or "gestalt" that two functional hierarchies "look the same" in some part or parts of their structure—even if a closer inspection were to show that none of the senses of functional similarity defined so far seem to apply.

There is no direct way to get a handle on such "gestalt" judgments, but it can be surmised that the *effect* of such judgments is just the same as if two functional hierarchies were selectively pruned until they or some portions of them were isomorphic. But clearly, *any* two functional hierarchies, if "cut back" enough, would exhibit isomorphisms on this criterion. Thus, there must be some informal and perhaps unconscious criteria—probably tied closely to "informed intuitions" concerning the specific comparisons at hand—which are used in determining what portions of the hierarchies can be ignored or pruned.

Judgments as to level (both theory and size-relative) probably enter, as do judgments of the relative functional importance of various branches and nodes. (This latter idea should be extremely important in judgments of functional similarity, and will be given separate treatment in the next section.) Ultimately, however, some of these judgments must be laid simply at the doorstep of the intuitions which come with expertise in a given field. Such intuitions are to some extent merely the bag into which all unanalyzed features are put, but HELMER and RESCHER (1958) have made at least a start in the analysis of the role of "judgments of expertise" in science.

21. Quantitative Criteria for Functional Similarity

It would be misleading to talk about functional similarity just in terms of isomorphisms of structure in functional hierarchies, the conditions of these isomorphisms, and the similarity of purposes associated with these hierarchies. Things are not only functional, non-functional, and disfunctional, but they clearly can also be more or less functional and more or less disfunctional.

The structure of the functional hierarchy is determined by the interrelations between behaviors regarded as functional, but some are so central to the operation of a system that their absence or malfunctioning would, under almost any conditions, prevent purpose-attainment by that system. (Any loss of these functions is an unconditional lethal.) Other behaviors might have so little importance that their performance makes no difference at all under most conditions and only a small contribution in the remainder. Is the normal operation of the heart to be given the same status as the normal growth of a fingernail in analyzing functional hierarchies and functional similarities?

I assume that we can make comparative judgements of this sort, and even sometimes talk about the relative magnitudes of functional contributions.

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(Thus there are some cases in which selection coefficients might stand in for "degree of functional contribution", but many other times-far more commonly in fact where such a simple standard is not to be had.)³⁴ Some of the problems in defining such a measure, or making the relevant functional comparisons are considered in my 1972. Suppose we had solved these problems. (The uses I make of such comparisons here aren't very demanding.) By definition the functional hierarchy includes only functional behaviors and systems of behavior, so suppose that each node in the hierarchy is assigned a value of this index between 0 and 1. Assuming that they are reasonably assigned, consider two corrsponding nodes in three distinct, but otherwise similar hierarchies. In the first, they have values of .9999 and .0001. In the second, they both have values of .5000, and in the third, values of .0001. (FISHER considered selection coefficients down to this value to be significant.) But could corresponding nodes with such divergent values represent very similar components? Not likely! And whether they do or not, these three systems are obviously quite dissimilar in their functional architectures, in spite of their formal isomorphism on structural grounds.

So far, these remarks just suggest a further constraint operating within the framework of structural isomorphisms in the hierarchies to give a finer classification of degrees of similarity among parts which are already structurally isomorphic. But this is mistaken. Quantitative relations between values should clearly in some cases *overwhelm* judgments of structural isomorphism in deciding to which of two parts a third is more similar. And this is just a static comparison: large differences in selection intensities acting on a trait can have a variety of effects on other linked and unlinked traits, and is capable of entirely changing the course of evolution for that system. (See WADE 1996).

Nor in making such comparisons should we merely be concerned to delete nodes. After all, where we could delete a node of .001in one hierarchy to make a comparison, we could just as well add a node of .000 in the other. Unless there are clear historical or functional markers indicating a preferred direction, we should consider transformations symmetrically. Such pruning or adding of nodes may seem to be *ad hoc*, but in reality should be the fruitful move. The defense of categorical structural isomorphisms is often far more *ad hoc* for real measurements on real systems:

(1) We may have data of different quality or degree of detail or of accuracy for the two systems being compared—producing artifactual differences. Small functional contributions or small differences in functional contributions may go unnoticed, or may be impossible to detect in one system, whereas the greater experimental tractability or background data on another may yield apparently greater functional elaboration.

(2) We should try different resolutions or resolving powers on the functional systems being compared, to see whether focussing or defocussing yields greater similarities, and if so, in what particular functional systems or subsystems.

(3) Indeed, selection looks at functional hierarchies also. Nodes with small effect are nodes which are more easily lost due to drift or not protected in evolutionary changes from interactions with other systems which destroy their effects. So for a variety of reasons, defocussing—not looking with too much detail and precision may be advantageous in studying functional hierarchies. This is not always true however in particular, focussing on details can sometimes lead to significant reassessments of function. If from a "God's eye" view, we knew that the functional effects of an adaptation were very small, we might well be able to ignore it, but we don't have that kind of knowledge, and in any case, today's nearly irrelevant trait may be tomorrow's pivotal exaptation.

While quantitative similarity could be characterized more precisely, I don't see a compelling reason to try. Precise distinctions would suffer from the same arbitrariness as does calling any Chi-square level greater than 95% "significant" and all others not. Applying names and making distinctions in this manner may be a useful guide in establishing a common procedure (much as agreeing upon units in which measurements are to be expressed), but does not cut any relevant conceptual ice here. Similar remarks would apply to quantitative criteria for when to prune nodes in considering functional organization or isomorphisms. "Should a node be pruned when its index is one order of magnitude less than that of the node in the same system with the next smallest index-or two orders of magnitude, or one-half?" That depends upon the purposes of the investigator and other features of the situation, and should not be decided by fiat.

Part III: Heuristics, Adaptations, and Intrinsic Fallibility—Functional Failures and Functional Localization Fallacies

22. Adaptations and Heuristics

It is probably more correct, given the multiplicity of adaptive constraints which the effects of any new mutation must satisfy, to treat adaptations as differ-

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entiating—over evolutionary time—out of interactions between a largely but variably structured whole with its environment than treating adaptations as if they were assembled out of a tinker toy set of modular components. Of course, in a way, both descriptions have to be true—each on its appropriate size and time scale, but we don't assemble functional hierarchies on the same scale as we assemble nucleotide tinkertoys. Nonetheless the abstractions and reifications we deal with force us to treat whole functional systems as if they were constructed out of functional tinker-toy like components which we put together in various ways, as if we were articulating procedures in a structured computer program.

I want for the time being to treat adaptations as if they were unitary pieces of machinery, like computer code which we assemble into larger constructions to accomplish complex tasks which have to meet a variety of constraints. I do this because I want to emphasize the deep similarities between adaptations and heuristic procedures of the sort workers in artificial intelligence and students of problem-solving have chosen to study. Our complex plans can be construed as articulated structures of heuristics which we actually do construct in this way, and as such they are important examples of functional organization. But these heuristics share many fundamental features with biological adaptations. It is useful to think of these parallels when considering how we build conceptual models of functional organization.

Most fundamentally, problem-solving heuristics and adaptations each inevitably have conditions under which they fail, and studying them can give us clues both about how we should articulate them to work reliably, and ways of figuring out when and how they will break down. Articulated heuristics are not only instances and models of functional organization, but we also use heuristics to figure out how functionally organized systems work. Here the structure of error becomes particularly relevant, for we can debug our own inferences involving function. I close by laying out a number of specific problem-solving heuristics used in the analysis of complex systems, and describing their biases, and then look particularly at fallacies associated with functional localization, which is the cutting edge in the analysis of functionally organized systems. Thus the functional structure of our tools becomes both topic and tool in our analysis and construction of functional hierarchies.

The last three decades has seen a rapid growth of interest in tools of discovery and problem-solving techniques, beginning with the pioneering work of Herbert SIMON on "satisficing" and heuristics (see his

1969/1996 for an accessible overview). These heuristic procedures, unlike truth-preserving algorithms, do not guarantee correct results, even when correctly applied. But they are used because they generally or frequently produce correct answers with far less effort or computational demands, and are thus cost-effective solutions (WIMSATT 1980b). This is particularly (but not exclusively) the case for dealing with ill-structured problems, for which there may be no algorithmic solutions.

WIMSATT (1980b, 1985) provides a list of general features of heuristic procedures, of which the following is a slight elaboration. (See also LENAT 1982 for a penetrating discussion). While they are expressed in propositional mode, talk of solutions can also be applied to goal-directive actions or adaptive (or maladaptive) behaviors. The most important properties of heuristic procedures are as follows:

23. Properties of Heuristics

There are at least five properties of heuristic procedures of the sort used for problem-solving. These are sufficiently central to list as fundamental characteristics. There may well be others, but these are all I will consider here:

(1) By comparison with truth-preserving algorithms or with other procedures for which they might be substituted, heuristics make *no guarantees* (or weaker or more conditional guarantees) that they will produce a solution or the correct solution to a problem. A truth-preserving algorithm correctly applied to true premises *must* produce a correct conclusion. But one may correctly apply a heuristic procedure to correct input information without getting a correct output.

(2) By comparison with the procedures for which they may be substituted, heuristics are very "*cost-ef-fective*" in terms of demands on memory, computation, or other limited resources. (This of course is why they are used.)

(3) The errors produced by using a heuristic are not random, but *systematically biased*. This implies three things: (a) The heuristic will tend to break down in certain classes of cases and not in others, but not at random. (b) With an understanding of how the heuristic works, it should be possible to *predict* the conditions under which it will fail. (c) Where it is sensible to speak of a *direction of error*, heuristics will tend to cause errors in a certain direction, which is again a function of the heuristic and of the kinds of problems to which it is applied. These systematic biases can be useful in two ways: (1) Their analysis provides a "cal-

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ibration" of the heuristic—an evaluation of the conditions under which it can be safely used. (2) If different heuristics leave characteristic "footprints" (heuristic-specific biases), detecting systematic biases can provide clues as to the heuristic reasoning processes which produced them. TVERSKY and KAHNEMAN (1974) used this procedure in their classic study of biases in probabilistic reasoning. I discuss and apply it further in WIMSATT (1980b, 1986a).

(4) Application of a heuristic to a problem *trans-forms* the problem into a non-equivalent but intuitively related problem. Answers to the transformed problem may not be answers to the original problem, though if the reformulation leads to an adaptive solution, various cognitive biases operative in learning and science may lead us to ignore this. A solution where none was before possible may be taken as evidence of the correct formulation of the problem. Even if different, we may say that "this is what they were looking for all along".

(5) Heuristics are *useful for* something—they are *purpose relative*. Tools which are very useful for one purpose may be very bad for another. This can give a useful way of identifying or predicting biases: one would expect a tool to be relatively unbiased for the applications it was designed for, and perhaps quite biased for others: increases in performance in one area are commonly accompanied by decreases elsewhere.

The five properties of heuristics listed here provide a direct link between cognitive tools and the analysis of adaptations, forging an immediate connection between the study of heuristics and a general model of evolution of the sort advocated by CAMPBELL, HULL and others. This link is direct in two ways, because both biological and cognitive adaptations have all of the above properties, and can thus be regarded as special kinds of heuristics. CAMPBELL (1974) describes a special class of biological, psychological, and cultural adaptations which he calls "vicarious selectors", which are crucial to his account of evolutionary epistemology. I have argued (1980b, 1981a) that vicarious selectors have all of the properties of heuristics. Biological adaptations in general have these properties, given below with corresponding numbers.

24. Properties of Adaptations

(1') The proper performance and use of an adaptation, even in its normal environment, does not guarantee the survival of an organism or its production of viable offspring. (2') Adaptations are however cost-effective ways of contributing to that end, which it is assumed (on the "adaptationist program") is the reason for their selective incorporation and maintenance.

(3') Any adaptation can be made to malfunction under the appropriate circumstances, and the conditions under which an adaptation will fail are systematic. In fact the use of experimental conditions to cause malfunctions is one of the most powerful tools for discovering how a system functions, providing not only clues as to how the system is organized and works, but also an analysis of what conditions are required for it to function properly.

(4') This condition is easiest to demonstrate for sensorimotor functions, but a recognition of how it applies in these cases suggests how to generalize it. Consider the problem of how to detect seasonal changes in species whose morphology or behavior must change to allow survival or proper functioning in the changed environment. As LEVINS (1968) observes, this is characteristically done by sensing, tracking, or responding to an indicator variable which is a reliable predictor of the oncoming change. It may be that temperature change or food availability may be the survival-relevant parameter which necessitates the change in an organism, but it is far easier to detect changes in day length. This change is only contingently correlated with the adaptively relevant variables, a correlation which may break down under unusual circumstances, either in nature or as deliberately produced in the laboratory, but its use as an indicator to generate the appropriate changes transforms and enormously simplifies the problem of "deciding" when to make the appropriate changes.

(5') Adaptations are clearly adaptations to or adaptations for something—ultimately for maximizing fitness and its heritability, but more specifically for detailed tasks which are determined by the role of that adaptation in the functional organization of the organism. These are what we describe as its functions. Adaptations can acquire other tasks through evolutionary time (they are then called "exaptations", GOULD and VRBA 1982), but in doing so, they are commonly differently elaborated and pruned under the new selection pressures, indicating that an adaptation designed for one purpose is not generally good (and must be modified for) other purposes.

An addition to (5) is of deep significance for understanding the structure of functional hierarchies, and that is that new (functional) analogies are al-

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ways elaborated against a background history of homologies. If they are deeply generatively entrenched (RIEDL 1978, WIMSATT 1986), adaptations may persist as vestigial traits even if they have lost their original function, and adaptations will tend to preserve an "audit trail" of prior functions. There are a variety of heuristics of adaptive design and evolution for generative structures—structures which show differential degrees of burden or generative entrenchment. I will leave their fuller discussion for another occasion.

However, the analysis of any system proceeds in the context of many heuristics specific to reductionistic problem-solving. I here present a list which is much elaborated from the original (WIM-SATT 1980b). They are broken down by broad methodological category. Each heuristic procedure has cases or situations for which it works particularly well-usually a type of system which has a particularly simple or idealized architecture. All or almost all real systems deviate from these ideal conditions, so when they are applied to real systems, these problem-solving procedures tend to produce skewed or biased results. Nonetheless, they are often useful-sometimes as a rough approximation, sometimes as a poor approximation, but a useful stage to a better one.

25. Reductionistic Heuristics and Their Biases

In the processes of reductionistic analysis, the standard procedure is to analyze a complex system into its parts, analyze the behavior of these parts in isolation, and then resynthesize these parts and the explanations of their behavior into a composite explanation of the behavior of the whole system. [This practice of decomposition and recomposition constitutes a "near-decomposability" metaheuristic for reductionistic problem-solving.] In doing so, a number of heuristic strategies are employed which carry with them systematic biases which lead to ignoring or downplaying contextsensitivity of results and the importance of the environment. These are numbered in order of discovery-a numbering preserved to correspond to past references. Numbers 1-9 first appear and are used in WIMSATT 1980b. The first three categories were designed originally to follow the cycle of conceptualization, construction, and testing followed by modelers. The functional localization fallacies occur more commonly in the experimental study of systems, but some could also arise in modeling contexts.

A. Biases of Conceptualization: (1) *descriptive localization*: describe a relational property as if it were monadic, or a lower order relational property. Thus, e.g., describe fitness as if it were a property of phenotypes or genes, ignoring the fact that it is a relation between organism and environment. (justified/facilitated by fixing the environment, making it artificially disappear as a variable—see numbers 5, 8.)

(2) *meaning reductionism*: assume that lower-level redescriptions change the meanings of terms, but higher-level descriptions do not. result: philosophers (who view themselves as concerned with meaning relations) are inclined to a reductionistic bias.

(3) interface determinism: Assume that all that counts in analyzing the nature and behavior of a system is what comes or goes across the system-environment interface. This has two complementary versions: (a) black-box behaviorism-all that matters about a system is how it responds to given inputs; and (b) *black-world perspectivalism*—all that matters about the environment is what comes in across system boundaries and how the environment responds to system outputs. (e.g., FODOR's "methodological solipsism"). Either can introduce reductionistic biases when conjoined with the assumption of "white box" analysis-that the order of study is from a system with its input-output relations to its subsystems with theirs, and so on. The analysis of functional properties, in particular, is rendered incoherent and impossible by these assumptions.

(4) *entificational anchoring*: Assume that all descriptions and processes are to be referred to a entities at a given level, which are particularly robust, salient, or whatever. This is the ontological equivalent of assuming that there is a single cause for a phenomenon, or single level at which causation can act. Thus the tendency to regard individual organisms as primary, and more important than entities at either higher or lower levels (or similarly for genes for some reductionist neo-DARWINIANS.) cf. *perceptual focus* (number 19 below) and *multi-level reductionistic modeling*.

B. Biases of Model-Building and Theory Construction: (5) *modeling localization*: look for an intrasystematic mechanism to explain a systemic property rather than an intersystemic one. *Corollary 5a*: *Structural* properties are regarded as more important than *functional* ones.

(6) *contextual simplification*: in reductionistic model building, simplify environment before simplifying system. Thus the environment may be treated as homogeneous or constant (in space or in time), regular in some other way, or random. This strategy often

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legislates higher-level systems out of existence, or leaves no way of describing systemic phenomena appropriately.

(7) *generalization*: When starting out to improve a simple model of the system in relation to its environment, focus on generalizing or elaborating the internal structure, but ignoring generalizations or elaborations of external structure. *Corollary 7a*: If a model doesn't work, it must be because of simplifications in description of internal structure, not because of simplified descriptions of external structure.

C. Biases of Observation and Experimental Design: (8) *focussed observation*: Reductionists will tend not to monitor environmental variables, and thus will often tend not to record data necessary to detect interactional or larger scale patterns.

(9) *environmental control:* Reductionists will tend to keep environmental variables constant, and will thus tend to miss dependencies of system variables on them. (*Ceteris paribus* is regarded as a qualifier on environmental variables.) Mill's methods applied with this heuristic will yield as a systematic bias apparent independence of system variables from environmental variables, though the right experiments won't have been done to establish this.

(10) *locality of testing* Test a theory only for local perturbations, or only under laboratory conditions, rather than testing it in natural environments, or doing appropriate robustness or sensitivity analyses to suggest what are important environmental variables or parameter ranges.

(11) *abstractive reification*: Observe or model only those things that are common to all cases; don't record individuating circumstances. Losses: (1) sense of natural (or populational) variation; (2) lose detail necessary to explain variability in behavior, or exploitable in experimental design. The search for context-free theories often leads in this direction.

(12) Articulation-of-Parts (AP) coherence (KAUFF-MAN/TAYLOR/SCHANK): Assuming that studies done with parts studied under different (and often inconsistent) conditions are *context-independent*, and thus

behavior will result in selection of systems which may be uncharacteristically stable because they are relatively insensitive to environmental variations (SCHANK: regular 4-day cyclers among SPRAGUE-DAW-LEY rats are insensitive to conspecific pheromones; WIMSATT: MENDEL's selection of 7 out of 22 characters which are relatively constant and insensitive to the environment probably resulted in unconscious selection against epistatic traits.)

D. Functional Localization Fallacies: These fallacies correspond to common procedures for the inference or localization of functions. Each of them works fine in sufficiently simple cases, but each of them is employed more broadly, sometimes with comical results. Probably many errors produced by use of these heuristics still persist undetected.

(14) Assuming that the function of a part is to produce whatever the system fails to do when that part is absent, (e.g., Spark plugs as "sputter suppressors"), or produced when that part is activated or stimulated. More generally, the error involves reifying added or subtracted behaviors of the system as functional properties of the manipulated unit. GREGORY (1962) notes that the things obviously not done with lesion or deletion experiments may simply be the most obviously affected (rather than the most important). The part could have more importance to functions which are strongly canalized or whose defecits are not revealed under the testing conditions. More generally, even if a part does realize a function, it does so usually only against a background of activities by other interacting components. Judgements of modularity are often insufficiently justified.

(15) Assuming *simple 1-1 mappings between* recognizable *parts* and *functions*. This can lead to problems in two ways: (1) ignoring pleiotropy: stopping search for (more) functions of a part when you find one [e.g., the newly discovered region of hemoglobin implicated in NO⁺ transport, because it was assumed that *the* function of hemoglobin was oxygen transport]; (2) ignored division of labor (when a part's necessity is shown through deletion studies, etc.)

still valid when put together to give an explanation of the behavior of the whole. (SCHANK: Checking this gives a non-trivial use for computer simulation.)

(13) *behavioral regularity* (SCHANK/WIMSATT): The search for systems with relatively regular and controllable Author's address

William C. Wimsatt, Department of Philosophy, Committees on Evolutionary Biology and Conceptual Foundations of Science, 1050 E. 59th St., University of Chicago, Chicago, IL, 60637. Email: w-wimsatt@uchicago.edu [missing other parts' roles in the hypothesized function because they are part of the constant context, so they are always there to provide it]. Given the frequency with which we today see talk of "discovery of the gene for X"—a tendency blamed on classical geneticists, it is worth noting that from the beginning, Thomas Hunt MOR-GAN and all of his group were more careful in describing the relation between genes and characters than many geneticists today:

"It is important to note that mutations in the first chromosome are not limited to any part of the body, nor do they affect more frequently a particular part. The same statement holds equally for all of the other chromosomes. In fact, since each factor may affect visibly several parts of the body at the same time, there are no grounds for expecting any special relation between a given chromosome and special regions of the body. It cannot too insistently be urged that when we say a character is the product of a particular factor we mean no more than that it is the most conspicuous effect of the factor." (MORGAN 1916, italics in original.)³⁵

(16) *Ignoring interventive effects* and damage due to experimental manipulation as a source of possible artifacts.

(17) *Mistaking* lower-level *functions for* higher-level *ends*, or misidentifying system which is benefitted. This is common in units of selection controversies—either of the apocalyptic variety as with DAWKINS (1976) who denies all units of selection at higher levels than the gene, or for eliminative reductionists, who want to deny the existence or significance of large domains of cognitive function. There are legitimate concerns of level in both disputes, but the extremists are almost certainly seriously wrong.

(18) Imposition of *incorrect* set of *functional categories* on the incorrect assumption that we know perfectly well what the system is doing and why. (Common in philosophy of psychology which ignores ecology and evolutionary biology.) **E. Other Important Biases:** (numbers 10, 11 and these can generate either reductionistic or holistic biases in different contexts.)

(19) Extra-perspectival blindness: Assuming that a system can be exhaustively described and explained from a given perspective because it has been very successfully and powerfully so described. (Not all problems of biology are problems of genetics, or of molecular biology, physiology, or anatomy (to cite other past excesses) and (as we can now see from a safe distance), not all problems of psychology are problems of behavior. Insist on (or slip into) choosing that perspective or level in describing phenomena from other perspectives at other levels. [Thus a perceptual focus on individual selection leads to descriptions of groups as "collections of individuals". Within the context of the units of selection controversy, this is a biased description because it assumes that they have no significant organizational properties as a group. A way to correct for this is to insist on building models at a variety of levels or from a variety of perspectives, using devices intrinsic to that perspective, and then cross-checking to compare results to see where the different approaches fail to fit, and assessing the resultant biases. This is what I have called *multi-level reductionistic modeling*. See WIMSATT 1980b.]

(20) *Tool-binding:* Becoming sufficiently bound to a specific (usually very powerful) tool that one chooses problems for it, rather than conversely ("The right job for the organism", rather than "The right organism for the job"!) This applies to theoretical models and skills as well as to material tools. This may be an efficient division of labor if mastery of the tool is very demanding—it is problematic only when it facilitates errors number 11 or 16.

Notes

- 1 WRIGHT (1976) misrepresents my analysis by not recognizing that it is fundamentally selectionist, and not seeing that the additional apparatus I provide becomes necessary when talking about the functional comparisons so widely used throughout biology, and discussed there and here.
- **2** On the analysis of homology, see JARDINE, (1967). The analysis presented here could be seen as developing a special kind of homology as he defines it—homology with respect to *functional* properties. Then the greater detail and specificity of this analysis is due to the special logical features of the concept of "function", rather than talking about arbitrary relations.
- **3** This third sense is misleadingly precise (though not obviously so), because evolutionary biology is racked with numerous theoretical and empirical disputes concerning the nature and efficacy of selection operating at various levels of organization from that of the gene to that of the ecosystem and species. These disputes are obviously reflected in arguments over the proper meaning of "fitness" in evolutionary theory, and less obviously, in the choice of more specific definitions for "function" in the third sense (see THODAY 1953, and LEWONTIN 1961 vs. DOBZHANSKY 1968). For more recent reviews, see my 1980b 1981b, BRANDON and BURIAN 1985, and LLOYD 1988.
- **4** It is generically related to my more general sense in the following way: I take "function" (in any sense which can

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properly be called "teleological") to be inextricably connected with the concept of "purpose", and my general schema for function statements (see below in text) involves explicit reference to that concept. However, I take the latter concept not to involve any connections with consciousness or intentional action but merely as implying that certain logical features are present which are central in the talk of purposes in the human case. Thus, in principle, the concept of "purpose" may apply as a theoretical construct in areas and in ways that do not involve the postulation of a conscious agent or any other mental or vitalistic properties. In this generalized sense of "purpose", the evolutionary process involves, and the third sense of "function" relates to, purposes-which are, roughly, given a theoretically determined choice of evolutionary units, that which the net effect of selection operating at various levels maximizes for these units (see also WIMSATT 1972).

- 5 Unlike others cited for the second sense, KAUFFMAN explicitly takes the causal role of the entity supposed to have a function as being relative to some "perspective" of the system—be it determined by theoretical or pragmatic considerations or both. My generalized view of the third sense may thus be considered as his version of the second sense with the constraint that the "perspective" be such that the function may be legitimately construed as promoting the attainment of some purpose, in my generalized sense of that term.
- This maximization is subject to genetic, population genetic, 6 and environmental constraints. The appropriate evolutionary unit might be a single organism or clone (as with asexually reproducing bacteria), a breeding population or ensemble of breeding populations of the same or of hybridizable species, an ecosystem, or even a genus or combination of higher taxa of species. The choice of an evolutionary unit and of a time with respect to which the probability of survival of this evolutionary unit is maximized are determined by isolation factors and the relative magnitudes and temporal patterns of interaction of the forces of selection operating at these and other levels. This choice of the P variable for "function" in biology is inspired by THODAY'S (1953) discussion and is related to a modification of his definition of "fitness." These modifications have been suggested in part and in various ways by the work of LEWONTIN and LEVINS. [Addendum: I would now describe this as a satisficing optimization rather than a maximization, and want to supplement and/or modify these comments in the light of subsequent work on units of selection. See e.g., my (1980, 1981b) and BRANDON and BURIAN (1985), or for the latest work on group selection, see WADE (1996)]. Note that WILLIAMS (1966) and DAWKINS (1976) take for granted [too quickly] that the appropriate time period is a single generation.
- 7 It seems reasonable to require on conceptual grounds that functional hierarchies contain only functional behaviors, so "ideal" functional hierarchies do not contain non-functional or disfunctional nodes. But conceptual requirements may conflict with requirements of pragmatic usefulness, simplicity, and theoretical desirability because functional desiderata may not be the only things that apply:

(1) As outlined above, the tree contains a huge number of nodes, but can be simplified by representing sets of different conditions by a single node rather than individuating a node for each set of values of the five variables. But it may then be desirable to include behaviors which under some environmental conditions, or in some systems are nonfunctional or disfunctional, rather than specifying exhaustively all combinations of conditions under which it is functional, if its presence under these conditions is a natural consequence of its presence under the conditions under which it is functional.

(2) Physical objects are a natural case for this lumping. Whenever for pragmatic reasons (see below) individual nodes are interpreted as physical objects, these physical objects may have nonfunctional and disfunctional consequences of their operation in the system (as well as the functional consequences responsible for their inclusion).

- 8 Even the topmost node (which might be defined as that node which has no arrows leaving it) is *connected*, directly or indirectly (via incoming arrows) to all other nodes in the hierarchy.
- **9** And indeed, symmetry arguments suggest that all of the nodes in a closed functional loop must be assigned the same level in that functional system. And it is hard to see how this could be the top level unless each and every one of the functional behaviors in the loop could qualify at a topmost end—at least as a part of that collectivity.
- **10** This may appear to beg the question of the relation between human action and biological evolution. However, if it should ever be the case that psychology and sociology are "reduced" to biology, this example merely needs to be reclassified as a case of type (2) or (3). I am assuming (by classifying this as a case of type (1))) that this reduction has not yet been accomplished.
- **11** Roughly, the gene is recessive for sickle-cell anemia and dominant for increased resistance to maleria—a functional trait which results in a certain frequency of the gene in tropical and sub-tropical regions but lower frequencies in the temperate zones where malaria-resistance is less important.
- **12** The first case cited in discussing changes in the *T*-variable also fits here. Human purposes are generally different from (and frequently at odds with) evolutionary ones, and the fact that the purposes are different is at least as important as the fact that the theories are held to apply to diverse realms in deciding that two distinct functional hierarchies are involved. It might even be held as a reason for saying that the theories apply to distinct realms of phenomena, though of course the assumption that there are two purposes involved is itself open to doubt if reduction of one level to another is envisioned.
- 13 Such a system would certainly belong in LEIBNIZ's "best of all possible worlds", for literally everything that happened in it would be functional. This is an absurd view to hold for real systems, but "postulates of universal functionalism" could be construed as saying this much. Zealous advocates of the adaptationist program may take it as a methodological working hypothesis for each trait that they consider even if they do not believe its literal and exhaustive truth. It should be emphasized that the perspective of evolutionary biology suggests that it will be false for just the kinds of reasons adduced in this section.
- 14 The implicit distinction suggested here (between systems which make "positive" functional contributions and those whose functional contributions are "preventative") presupposes that certain systems are either given or necessary, and that the "preventative" functional systems are secondary additions to moderate any disfunctional occurrences or interactions between the given systems. Such talk might be given a precise use in connection with discussions of the temporal order of evolution of functional systems and changes in these systems. [Note added 1997: Indeed RIEDL's (1978) concept of "burden" or my own concept of "generative entrenchment" (WIMSATT 1986) would seem to be the

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appropriate tool here.] Thus, "co-adaptation" at genetic, conspecific, and intra-specific (ecological) levels can probably be construed as referring to the evolution of "preventative" functional systems which moderate the bad interactional effects of existing and, to some extent, competing functional systems.

- **15** The frequency of such events is important in two ways: First, for natural selection to operate effectively in changing the form of an entity, the occurrences in question must have some minimum frequency whose value is determined by the parameters of the situation. Secondly, in evolutionary arguments, there is often at least an implicit appeal to considerations of efficiency. A complex system is not evolved if a simple system will do, and a higher frequency of disfunctional events often requires or justifies a more complex system.
- 16 This is a slippery distinction: To say that two alternative genes or genotypes are selectively neutral relative to each other is not to say that either is non-functional but rather that they are equally functional. On the other hand, one could say that the substitution of one gene for another was a non-functional transition if the two are selectively neutral. FITCH and MARGOLIASH have argued that many mutations causing amino-acid substitutions in cytochrome-c molecules appear to be non-functional transitions. Some biologists feel that the amount of polymorphism in enzymes found by HUBBY and LEWONTIN (HUBBY and LEWONTIN 1966, LEWONTIN and HUBBY 1966) can be explained only if these polymorphisms are either selectively neutral or very nearly so. Even though there are now more recent treatments, the reasons behind this thinking-and why it has been difficult to resolve-are nicely reviewed in LEWONTIN 1974.
- **17** Added 1997: As BUSS (1987) nicely illustrates, this problem is compounded when one considers multiple possible levels and units of selection, and the possibility of intra-organismal competition among cell-lineages.
- **18** When a functional part is removed from a system and the system fails as a result to perform some task, it cannot generally be inferred that the function of the part is to accomplish that task. But this mode of inference has been widely used in attempts to analyze brain function via ablation experiments and behavioral function via deprivation experiments. R. L. GREGORY (1962) has criticized ablation studies, and Konrad LORENZ (1965) has analyzed the shortcomings of a simplistic use of deprivation experiments. As GREGORY suggests, to suppose that the function of a part of the brain is to accomplish some thing which is not done when that part is removed is as absurd as supposing that the function of the spark plugs in an automobile engine is to prevent the engine from sputtering because the engine sputters more severely as more spark plugs are removed.
- **19** Of course functional systems (in biology, at any rate) are systems of physical objects, but that does not imply that the best way of describing the *functional* organization of these systems makes use of the usual criteria used in cutting a physical system up into physically recognizable (spatio-temporally compact) parts.
- **20** Surely one of the temptations for vitalistic and anti-reductionistic thinking in biology and psychology results from the failure of functional systems to correspond to spatially compact and well delineated physical systems. From the fact that functional organization does not correspond to the most readily observable physical organization—the organization of physical *objects*—it is tempting (and fallacious) to infer that functional organization is not physical. So LASHLEY's failures to localize specific functions in specific areas of the cerebral cortex led to the rise of Gestalt

psychology, a holistic theory that artificially cut ties between psychology and physiology for many years. FODOR (1965, 1968) and PUTNAM (1967) have advanced equally fallacious arguments that functional and physical descriptions and analyses are intrinsically incommensurable. The key to their argument is the fact that the functional equivalence or isomorphism of two systems does not entail the equivalence or similarity of the physical *objects* in those systems. But there is more in the physical world than objects!

- 21 This will not happen if the system just happens to be so organized that in all environments and at all levels of analysis, there is a one-one correspondence between functions and physical objects. The probability of finding this in natural systems is highly unlikely, but it is more closely approximated in artifacts. It is common (but unsophisticated) design procedure to separate the functions of the proposed mechanism and to design individual parts or sets of parts to perform each function. This is an accident of our way of analyzing problems into a number of sequential steps or operations. Minimization techniques applied to problems in computer design have resulted in substantial increases in economy and reliability with given components by combining the functions of components. [See exposition of the COWAN-WINOGRAD theorem in ARBIB 1963.] Efficiency and reliability are presumably both selected for in evolutionary processes. It is thus not surprising that organisms do not exhibit a one-one correspondence between functions and physical parts or systems of objects. [Note added 1997: When this was written, programming and design time were cheap, and components expensive, so it was important to get the biggest computational bang out of the smallest number of components, and functional multiplexing of components seemed the way to go for increased efficiency in computer design. The invention of the integrated circuit, economies of scale in production, and Moore's law: the rough exponential doubling of computational power, speed (and number of components on chips) every 2 years has made it easier to use standard CPU chips for a wide variety of tasks for which they are profoundly overqualified, and the excess power and memory capacity has made has made program speed and efficiency relatively unimportant in most applications. The net effect is that, at the chip level, there may be in some contexts an increase in 1-1 mappings between functions and components, though the components are now polyfunctional, and identical chips are programmed differently to perform different tasks. So within different instances of a given chip type, the same component may be wildly polyfunctional. Note that similar things can happen with polyploidy and gene duplication.]
- 22 In what follows I will sometimes talk as if the functional hierarchy has a tree-structure, even though it does not. Most of the definitions and discussions below are not affected by what structure the hierarchy has. Of those affected by the presence of closed functional loops, most are modifiable in relatively simple ways. I proceed in terms of trees for pedagogical simplicity. Precise definitions which allow for closed functional loops are given in appendix I.
- **23** A fifth class of conditions is naturally suggested by the function statement schema—viz. conditions on the isomorphism of theories used in constructing the functional hierarchies. Thus, in the characterization of cultural evolution, ideas have been compared to conceptual viruses, largely on the assumption that both can be treated as kinds of human parasites, whose horizontal spread can be wellmodeled by the appropriate epidemiological equations.

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[Note added 1997: Indeed, given the elaboration of theories of cultural evolution, this may become a richer source of functional analogies (particularly at a relatively molar level, singe most sets of formal equations ar relatively non-specific] than many of the others.]

- 24 Experimental "fluid logic" computers were actually considered and simple partial prototypes built in the late 1950's by the U.S. military which reasoned that they would be more resistant to radiation effects which could "fry" electronic computers in missile nose cones. The IBM 709 and 7090 had a similar kind of (in this case, working) relationship: they were built according to the same circuit logic diagrams, but in one case realized with tubes and the other case with transistors. The greater volume and heat production of the first necessitated different space, power, and air conditioning requirements, the greater reliability and lower cost of the transistors presumably produced different maintenance schedules and purchasers, and the faster 7090 could accomplish some tasks that the 709 couldn't if online prediction was required. Each of these meant that the whole functional architecture of the computer installations could not have been identical, however similar their logic circuits were.
- **25** This set of conditions is not entirely independent of the first set. Isomporphism under substitution would seem to imply that the purpose(s) associated with the hierarchy(ies) in which the substitution is preformed are identical before and after the substitution. Thus, the full range of these conditions would seem to apply only for isomorphism under comparison. it is interesting to ask if in addition, one assumes that the hierarchy is efficiently organized for its ends whether any further conditions on similarity of purposes follows.
- **26** Clearly, mere extensional equivalence of the purpose-attainment states will not do. I doubt that extensional equivalence would even guarantee that the purposes are even intuitively similar.
- **27** Presumably, criteria for identity and similarity of two purpose-types would also be unpacked in terms of logical relations between their sets of criteria, but here, the criteria would contain no indexical references.
- **28** Several things of formal interest are worth noting:
 - (i) (S.1) implies (S.2) which implies (S.3) and similarly for (S.4), (S.5) and (S.6). None of the converses hold however. Also, (S.1) implies (S.4), (S.2) implies (S.5) and (S.3) implies (S.6), but not conversely, as suggested in section 14. (In general, for all of the senses below, the "substitutional" condition implies but is not implied by the corresponding "comparative" condition.)

(ii) As the strongest condition, (S.1) certainly deserves to be christened "structural functional *identity*", and if the quantitative indices (see section 21) also remain equal before and after the substitution of parts, should be named "functional identity." This last sense amounts to the satisfaction of LEIBNIZ's law of the identity of indiscernible for all *functional* properties. It is interesting to consider whether functional identity entails that LEIBNIZ's law is met for all causal properties of the intersubstitutable objects.

29 "Exactly similar" parts made of the same materials on a production line presumably approximate to the ideal of absolute substitutional isomorphism, though minor variations from piece to piece would almost certainly defeat this claim if it were applied with absolute rigor.

Various weakened versions of this constraint (e.g., "exactly similar" machine parts made of different materials, such as steel and brass, or the "corresponding" parts of the electronic and hydraulic computers) might meet some of the weaker conditions on this list, though this depends in part on what is taken as the functional hierarchy. (A brass part will fail under mechanical and temperature loads that a steel part will sustain, and the two metals have different electrical conductivities. Similarly, a hydraulic computer is slower than the corresponding electronic computer, but might be used in the absence of an electrical power supply. If the hierarchy is taken for a relatively restricted range of environments, these differences might not show up.)

- **30** The remarks about the use of sets of nodes to talk about functional similarities of physical parts in a given functional environment or set of functionally equivalent environments, or of behaviors or physical parts over a range of functionally different environments also apply here, of course. For simplicity's sake, however, I will just talk about the conditions of relative internal and external functional similarity for individual nodes.
- **31** We could extend these iterations further, resulting in a complete description and classification of all possible kinds of isomorphisms in parts of trees, but this seems excessive.
- **32** Parallel moves to those made for absolute and relative internal and external functional similarity could also be made in this case by defining super- and sub-levels and super- and sub-level complements, if desired.
- **33** I first characterized this notion of level in my 1976. For more complexities of biological organization, see also the notion of a perspective, defined in my 1974, and elaborated in 1994. These last papers are reprinted in my 1998.
- **34** The obvious move in defining such a measure is to talk in terms of the probability of purpose-attainment, and to define the degree of functionality as the probability of purpose-attainment given that the behavior or object occurs or is present minus that probability in its absence. This generates a range of values between +1 and -1 for this index, and makes the definitions suggested above for functional, non-functional, and disfunctional behaviors plausible.

There are many problems to be worked out in defining such a measure however:

(1) The probability of purpose-attainment in the absence of a functional object or trait cannot usually be evaluated simply by removing that object or trait from the system. This is because: (a) that object may be connected via non- and disfunctional interactions to other parts of the system and its removal will in general affect other parts of the hierarchy indirectly, and (b) the mere act of performing the physical extirpation of the functional item will usually perturb the system in diverse ways which will also prejudice the result. This is a corollary to the criticisms made by GRE-GORY and LORENTZ of ablation and deprivation experiments. (See note 18 above.)

(2) Leaving practical problems of evaluation aside, it might be thought that the degree of functionality might at least be *defined* in counter-factual terms as the difference between the actual probability of purpose-attainment (with the entity present) and the imagined probability of purpose-attainment in a *thought* experiment where just the entity in question is affected. However, it is a notorious feature of counter-factuals that there are an arbitrarily large number of states of affairs corresponding to a counter-factual of a given statement and also an arbitrarily large number of counter-factuals corresponding to a given statement. In effect, the thought experiment must be filled out in great detail or ambiguities result, and counter-factual statements do not specify these details. What is needed is a specification of the values of the mathematical function, "probabil-

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ity of purpose attainment", over the *n*- dimensional space of state descriptions of systems determined by *n* state variables, but there are a number of possible ways to do this, not all of which are equally desirable.

(3) Other constraints are placed on this measure if it is to have certain desirable properties as applied to the functional hierarchy. Thus, it is desirable (1) that the maximum value of this index be always found at the topmost node of the hierarchy under certain variations of the values of this index at all nodes in the hierarchy. (2) It would also be convenient to have it as an axiom that the degree of functionality of a node is equal to the sum of degrees of functionality of the nodes immediately below it and one level down—those nodes corresponding to its "next smallest" subsystems.

(4) The key to both problems in (3) above is not to be

solved by introducing axioms but rather by finding interpretive definitions according to which, it is true to say for real systems, e.g., that the degree of functionality of a system is equal to the sum of degrees of functionality of its parts.

35 Given MORGAN's early care when compared with modern geneticists in avoiding simplistic functional localization talk, there is some irony in the fact that his claims denying any spatial correspondence in mapping from chromosome location to expression in the body—claims that two decades ago would have seemed most secure—have fallen before the marvelous correspondences being discovered with the HOX gene complexes. MORGAN's warnings about the many-1 mappings in both directions between genes and characters have survived unchallenged however.

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Brain and Language: Beyond the Left Cerebral Hemisphere

Contributions of Subcortical Structures, Cerebellum, and Right Hemisphere to Language

the 19th century BROCA (1861) and WERNICKE (1874) were the first to describe severe speech disturbances after lesions of the left side of the brain. Since then, scientific interest for the neural substrate of language processing has almost exclusively focused on the left cerebral hemisphere which is considered the languagedominant in most people (overviews can be found in DAMASIO/GESCHWIND 1984, CAPLAN 1987, KOLB/ WHISHAW 1990). Compared to the crucial role of the left hemisphere for the generation and comprehension of spoken and written forms of language, possible contributions of other parts of the brain to language functions seemed of inferior interest

Several circumstances

may have further supported the widely held belief that only the left cerebral hemisphere would contribute to language functions. First, the cerebral cortex is much more easily to investigate than subcortical structures. It lies on the surface of the brain and is divided into anatomically defined gyri and sulci whereas the subcortical structures are lo-

Abstract

The crucial role of the left cerebral hemisphere for language has been known for a long time. Accordingly, the left hemisphere has attracted most of the attention regarding the neural substrate of language processing. In contrast to this, the present article focuses onto some of the brain regions and their possible contributions to language which are usually less regarded in this context. Subcortical structures, namely the basal ganglia and the thalamus, have traditionally been viewed as subserving purely motor and sensory functions. However, cortico-subcortical circuits seem to be of major importance for several aspects of language processing which are discussed in this article. It is further argued that the cerebellum, in addition to its motor functions, may contribute to cognitive processes, including language. In this relation, some evolutionary aspects concerning the human cerebellum and brainstem are discussed. Finally, it is emphasized that also the right hemisphere makes important contributions to language, particularly with respect to the emotional content of our verbal messages and certain forms of nonverbal communication.

Key words

Language, basal ganglia, thalamus, cerebellum, right hemisphere.

cated deep within the brain and their organization is much less obvious. Neuronal density in subcortical structures is much higher than in the cerebral cortex, thus minimal differences in the site of a subcortical lesion may result in very different neurological symptoms which makes it even more difficult to understand their topographical organization.

As a result of its superficial position, the cortex produces impressions on the inside of the scull thus allowing the investigation of cortical evolution in fossile records. The enormous growth of the cerebral cortex has consequently attracted most of the attention concerning the evolution of the human brain. On the other hand, virtually nothing is known about structural or

functional changes of subcortical structures in the course of hominid evolution.

Finally, the language functions of the left hemisphere are much more obvious and can be described more accurately than those of the right hemisphere, which makes them more easily accessible to systematic investigation.

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Figure 1: Coronal section showing the basal ganglia and the thalamus in relation to surrounding structures. (From KANDEL/ SCHWARTZ, Fig 40-1, 1985; p525)

In the following, I will try to draw the reader's attention onto some of the less regarded aspects of the relation between brain and human communication. First, I will discuss possible contributions of subcortical structures to language processing. Then, I will describe some evidence indicating a possible role of the cerebellum for cognitive functions, including language. Finally, I will try to illustrate contributions of the right hemisphere for language and, in this context, discuss some aspects of the neurological basis of nonverbal communication.

Subcortical Structures and Language

The basal ganglia

Anatomical and functional considerations. The basal ganglia are subcortical nuclei (accumulations of neuronal bodies) lying deep within the brain (Fig. 1). The following nuclei are considered as belonging to the basal ganglia: *nucleus caudatus, putamen,* and *globus pallidus* which are part of the telencephalon (endbrain), *nucleus subthalamicus* belonging to the diencephalon (between-brain) and the *substantia ni*-

gra which lies in the mesencephalon (midbrain). Nucleus caudatus and putamen are summarized as *corpus striatum* and putamen and globus pallidus as *nucleus lentiformis*.

The striatum receives input from the whole neocortex. This input is highly patterned in the sense that cortical areas which are reciprocically interconnected project in part to common regions of the striatum. The striatum predominantly projects to the globus pallidus and has reciprocal connections to the substantia nigra. Globus pallidus and substantia nigra project to the thalamus from where fibres leave to back the cortex (Fig. 2). In sum, these connections are building a reentry circuit which seems to modulate the cortical activity level. This might explain the fact that lesions of the striatum are associated with ipsilateral cortical hypometabolism.

The basal ganglia have usually been regarded primarily as an important part of the motor system. It is well known that disturbances of its function can lead to severe movement disorders such as the parkinson syndrome. Only recently, it was recognized that the basal ganglia also contribute to higher cognitive functions, including language.

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Figure 2: General organization of the cortical-subcortical circuits.

Basal ganglia and language. As mentioned above, BROCA (1861) was the first to describe severe speech disturbances after a lesion of the left hemisphere. Actually, this lesion was situated in a small area of the frontal cortex which BROCA consequently designated as the cerebral center of language. BROCA'S description stood at the beginning of the now huge amount of knowledge about language functions of the left cerebral hemisphere. Interestingly, the brain originally described by Broca was examined with a CT-scanner over 100 years later. To their great surprise the investigators saw that the lesion, which seemed to be confined to a small cortical area when looking from outside, was actually expanding deep into the brain and thereby involving important regions of the basal ganglia (SIGNORET et al. 1984). But this is only a particularly striking example among the expanding literature about subcortical contributions to language.

Several studies reported on patients exhibiting severe aphasia after subcortical lesions (ALEXANDER/ LOVERME 1980, BRUNNER et al. 1982, DAMASIO et al. 1982, NAESER et al. 1982). Moreover, the pattern of subcortical aphasia is distinct from the forms of aphasia seen after cortical lesions (DAMASIO 1983). The analysis of the relation between the exact site of a lesion and the profile of aphasia proved to be very difficult since even small variations in lesion site may lead to considerable differences in clinical presentation. Besides, the exact pattern of symptoms may also depend on the etiology of the lesion (ischemic stroke, intracerebral hemorrhage, tumor) which makes the comparison of different patients even more difficult. Just recently, the "core syndrom" of aphasia after subcortical lesions in the dominant hemisphere was described (MEGA/ALEXANDER 1994). According to MEGA/ ALEXANDER, this core syndrome of subcortical aphasia is characterized by deficient sentence generation with increased latencies, disturbances of naming, perseverations, and occasional bizarre content despite a generally grammatical and fluent conversational or responsive language output and preserved language comprehension.

Another indication for the role of the basal ganglia in language generation comes from phonological studies of the so-called voice onset time (VOT). This means the time which passes from the begin of a phonation to the burst which accompanies a stopconsonant (k, p, t). Usually, the VOT is shorter for soft consonants (i.e., g, b, d) than for hard consonants (i.e., k, p, t). Patients with a motor aphasia, but also parkinson patients, show a pathologic overlap of the VOT of soft and hard consonants. Moreover, the extent of this overlap correlates with deficits in syntax formation, language comprehension and concomitant cognitive deficits (LIEBERMAN et al. 1992). LIEBERMAN et al. viewed these findings as indications for subcortical circuits connecting the prefrontal cortex with BROCA's region.

Using positron emission tomography (PET; a brain imaging technique which allows regional measurement of blood flow and metabolism) MET-TER et al. (1988) found direct evidence of direct as well as indirect (through the frontal lobe) contributions of the basal ganglia to language functions.

Models about the role of the basal ganglia for language functions. Several models have been proposed to explain the influence of subcortical lelanguage production sions on and comprehension. According to these models, subcortical lesions may exert direct and indirect (through the cortex) influence on language generation (CROSSON 1985). The basal ganglia might influence the tonic activity¹ of cortical areas by regulating the flow of excitatory impulses from the ventral thalamus to the cortex. If tonic activity is maintained at too a high level, extraneous material will enter language-formulation. If tone is too low, language formulation will be inefficient or not occur at all. According to CROSSON, the basal ganglia might also be part of a motor release system which allows language segments to be released at a proper time, after semantic monitoring has taken place. In this context, CROSSON (1985) and WALLESCH/PAPAGNO (1988) argue for a circuit cortex-striatum-pallidum-ventral thalamus-cortex, where deep structures have the role of modulating cortical activity. Furthermore, a number of

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parallel acting, but functionally separate systems might constitute a fronto-striatal system.

The basal ganglia are connected to another large subcortical brain structure, the thalamus. Being clearly separated from the thalamus by anatomical and functional means, they are described separately. However, the possible roles of the basal ganglia and of the thalamus for cortical functioning are closely tied together, as is their implementation in respective models of cortico-subcortical circuits. For this reason, the basal ganglia will be encountered again when discussing integrative models of subcortical language functions in the next chapter.

The thalamus

Anatomical and functional aspects. The thalamus is a large accumulation of several nuclei and belongs to the diencephalon (see Fig. 1). Its most prominent function is that of a relais to the cerebral cortex: Virtually all input to the cortex (with a few exceptions) is relayed through the thalamus. Some of the thalamic nuclei receive afferent projections from our sense organs and send this information to the respective sensory projection areas of the cortex (this is true for all our senses expect smelling), whereas other nuclei of the thalamus receive projections from certain parts of the brain such as the cerebellum or the mamillary bodies and relay it to specific regions of the cerebral cortex. For this reason, the thalamus was characterized as the cortex's window to the world at every level (MUMFORD 1995). Moreover, many of these connections between the thalamus and the cerebral cortex are organized in a bidirectional way, such that the respective cortical areas (e.g., the motor cortex, premotor cortex and the temporo-parietal cortex) themselves project back to the thalamus, thus forming a massive system of local loops between the thalamus and the entire cortex. In addition to these so-called *specific* nuclei which are reciprocally connected to the respective cortical areas in an ordered topological pattern, there are also nonspecific nuclei which project diffusely, often to the entire cortex (MUMFORD 1995). Additionally to these, the reticular thalamic nucleus and the perigeniculate nucleus form a thin layer around the thalamus through which all thalamocortical and cortico-thalamic fibers must pass, and which send inhibitory projections back to the thalamus.

In contrast to the initially held belief that the thalamus is simply relaying sensory information

to the cortex, it is now believed that it exhibits significant influence on sensory processing, particularly under consideration of the massive feedback pathways from every cortical area to its thalamic input nucleus, which sometimes outweigh the thalamo-cortical connections by one order of magnitude (SHERMAN/KOCH 1986). According to MUMFORD, these cortico-thalamic (top-down) feedback connections might serve to enhance the bottom-up sensory signals which convey data from the external world through subcortical pathways, to reconstruct missing data, or to externalize for further processing views of the world created purely by mental imagery. It should be pointed out that not only the primary sensory areas are connected to the thalamus by such massive feedback-circuits but also secondary and tertiary sensory areas as well as association areas of the cortex. Therefore, it is not surprising that the influence of the thalamus on cortical functioning is not restricted to pure sensory processing but extends on higher cognitive functions as well.

Thalamus and language—Theories and Models. The theories about the role of the thalamus for language functions can be categorized in several groups (CROSSON 1984):

Descriptive theories point out similarities and differences of the language disturbances after (dominant) thalamic lesions compared to cortical lesions (JONAS 1982). In fact, a distinct type of thalamic aphasia has been defined, which is characterized by moderate to severe expressive aphasia with paraphasias (i.e., the construction of words containing wrong letters or syllables, e.g., *winker* instead of *winter* or the construction of sentences containing incorrect words), perseverations and disturbances of naming, in contrast to relatively well preserved language comprehension and repetition.

Emphasizing the numerous connections between the thalamus and the cortex, *unspecific theories* suppose that thalamic lesions disturb information processing in a rather diffuse and unlateralized way (LEVITA et al. 1967, BROWN 1975). These theories are challenged by the fact that language disturbances are observed only after lesions of the dominant thalamus and by the existence of well preserved language functions in thalamic aphasia (see above).

Some authors have pointed out the possible role of the thalamus for the level of cortical activation and argued that more complex language functions might require increased cortical activation (RIKLAN/ COOPER 1975, HORENSTEIN et al. 1978, MCFARLING et

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al. 1982). Indeed, the thalamus is connected to the reticular formation of the brainstem, and therefore thalamic lesions may lead to disturbances of cortical activation or vigilance. But neither these *activation theories* can explain the phenomenon of selective deficits encountered in thalamic aphasia.

Other authors have speculated that the thalamus might play a role in the integration of language (integration theories): cortico-thalamo-cortical circuits under control of the brainstem might be responsible for the final matching of verbal messages and for the formation of new motor patterns (BOETZ/BARBEAU 1971). It was also hypothesized that the thalamus might serve as a gating mechanism for storage and retrieval of verbal memory (REYNOLDS et al. 1979), or that it controls the release or inhibition of preformed speech patterns and the temporal ordering of speech (SCHALTENBRAND 1975). Emphasizing the disproportionate disturbance of semantic aspects of language in thalamic aphasia, CAPPA/VIGNOLO (1979) assumed that the dominant thalamus is not involved in phonemic aspects of language. Following this distinction CROSSON (1981; cit. from CROSSON 1984) hypothesized that the dominant thalamus might be part of a semantic preverbal feedack-loop between anterior areas for language formulation and posterior areas for semantic decoding, thus serving as a monitoring mechanism for potential language output.

Taking into account various aspects of different theories about the role of the thalamus in language, CROSSON (1985, 1987) formulated a model focussing on subsequent stages of language production. According to this model language is formulated in anterior regions and subsequently monitored for semantic accuracy in posterior cortical regions. The next stage comprises motor programming in anterior regions followed by phonological monitoring in the posterior cortical areas. In this model the role of the thalamus is considered under three aspects (from CROSSON 1987): (Fig. 3)

1. Maintaining Cortical Tone: The activity of the ventral anterior thalamus which receives input from the reticular formation of the brainstem (via intralaminar nuclei) transmits excitatory impulses to the anterior language areas thus maintaining optimal tone of the language formulation areas. The amount of excitation is regulated by the inhibitory influence of the globus pallidus which itself stands under inhibitory control of the striatum. Through its connections with the striatum the anterior language cortex could finally regulate its own activation.



Figure 3: Possible contributions of the thalamus to language according to CROSSON (1985, 1987). (A) Neural circuit for semantic and phonological monitoring. (B) Maintenance of cortical tone and response release mechanisms in a model of stepwise inhibition and disinhibition.

2. Semantic Monitoring: Considering the large amount of paraphasias in thalamic aphasia despite relatively preserved fluency, language comprehension, and repetition, it is likely that the thalamus is also involved in a semantic monitoring mechanism in a feedback-loop between anterior and posterior language regions as described above. This semantic monitoring might be mediated by the pulvinar, a thalamic nucleus which has reciprocal connections to the temporo-parietal cortex, the ventral anterior nucleus, and probably also to the posterior frontal cortex. According to CROSSON, phonological monitoring might subsequently be accomplished via the arcuate fascicle, a fiber pathway connecting the temporo-parietal and frontal language areas.

3. Response Release Mechanisms: With respect to the hypothesized role of the basal ganglia for the release of motor programs already formulated in the cortex, the thalamus might serve as an intermediary "buffer" for these language segments until their semantic monitoring is finished. According to CROS-

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SON, this might be accomplished in a loop of stepwise inhibition starting in the temporo-parietal cortex which inhibits the striatum. The striatum in turn inhibits the globus pallidus which on its turn inhibits the nucleus ventralis anterior of the thalamus (cf. Fig. 3). After completion of semantic monitoring, the striatum would be released from further inhibition through the temporo-parietal cortex, thereby leading to increased inhibition of the globus pallidus. This, in turn, would be followed by release of the nucleus ventralis anterior, which now could send excitatory impulses to the anterior language regions thus leading to a release of the preformulated motor program. In the stepwise model of language production this language segment would now undergo phonological monitoring until its final release as a spoken language segment. To complete this model, one has to imagine many of such processes happening simultaneously in many parallel pathways.

Though appealing, the described model has to be reconsidered for neuropsychological as well as neuroanatomical reasons. First, some authors have argued that the model cannot account for a number of neuropsychological phenomena observed in subcortical aphasia, such as errors in naming or certain types of neologisms (ROBIN/SCHIENBERG 1990, KENNEDY/MURDOCH 1993). Furthermore, the model is not fully compatible with some neuroanatomical details, since, contrarily to the proposed inhibitory influence of cortical areas on the striatum, at least the vast majority of cortical input to the striatum is, actually, excitatory (HOUK 1995, WILSON 1995). Trying to implement more recent neuroanatomical evidence into the model of CROSSON, we will have to consider the anatomical connections of the basal ganglia in some more detail.

The reader might recollect the anatomy of the basal ganglia and their connections described above (see Fig. 2). In short, we are confronted with a loop system starting with cortico-striatal projections. The striatum sends its output to the globus pallidus and the substantia nigra from where fibers reach the thalamus, which, in turn, projects back to the respective cortical areas. Many of such loops are organized in parallel in a highly topographical way. At this point, we have to consider a neuroanatomical detail, which was not mentioned until now. Actually, the striatal neurons comprise two distinct populations: one projecting to the external segment of the globus pallidus (GPe), and the other either projecting to the internal segment of the globus pallidus (GPi) or to the substantia nigra pars reticulata



Figure 4: Cortex-basal ganglia-thalamus circuitry under consideration of the two distinct striatal projections to the GPi and GPe. The substantia nigra and its connections which parallel those of the GPi are not shown. A further simplification shows the thalamostriate projection arising from the nucleus ventralis anterior, rather than from the centromedian nucleus. Connections belonging to the *direct* and *indirect* pathways are labelled correspondingly. GPi: Globus pallidus internus. GPe: Globus pallidus externus.

(SNr) (ALEXANDER 1995) (Fig. 4). GPi and SNr constitute the output nuclei of the basal ganglia sending inhibitory projections to the ventrolateral and intralaminar thalamus. GPi and SNr do not only receive direct inputs from the striatum (direct pathway), but also from the so-termed indirect pathway, which takes its origin from the striatal neurons projecting to the GPe. The inhibitory output of the GPe projects mainly to the subthalamic nucleus (STN) which sends excitatory fibers to the GPi/SNr, thereby completing one arm of the indirect pathway (The STN also sends excitatory fibers back to the GPe and to the striatum.). The GPe also sends projections to GPi/SNr themselves, thus completing the second arm of the indirect pathway. In sum, activation of striatal neurons associated with either arm of the indirect pathway will tend to increase basal ganglia output—in one way by disinhibiting the STN which on its turn activates the GPi/SNr and, on the other hand, by directly disinhibiting GPi/ SNr. Contrarily, activation of the direct pathway will decrease basal ganglia output by direct inhibition of GPi/SNr. Considering the reentrant organization of the cortico-subcortical system (cortexbasal ganglia-thalamus-cortex), cortical activation of the direct pathway will lead to disinhibition of the excitatory thalamo-cortical connections,

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Figure 5: Anatomico-functional relations between cortical and subcortical structures and their possible contributions to language processing. Simplifications and labelling correspond to those in Figure 4. Note that the connection between anterior language areas and the pulvinar might also be accomplished via its connections to the nucleus ventralis anterior which are not shown. Thalamic nuclei appear shaded. GPi: Globus pallidus internus. GPe: Globus pallidus externus.

thereby reinforcing cortical activation. On the other hand, cortically initiated activation of the indirect pathway will tend to attenuate thalamo-cortical excitation by increasing the inhibitory outflow of the basal ganglia to the thalamus. In addition to the direct and indirect pathway, we have to mention an inhibitory projection from the GPe to the nucleus reticularis of the thalamus (NRT), which, on its turn, sends inhibitory projections to the basal gangliarecipient zones of the ventrolateral thalamus. Thus, all of the functional pathways comprising the GPe (both arms of the indirect pathway as well as its projections to the NRT) tend to reduce corticothalamo-cortical interactions.

Under consideration of these functionally different cortico-subcortical loops (direct vs. indirect pathway), we now may refine the above described model of subcortical contributions to language functions. Accordingly, the role of the basal ganglia in the regulation of cortical tone and in the release of preformed motor programs (after semantic monitoring) might be accomplished by differential activation of the direct and indirect pathways. There are, however, limits to our current understanding of the detailed role of these pathways in cortical-subcortical loops, in that it is not known whether a given cortico-striatal neuron engages both the direct and indirect pathway in a balanced manner. Nor is it evident whether the convergence of inputs from the direct and indirect pathways onto individual GPi/ SNr neurons results in a functional interaction of these two pathways (should it be antagonistic or complementary) (ALEXANDER 1995). Furthermore, it is not clear at this point how a differential activation of the direct and indirect pathways might be accomplished and in which succession they might be activated during language processing. Nevertheless, Figure 5 tries to summarize the above described anatomico-functional relations under particular consideration of their possible role in language functions.

The cerebellum

Traditionally, the human cerebellum has been regarded as being involved primarily in the execution of well-trained motor programs and in the maintenance of balance. Only recently, data of several studies indicate a role of the human cerebellum in cognitive and language functions. Using PET,

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PETERSEN et al. (1989) found, somewhat unexpectedly, that the right lateral cerebellum was activated when a verb generation task was compared with simple repetition of nouns. This suggested that the cerebellum makes some sort of non-motor contribution to language processing. In another PET study of depressive patients those with impaired cognitive function showed a specific increase of regional cerebral blood flow (rCBF) in the cerebellar vermis which was associated with a decrease of rCBF in the left medial prefrontal cortex (DOLAN et al. 1992). SIL-VERI et al. (1994) described a patient with a rightsided cerebellar infarct who developed dysarthria and agrammatic speech closely resembling the type of aphasia found after lesions of the anterior (Broca's) cortical language area. In detail, the patient showed a slight reduction in verbal fluency, but, more interestingly, omission of grammatic morphemes, especially auxiliaries and clitics, as well as substitution of the infinitive for inflected verb forms. At the same time single-photon emission computed tomography (SPECT) showed a markedly decreased perfusion of the left cerebral hemisphere. The phenomenon of the correlation of metabolism between one cerebral hemisphere and the contralateral cerebellar hemisphere is known as crossed cerebellar diaschisis. It may also be encountered after lesions of a cerebral hemisphere when the contralateral cerebellar hemisphere shows corresponding hypometabolism.

Besides this functional evidence of a close interrelation between the activity of the cerebral cortex and that of the cerebellum, the anatomical correlates of these interrelations are being extensively studied. In addition to projections from the motor-cortex to the cerebellum which have been known for a long time, more recent studies showed cerebro-cerebellar projections from other large areas of the cerebral cortex such as from the superior and inferior parietal lobules, superior temporal sulcus, supratemporal plane and superior temporal gyrus (COLE 1994). Together with other (clinical, anatomical and functional) studies pointing in the same direction, these data further corroborate the assumption that the human cerebellum is involved in cognitive functions, including language.

Interestingly, this newly emerging concept of cognitive functions of the cerebellum can also be regarded from an evolutionary viewpoint. It has often been overlooked that in the course of hominid evolution not only the association areas of the cerebral cortex increased dramatically in size, but that also the lateral parts of the cerebellum showed an enor-

mous enlargement (Dow 1988). These parts of the cerebellum send their output to the dentate nucleus of the cerebellum, which is of particular interest with respect to its evolutionary differentiation. The phylogenetic new parts of the dentate nucleus (neodentate) can be distinguished from its older parts (palaeodentate) based on morphological, histological, embryological, histochemical, and pathological evidence (Dow 1988). Clues to the function of the neodentate lie in its output connections to other parts of the brain, which have been difficult to study. They can only be investigated in humans because in the monkey the neodentate is not yet fully differentiated and in apes it is not yet fully enlarged. The dentate nucleus sends its output to widely distributed areas of the brain, namely to the brainstem, the thalamus and (via the thalamus) to the cerebral cortex. From these, the primary target of the neodentate is the frontal lobe which is known to have strikingly enlarged during hominid evolution concomitantly with cerebellar enlargement. The projections of the neodentate do not only reach motor areas of the frontal lobe but also prefrontal areas including Broca's language area in the inferior prefrontal cortex and area 8 in the superior prefrontal cortex. The latter area seems to be activated not only during motor performance (eye movements) but also when listening to verbal information. Broca's area seems to be involved not exclusively in motor aspects of language but also in processes of word-finding which are regarded as cognitive processes. In this way the cerebellum seems to participate in cognitive wordprocessing (LEINER et al. 1993).

From Broca's area (as well as from other areas of the cerebral cortex) projections reach the red nucleus in the brainstem which deserve our special interest (LEINER, et al. 1993). In most mammals the red nucleus sends its major output to the spinal cord thereby subserving motor functions. In humans this part has diminished but the red nucleus now sends its major output to the inferior olive in the brainstem which, in turn, is connected to the dentate nucleus of the cerebellum which, again, projects to the red nucleus (Fig. 6). Thus, in the human brainstem a neural loop has evolved involving language areas of the cerebral cortex as well as brainstem nuclei and parts of the cerebellum. LEINER et al. speculated that this newly evolved neural circuit might function as a language-learning loop. Under consideration of other large tracts containing about 20 million nerve fibers connecting the cerebral cortex and the cerebellum, they further proposed that the cerebellum could improve the performance of these parts of the

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Figure 6: Newly evolved connections between cerebellum, cerebral cortex, and red nucleus.

brain to which it is reciprocically connected. The specific functions of the cerebellum may differ with respect to the respective area of the brain to which it is connected in different vertebrate species: e.g., in electric fish it is connected to sensory parts of the brain, in mammals it is connected to limbic parts of the brain and in humans, specifically, it has connections to cognitive parts of the frontal cortex as described above. In the light of these data our traditional view of the cerebellum as serving purely motor functions seems to be unduly narrow. Instead, the role of the cerebellum may well extend on cognitive functions, including language and other processes, which are usually regarded as being restricted to the cerebral cortex.

The right hemisphere

Most commonly, it is assumed that language functions are confined to the left hemisphere (which is the so-called dominant hemisphere in most righthanded persons). Accordingly, though sometimes implicitly, I also referred to the left (language-dominant) side of the thalamus and the basal ganglia in the discussion of their possible roles for language. In this chapter of the article, I will present some evidence that also the right hemisphere makes important contributions to language.

Prosody

The most obvious role of human language is the transport of verbal information, which includes all its lexical, grammatical, and articulatory differentiations. These can be summarized as the propositional aspects of language. It is undisputed that the left hemisphere plays a major role in the neural processes governing the production and comprehension of propositional language. However, language can convey other information which goes beyond these propositional aspects. This information includes melody, pauses, timing, stress, and accent, as well as intonation, which are subsumed under the term *prosody* (This is true only for the non-tonal languages, i.e., English or German, where melody conveys affective information. Contrarily, in tonal languages, e.g., Mandarin Chinese, melody affects the semantic meaning of a word.). In contrast to the discretely organized propositional features of language, its prosodical aspects are commonly graduated and are, therefore, more difficult to analyze (CRYSTAL 1975). It has been demonstrated that during childhood the acquisition of prosodical aspects takes place before that of the propositional features of language, and that prosody serves as a fundamental building block for the development of language in children (LEWIS 1936; cit. from Ross 1993). Furthermore, prosody is a crucial parameter of communication since it exerts major influence on the sense of a verbal information. If verbal and prosodical content of a message do not fit together, it is usually prosody which determines the sense of the message (e.g., If the sentence "I really had a great day" is spoken in an ironic tone it is usually understood as a negation; cit. from Ross 1993).

The prosodical components of language can be classified into four categories (MONRAD-KROHN 1963; cit. from Ross 1993): (1) *Intrinsic prosody* subserves linguistic functions such as stressing specific words in a sentence to clarify the meaning or raising the intonation at the end of a statement to indicate a question. (2) *Intellectual prosody* enables the speaker to add attitudinal components to a statement (e.g., If the sentence "You are clever" is emphatically stressed on "are" instead of "clever", it may change its meaning from a compliment to slight sarcasm.). (3) *Emotional prosody* indicates the mood of the speaker (e.g., anger, sadness, happiness). (4) *Inarticulate prosody* is the use of paralinguistic elements, e.g., sighs, grunts etc.

The disorders of prosody may also be categorized with respect to their clinical presentation. The term

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aprosodia refers to the absence of prosodical elements of language which can be encountered e.g., in parkinson's disease. *Hyperprosody*, which is observed in manic patients, means the excessive use of prosody. *Dysprosody* is a change in dialectical and idiosyncratic voice quality which leads to the foreign accent syndrome and can be observed sometimes in patients who have recovered from motor aphasia after a left hemispheric (predominantly left-frontal) lesion.

Evidence that the right hemisphere is particularly important for comprehension and production of prosodical aspects of language stems from neuropsychological investigations as well as from neuroimaging studies in healthy persons. Clinical studies have shown that serious disturbances of prosody may be particularly encountered after right brain damage and that various combinations of deficits in production, comprehension, and repetition of the prosodical elements of language may occur (without disturbance of the propositional elements of language) (e.g., TUCKER et al. 1977, ROSS 1981, KENT/ROSENBECK 1982, BOWERS et al. 1987, GORELICK/ROSS 1987).

In an attempt to correlate the different forms of aprosodia with specific brain regions within the right hemisphere Ross (1981, 1993) and GORELICK/ Ross (1987) proposed that the disorders of prosody in right-hemispheric patients might mirror the respective forms of aphasia after left-hemispheric lesions. GORELICK/ROSS investigated different prosodical capacities of 14 patients with righthemispheric lesions. Studying spontaneous affective prosody and gesture during conversation, affective-prosodical repetition (patients had to repeat sentences with different emotional tone) and comprehension (patients had to identify the emotional tone in which a sentence was presented to them), and comprehension of emotional gesturing, they identified different forms of aprosodia in their patients. Furthermore, they were able to correlate specific forms of aprosodia to different brain regions. Patients with motor-aprosodia (i.e., a deficit in the generation of prosody) had lesions in the frontal and anterior parietal and temporal lobe, whereas sensory aprosodia (i.e., disturbed comprehension of prosody) was found in a patient with a temporoparietal lesion. In analogy to several other forms of aphasia after left-hemispheric lesions they identified further subgroups of aprosodia (condution aprosodia, transcortical sensory aprosodia, pure affective deafness, global aprosodia) and related these to the respective regions of the right hemisphere. However, their results have been questioned by

other authors (BRADVIK et al. 1991) and the details of their assumptions remain to be confirmed by others.

The above mentioned studies represented neuropsychological studies in patients with brain lesions. Very recently, the brain regions involved in the understanding of emotional prosody could directly be visualized in healthy subjects in a PET investigation (GEORGE et al. 1996). In this study, subjects listened to three similar sets of spoken sentences. In three different tasks, their responses were based on the emotional propositional content, the emotional intonation of a sentence (prosody), or on their ability to repeat the second word in the sentence (control). It turned out that understanding of the propositional context activated the prefrontal cortex bilaterally whereas responding to emotional prosody activated the right prefrontal cortex.

Several attempts have been made to further specify the contribution of the right hemisphere to prosodical aspects of language. Some authors have argued that the right hemisphere might be specialized in the processing of phonetic aspects which might be independent of emotional categorization, such as vowels and the tonal contour of sentences (WEINTRAUB et al. 1981, SHAPIRO/DANLY 1985). Accordingly, there are indications for right-hemisphere dominance in the discrimination of nonverbal tonal patterns, including low-level analysis of music (ZATORRE 1989). However, several studies found a greater impairment of emotional prosody than of non-emotional prosody after right-hemisphere damage (HUGHES et al. 1983, HEILMAN et al. 1984).

It was also supposed that the right hemispheric superiority in the comprehension of prosody might be a consequence of a selective attention of the right hemisphere to prosodical-semantic relations or, alternatively, resistance to distraction. This might be relevant in tasks when subjects have to evaluate the congruency of the semantic and prosodical contents of sentences. It could be difficult particularly for right hemispheric patients to modulate attention over experimentally dissociated semantic and prosodical task demands which are not usually dissociated in natural discourse (BOWERS, et al. 1987).

LALANDE et al. (1992) investigated prosodical and semantic comprehension in patients with left- or right-sided cerebral lesions. The patients had to perform three different tasks: (1) They had to judge the emotional meaning of sentences connoting one of

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six primary emotions (joy, sadness, fear, surprise, disgust, anger) spoken in a neutral tone (verbal contextual task); (2) The same sentences were presented, but voiced emotionally by humming with closed mouth (pure prosody task); (3a) Half of the sentences was spoken with concordant emotional tone and (3b) the other half was spoken with discordant emotional tone (emotional concordance task). Whereas patients with left-hemispheric damage were (tendentially) worse in the verbal contextual task, patients with right-hemispheric damage were significantly impaired in the pure prosody task and the emotional concordance task. The investigators supposed that the prosodical deficits of the right-hemispheric patients might be interpreted rather as phonetic deficits at the vowel or the sentence contour level than as emotional deficits. On the other hand, the impairment in the emotional concordance task might be a consequence of a distraction effect on impaired attentional resources.

However, there are observations which are not fully consistent with this view and which indicate that the right hemisphere might be generally specialized for the categorization of emotional stimuli. This view is supported by a number of studies showing the importance of the right hemisphere in the recognition of the emotional expression of faces (e.g., DE KOSKY et al. 1980, KOLB/TAYLOR 1981) and in the understanding of emotional metaphors, emotional gesture, and humor (ROSS/MESULAM 1979, BIHRLE et al. 1986, BRYAN 1988). Furthermore, several studies indicate that the right hemisphere might be important for the modulation of mood (see SILBERMAN/WEINGARTNER 1986; for a review).

An interesting point was raised when the effects of right-sided brain lesions were compared in speakers of non-tonal and tonal languages. In nontone languages, such as English or German, the most salient acoustic feature for conveying affect in speech appears to be intonation. In tonal languages (e.g., Mandarin), which comprise about half the world's languages, intonation is used for word meaning and is not, for the most part, modulated for the signalling of affect. Nevertheless, Mandarin speaking patients with right sided brain lesions exhibited forms of aprosodia comparable with those of English-speaking patients but showed distinct differences in their acoustic profiles compared to English-speaking patients (Ross 1993). Ross followed that it is rather the affective behavior itself than the ability to modulate a certain set of acoustic features which seems to be lateralized in the human brain. Furthermore, he argued that though the right hemisphere seems to modulate graded affective behaviors associated with language, it is not responsible for the organization of extreme emotional displays. This view is supported by the observation that patients with aprosodia exert seemingly flat affective behavior on the one hand but may show extreme emotional behavior in other situations. These patients may also continue to experience the entire range of emotional feeling states despite their lost ability to execute the respective behavior (except extreme emotional reactions). These observations indicate that the right hemisphere may be important for the control and modulation but not the entire organization of emotional behavior. Particularly, the critical areas for the motoric organization of extreme emotional behavior lie in the temporal limbic system and basal forebrain. Moreover, emotional experience and graded affective behaviors associated with language are dissociable, with each having a different neuroanatomic basis (Ross 1993). Thus, the specific role of the right hemisphere for generation and comprehension of prosody is still under debate. However, the importance of the right-hemispheric contribution to the prosodical aspects of language is uncontroversial.

Metaphors

Another interesting aspect of the role of the right hemisphere in language was highlighted by a recent study (BOTTINI et al. 1994). Neuropsychological investigations had previously shown impairments of the comprehension of metaphors in patients with right-hemispheric lesions (WINNER/GARDNER 1977), and it had been argued that the right hemisphere was specifically capable of bringing together multiple semantic associations (while the left hemisphere performs the more usual task of single denotative representation) (BEEMAN et al. 1994). Using PET, BOTTINI et al. mapped the anatomical structures involved in sentence and metaphor comprehension in subjects who had to perform three different tasks: (1) A metaphorical comprehension task in which subjects had to decide whether a sentence was a plausible metaphor or not (e.g., "The investigators were squirrels collecting nuts" or "The investigators were trams"); (2) A literal sentence- comprehension task in which subjects had to decide whether sentences were plausible or implausible at the literal level of analysis (e.g., "The boy used stones as paperweights" or "Tim used feathers as paperweights"), and (3) A lexical-decision

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Figure 7: A patient interpreting this picture-sequence as an African elephant-trap covered with leaves might have a right-hemispheric lesion. (From LANDIS et al., Fig. 6, 1990, p 438)

task where subjects had to indicate if a non-word was present in a sentence-like string of words. Analysis of the PET data revealed that comprehension of sentences compared with the lexical-decision task leads to extensive activation in several regions of the left hemisphere, including the prefrontal and basal frontal, as well as temporal and parietal regions. In the metaphor task, similar activations in the left hemisphere were observed, but additionally, several regions of the right hemisphere were activated, including the prefrontal cortex, the middle temporal gyrus, the precuneus, and the posterior cinguli. Trying to interpret these data, the authors argued that the prefrontal areas might play a role in the retrieval of episodic memory. Indeed, the majority of subjects reported on the use of personal experience when they had to evaluate the metaphors, which was not necessary, on the other hand, for the sentence comprehension and lexical decision task. Furthermore, lexical analysis of metaphors is ineffective, since it is just the violation of lexical-semantical rules which is characteristic for metaphors. Therefore, mental imagery, which has been shown to activate right frontal regions (ROLAND/FRIBERG 1985), might also be important as an alternative representational system in metaphorical comprehension. Concerning the activation of the right middle temporal gyrus, BOT-

TINI et al. speculated that this region might play a particular role in information processing related to complex tasks such as metaphor comprehension—thereby mirroring the role of the left middle temporal gyrus in complex semantic judgements. Finally, the right precuneus, which was also activated during metaphor comprehension, is suspected to play a role in long-term memory (SHALLICE et al. 1994). According to BOTTINI et al. metaphors may thus reflect conceptualizations of experiences in long-term memory which had been previously supposed by GLUCKSBERG/ KEYSAR (1990).

Pantomime, gesture

Despite its primarily verbal character spoken language is usually accompanied by non-verbal elements of communication which considerably influence the content of a spoken message. From these, the term pantomime refers to movements conveying specific semantic information whereas gestures are movements used to color and emphasize the spoken words. Few neurological studies referred to the disturbances of pantomime and gesture². Disruptions of pantomimal elements were seen in patients with aphasia after left-hemispheric lesions. They were either interpreted as a general impairment of the interpretation of symbols or as specific disturbances of movement control (apraxia) (GOODGLASS/KAPLAN 1963, GAINOTTI/LEMMO 1976, DE RENZI et al. 1980).

In contrast to pantomime, gestures are often preserved in patients with aphasia. Furthermore, Ross/

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Wolfgang Lalouschek, MD, University Clinic for Neurology, Währinger Gürtel 18-20, A-1097 Vienna, Austria Phone: +43-1-40400-3117 Fax: +43-1-40400-3141 MESULAM (1979) described a patient with a right frontal lesion and complete loss of any gestural activity in the nonparalyzed right face and limbs without any apraxia. This led to the assumption that the right hemisphere might be dominant for the

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control of gestural behavior. In the meantime, a number of studies corroborated this assumption by showing that the right hemisphere controls not only the production but also the comprehension of gestural behavior (DEKOSKY, et al. 1980, ROSS 1981, BENOWITZ et al. 1983, GORELICK/ROSS 1987).

Finally, it might be noted that the right hemisphere also contributes to our sense of humor. As a consequence, patients with right-hemispheric brain lesions not only show disorders of prosody and gestural behavior but also of the interpretation of comedy of situation and the appreciation of jokes (LANDIS et al. 1990). To illustrate this point, the reader may be referred to Figure 7.

Concluding Remarks

Language belongs to the most complex and fascinating cognitive functions. The crucial role of the left cerebral cortex for the generation and comprehension of most aspects of language is undisputed. However, the presented evidence may indicate that we have to update our traditional view of the basal ganglia, the thalamus, and the cerebellum as being involved purely in motor functions or in the relaying of sensory information to the cerebral cortex, respectively. Instead, these structures exert a considerable influence on cortical activity and seem to contribute to a number of cognitive functions, including language. We are also reminded not to focus exclusively on the cerebral cortex when thinking about the function and evolution of the hominid brain, but also to consider deeper lying and phylogenetically older structures (which though may exhibit some evolutionary plasticity). Finally, also the right hemisphere, traditionally regarded with respect to visuo-spatial functions, is of major importance for certain aspects of language, particularly with respect to its emotional content and accompanying gestural behavior. Also complex language-related processes, such as the interpretation of metaphors deserve the intact function of the right hemisphere.

The implementation of these concepts in comprehensive and dynamic theories of the neural basis of cognitive processing will constitute a major challenge in the future.

Notes

- 1 The term tonic activity or tone refers to the degree of excitation in a particular cortical area or the whole cortex
- 2 These visuo-spatial aspects of (spoken) language are not to

be intermingled with sign language which displays the complex linguistic structures of spoken languages. In analogy to disturbances of spoken language (aphasia) sign language may be severely disturbed after left-hemispheric lesions (POIZNER et al. 1984; DAMASIO et al. 1986).

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From Four Forces Back to Four Causes

I Introduction

It is now generally acknowledged that all processes in this universe may be traced down to a transmission of forces, viz., the four basic physiinteractions: cal the strong and weak interactions, electromagnetism, and gravitation. No other forces are to be expected, or seem to be necessary, for a sufficient explanation of whatever process that might occur in this world.

On the other hand, at the dawn of causal thinking in the history of science, ARISTOTLE differentiated four causes, again indispensable for sufficient explanation, and which of course had little or nothing to do with the four physical interactions.

About the background

The four causes account was a fashionable way of thinking before philosophers came to expect that one of the four causes had to be the original cause of the others, while at the same time providing a

Abstract

The paradigm of physics provides sufficient explanation of all phenomena in terms of only four forces: strong and weak interactions, electromagnetism, and gravitation. The necessity to deal with complexity in our time, however, has led to a renaissance of thinking in terms of four causes (power, material, formative and goal-seeking processes), an account with roots going back to ARISTOTLE.

This reorientation is not a nostalgic regression to archaic thinking, but a methodological necessity, for two reasons: (1) The growing number of phase transitions in the development of complex systems, particularly organisms and artefacts, makes it increasingly unsatisfactory to trace their forms and functions back to the four forces. (2) The four causes account seems to be the only complete account of explanation. Yet after ARISTOTLE, it gave way to two opposing paradigms, which both expressed the expectation of sufficient explanation by means of only one of the four causes; viz. power in the sciences, and aims in the humanities.

On one hand, a growing number of authors who deal with complexity are revisiting the four causes, with a variety of outcomes that partly contradict one another. On the other, we have no indication that the world may be divided into four causes (compare ARIS-TOTLE Phys. I2, 185a 12–14). Evolutionary epistemology (EE), however, rather has us expect constraints, or symmetries. Keeping this in mind may be helpful in trying to justify the four causes account, to overcome contradictions, and to use this account as a guide enabling us not to fall into the traps of fallacious simplification again.

Key words

Physical intractions, complex systems, ARISTOTLE, four causes, cognition, cognitive constraints, cognitive

sufficient explanation of the world. This was before philosophy split into two branches (empiricism versus rationalism), with the divide between the sciliving systems, on one side, and the epistemologies of PIAGET (1967) and LORENZ (1971), on the other, got the ball rolling.

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Yet the four causes ac-

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terest. Forerunners of this

scientific renaissance were

MORGAN'S 'emergentism'

(1923) and TEGGART'S the-

ory of history (1925). Both

scholars observed that

causal actions must come

from two sides. Only

much later did the con-

cepts of 'bottom up' and

come into use (e.g., CAMP-

BELL 1974). The develop-

ment of systems theory by

WEISS, which started in the

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A renaissance

First references to ARISTOTLE may stem from Marjorie GRENE (1967), POLANYI (1968), DELBRÜCK (1971), and MONTALENTI (1974), with each of them referring to only a single cause at once; first full discussions of the four causes are offered in PATTEN (1976) and RIEDL (1976, 1978/79). In the 1970s and early 1980s, several contributions that were mainly engaged in questions such as 'different level constraints' and 'focal level observations' came close to the topic. They are widely referred to by SALTHE (1985), who provides another full discussion of the four causes. I myself followed up the item in successive books (RIEDL 1980/84, 1985, 1987, 1992), but SALTHE and I did not know about one another's work.

More recently, the discussion has included a comparison of the four causes with the division between 'structuralism' and 'functionalism', as well as attempts to formalize the matter (LENNOX 1980, RIEPPEL 1990, CASTI 1989, ROSEN 1987, 1991a, 1991b, ALVAREZ DE LORENZANA 1997); again with a variety of outcomes.

Furthermore, this whole four-decade endeavor of reinvolving ARISTOLE'S analysis back into the scietific debate has to be viewed within the larger framework of rediscussing complexity (which has been going on for six decades); in being at least one of the possible attempts to cope with complexity. This field ist too wide to be referred here. Pertinent monographs are from NICOLIS and PRIGOGINE (1987), more recently from LEVIN (1992), MAINZER (1994), CORNWELL (1994) and GELL-MANN (1994). It is clear that this whole discussion is still crisis-ridden (HORGAN 1995).

A cognitive interpretation

The stuation described above encourages us to attempt a cognitive investigation of the phenomenon of the four causes, which will take the form of four clarifications: (II) What are the ARISTOTELIAN causes? (III) How can they be understood by cognitive symmetries? (IV) What urges us to shift from forces to causes?, and (V) How are these causes composed of forces?

II What are the Aristotelian causes

For the following interpretation and comments I use the translation of ARISTOTLE edited by ROSS (editions 1966 and commentary 1970) and my retranslation from the German edition by BONITZ and SEIDEL (1978 edition). The commonly used version is in italics. In ARISTOTLE'S text, today labeled 'Older Metaphysics' (Met. I3), he states: "Only then can we say that we comprehend something, if we think we know its primary causes. We talk about causes in multiple ways, however: first, we call substance a cause.; secondly, matter or substrate; thirdly, that which causes the origin of a motion; and fourthly, the contrary of the latter: aim or purpose" (These terms are not quite equivalent to what ARISTOTLE meant—a point I will return to). A later text makes this more explicit.

The specific standpoint

In the second chapter of his 'fifth book on Metaphysics', ARISTOTLE states: "Evidently we have to acquire knowledge of the original causes (for we say we know each thing only when we think we recognize its first cause), and causes are spoken of in four senses". 'First' and 'original' may, in addition, be taken as 'basic' and 'fundamental', by other authors as 'primary' or the 'proximate cause'. To summarize:: what has to be considered whenever we search for explanation?

(1) "Cause means that from which, as immanent material, a thing comes into being, e.g., bronze is the cause of a statue and silver the first cause of a saucer, and so are the classes which include these." Matter and substance, even substratum is meant here. This focuses on causes "insofar", ARISTOTLE says, "that they are the substrate (hypokeimenon) from which something can be made, for example the parts. the material of a product."

This is easy to understand as matter and material, as well as materialized components and compartments, be it marble, playing cards, spider threads, or what-have-you. House building requires material; bricks and lumber. A pre selective situation is envisioned, since material itself has an influence on what can be built from it. The commonly used Latin form is *'causa materialis'*.

(2) " The form or pattern, i.e. the definition of the essence, and the classes which include this . and the parts included in the definition." The German translation speaks of 'Form' and 'Musterbild', which also corresponds to 'model', 'design', and 'paragon'. 'Essence' may also be taken as 'gist' or 'pith'. Meant here are 'form-giving principles': that which gives matter form and shape; which makes bronze into a statue and silver ito a saucer. ARISTOTLE'S "tò ti en einai" comes close to 'substantial being' or 'essence', a notion which played a great role in the philosophy of his time. This interpretation imposes itself on us when we read his summary: "namely, the wholeness and the composition of the form."

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Compared to house building, messing without a blueprint—even with lots of material at hand and repeated attempts (and power)—hardly produces a house. The process most obviously needs a selective process, assortment or arrangement (because not every material is suited to form everything), and some management in the sense of information or instruction as to which material to put on which place in which position. Processes of decisions, assortment, and choice lead to such a product, in general: a builder's plan, selection from outside. The Latin term is '*causa formalis'*.

(3) "That from which the change or the resting from change first begins." The German translation is 'Anstoß', which is equivalent to 'inpulse', 'push', but also 'impetus'; "in the sense", he states, "that motion or rest originates from it." 'Impulse', 'drive', 'action', and 'propulsion' all come close to what ARISTOTLE meant. The best translation is 'power', in the sense of 'force', which in contemporary usage reaches from 'energy' and 'work' to 'labour' and even 'capital'. Compared to house building, material and a blueprint are still not enough to erect the buiding. Labor and capital are required. The Latin term is 'causa efficiens'.

The suggestive translation (see Ross 1966, 1970) as 'efficient cause' one encounters most is misleading in two respects. First, the term has nothing to do with performance, competence, or cleverness. Secondly, and even worse, it suggests that the other three causes are probably inefficient.

(4) "The end, i.e. that for the sake of which a thing is; e.g., health is the cause of walking. For 'Why does one walk?' we say; 'that one may be healthy'; and in speaking thus we think we have given the cause; for all these are for the sake of the end. . It is the cause", ARISTOTLE says, "for which something occurs or happens." 'Aim' or 'purpose' would be a reasonable translation, but only on the understanding that such consciously guided processes are subsumed under a wider category (intentionality is not required).

In essence, all successful programs—or programs of which we assume that they might be successful in the living world—are to be subsumed here. The German term 'Zweck' corresponds to ARISTOTLE'S notion. Thus the program in a chicken's egg is likely suited to produce a chicken. We will return to this in more detail. In general, goal-seeking, repetitive, and highly tested processes are meant. In our example of house building, we understand that even power, material and an architect's plan will not necessarily erect a building. The power may sit in the bank, the material on a storage site, and the plan in a drawer. In addition, something must occur to get the thing going, regardless of whether this happens erroneously or not. The Latin form we use is *'causa finalis'*.

Other commentaries

I find my interpretation in agreement with several authors, and I add two of their generally shorter versions. RIEPPEL (1990, p300) refers to ARISTOTLE'S 'techne' model and uses "the familiar simile of a carpenter constructing a cabinet. The wood that the carpenter uses to build the cabinet is the material cause (*causa materialis*); the force that he invests in his project through the work of his hands is the efficient cause (*causa efficient*); before the carpenter can start his work, he must have a plan or idea as to how the cabinet is going to look, and this plan is the formal cause (*causa formalis*); and the intended use of the cabinet is the final cause (*causa formalis*).

MAINZER (1997, p22), who starts from the three kinds of changes ARISTOTLE distinguishes, and focuses on the causes of change, agrees:

"Why does a plant grow? It grows (1) because its material components make growth possible *(causa materialis)*, (2) because physiological functions determine growth *(causa formalis)*, (3) because external circumstances (nutrients in the earth, water, sunlight, etc.) occasion growth *(causa efficient)*, (4) because, in accordance with its final purpose, it is meant to open up into the perfect form *(causa fina-lis)."*

With some other authors, the agreement is smaller, mainly because of their attempts to sort the four causes according to the distinction of structuralism and functionalism. The deviations have to do with uncertainties related to this distinction itself. To investigate this uncertainty would go beyond the scope of this contribution.

The ancient view

The smooth-looking interpretation given above notwithstanding, one should not disregard the remarkable changes that occurred in the world view in general, and the view of life in particular, since ARISTOTLE's time. The four causes are explained in analogy to human activities, although they have been taken to be applicable to all complex processes in sublunar nature.

This is part of his methodology. The aim of all sciences, in ARISTOTLE's sense, is to form an intelligible universe by discovering the universals in the particulars, which are the primary and only substances

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or existences, but which have no existence independently of the universal which is their 'form' and makes the class or kind of existence which they are. Starting from repeated sensations, we ascend to memory and eventually to experience, making a world we can understand, a world of knowledge. Such a program, and the justification of such an approach to nature, was not followed after ARISTOTLE's death. For centuries it remained submerged, but later on it found all kinds of renaissances.

Yet, after long philological discussions it is confirmed (e.g., KULLMANN, 1979) that in the case of the four causes ARISTOTLE's account can be translated confidently in modern scientific language.

III The cognitive symmetries

In contrast to the four forces, which have become palpable within quantum field theory, we have no indication that the world is assembled by four different causes. We must rather assume that this division is based on the outfit of our mind; and the following may support this view. Two symmetries are to be described. One was already seen by ARISTO-TLE. He felt, that *c.mat.* and *c.form.* are elements of the systems themselves, while *c.eff.* and *c.fin.* are acting from outside. I will add a second symmetry, in connexion with the perspective of the hierarchically layered structure of complex systems.

A first symmetry

The symmetry observed by ARISTOTLE is supported by two observations from modern time.

(1) *C.eff.* always comes from outside, and even from far outside the system, regardless of whether it is an inorganic system, an organism, or an artefact. The system of ocean currents is driven by temperature inputs, either from the sun directly, or by transmission through the atmosphere. All activities of living beings depend on nutrition. Even man's energy support, including the fuel which moves your cars, ultimately depend on photons that are reaching, or have reached, the earth. And the charge of a battery may come from a hydroelectric power station (depending on a river assembled by evaporation and rain), which is driven by gravity.

C.fin. is always acting from outside (typically from the environment of the system) as well. We no longer speak of *c.fin.* in inorganic systems, but with regard to organismic systems and artefacts we do. The goal which is to be reached by the program of a chicken egg is to maintain the species, in competition with other species; and the goal of you owning a car is to meet the requirements of a mobile society in competition with other mobile societies, and so on.

In contrast, the nouns describing the objects of *c.mat.* are always parts of the layers of the system itself. At most, an immediately neighboring material may be considered. Clay for a brick, the carrot for a hamster, ore for making a car, become remarkably altered by their contribution in building these three systems. What catches more the eye are the changes between the layers of the system itself; such as from bricks to facades and a castle, from cells to organs and a hamster, and from sheet metal to fenders and a car.

C.form. is also mainly a phenomenon that is internal to a system. Of course, a contemporary style influences the shape of a castle, and a species is altered by its environment. But the style of a castle influences much more the diversity of its towers, stairways, rooms and their functions, as the form of a species influences the shape of its organs, tissues, cells and their functions (compare fig. 1).

(2) It is also evident that *c.mat.* and *c.form.* differ from *c.eff.* and *c.fin.* by the semantic characters of the terms to be used in them. While *c.eff.* and *c.fin.* refer to functions, expressed in verbs or verbs used as nouns, as 'impuls', 'drive', 'impel', and 'action', or as 'aim', 'purpose' and 'selection'. In contrast, *c.mat.* and *c.form.* refer to structures expressed in nouns, such as cells, organs, and legs, as bricks, walls and towers. And this regardless whether we have, as in *c.mat.*, the material of a leg or a tower in mind, or, as in *c.form.*, the cause of their form and function.

One may recall that all human languages differentiate in nouns and verbs. EE traces this fact back to a universal outfit of the human capacity to compute data from the outer world (RIEDL 1987). The steps preparing gestalt perception result in typological concepts or connotations with defined boundaries, as between trees and forests, or bricks and walls. This leads to nouns. Our preparation to grasp processes, leading to verbs, is less well understood. Here time is to be considered much more pertinently than in Gestalt perception, whereas shape and form are to be abstrained or refrained from. E.g., 'running' depicted in a cartoon does not even show the legs they decompose in a swirl (a cloud of dust behind the runner).

This allows us to understand why naming the contents of *c.mat.* and *c.form.* requires different concepts for every layer of a complex system, separated by phase transitions. In contrast, *c.eff.* and *c.fin.*, un-

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Figure 1: *The two cognitive symmetries.* First symmetry observed by ARISTOTLE. Note, that ARISTOTLE did not jet consider a hierarchy of layers, but causes coming from outside versus such inside the systems. The second symmetry is observed by the author, deviding in top down and bottom up causation.

derstood as 'power' and 'goal seeking programs', remain conceptually unchanged, whatever the layer on which we focus.

The second symmetry

The second symmetry, not yet considered by ARIS-TOTLE, becomes obvious when we face the hierarchical structure of complex systems, series of sub- and superimposed units, roughly spoken of as lower and higher layers (RIEDL 1978/79, 1985). In contrast to the cognitive symmetries, this is taken as an ontological concept, a character of the outer world, and not as a mere way of speaking, or consequence of our mind, in the sense of accepting quanta, atoms, molecules, cells, tissues, organs, organisms, and societies as real entities.

Much has been written about the reality of this hierarchy since LEIBNIZ's time. This topic cannot be discussed here; my own position is spelled out in earlier contributions (RIEDL 1978/79, 1980, 1985); compare, e.g., WEISS (1971), SALTHE (1985). In the required shortness: boundaries of hierarchical layers are to be drawn, wherever phase transitions have to be postulated in the path of the history of a complex system. Now such phase transitions are preserved, and can be found, wherever the semantics between (chains of) disciplines is changing. Consider the transition of nouns standing for subject matters between, e.g., microphysics, chemistry, cytology, histology, comparative anatomy (organology), taxonomy, psychology, and sociology.

The layered view of organization must consider the insight that new layers do not emerge by simply putting a new layer on top of the others. Every new layer originates not only by assembling given materials, it also originates within a preexisting, superimposed environment. New layers, as all differentation in this world, emerge as insertions between layers. The subimposed systems provide the components or constituents, the superimposed one decide about the conditions for the maintenance of the newly assembled organization (RIEDL 1985). Let us compare (1) the empirical evidence with (2) the cognitive background.

(1) If one accepts the layers view it becomes obvious (compare fig. 2) that two of the causes, namely *c.eff.* and *c.mat.* are acting bottom up, the two others, *c.form.* and *c.fin.*, top down. This may be trivial for *c.mat.* (because all constituents of systems start from quanta, atoms, and molecules). But also the bottom up flow of *c.eff.* is not difficult to pursue for organisms and artefacts. Because even the power of an army is due to human activity, cell metabolism, chemical bounds, and finally the forces of quanta.

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Figure 2: *The age and the implementation of the layers* between the whole and the parts. Causa materialis and formalis are drawn for every connection, causa efficiens and finalis (not to overcharge the diagram) are set only at their starting and recent position. Note the different terms which denominate selective processes (after RIEDL 1985, with amendments).

(1a) The differentiation between bottom-up and top-down causation has been used sometimes in recent literature. It emerged with MORGAN (1923), was investigated in a broader context by WEISS (1969, 1971), and the terms were coined by CAMPBELL (1974); see also SALTHE (1985). It corresponds in part to a first step of our investigation.

(1b) More circumspection is required to grasp the universality of a downstream of causes. *C.form.*, as we recall, is a form-giving principle that shapes structures by selection. This is always caused by a super-imposed layer (as when a society selects the kind of its members, or an environment its species), but also as the functional requirements of a species influence the kinds and shapes of its organs, the organs their tissues, and the tissues the functions of their cells.

The same can be said about *c.fin* As soon as a successful program develops in a system, all the required functions can be understood from superimposed layers; the function of a cell by its tissue, the tissue by its organ, the organ by the specimen of a species. Even the specimens' behavior and interaction with other specimens can be understood with the goal to support, to secure and to improve the maintenance of a species.

(2) Looking back to such a bottom-up and topdown causation, we must again confess that nature gives no indication to be split in such kinds of causes. In contrast to the reality of the layers, it is again a cognitive dualism which guides our thinking. This cognitive dualism is not so easy to locate in our grammar as in the division of gestalt versus

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motion and change, resulting in nouns and verbs. The division in mind has more to do with our subjective feeling for active and passive processes—to assemble or to become assembled.

At a first glance, this does not sound like much, but a deeply rooted principle of perception sits behind this: the 'Reafferenz' principle (VON HOLST 1996/70, LORENZ 1978), which is of life-supporting importance. Recall that the efferent nerves actively direct our actions; conversely, the afferent ones tell us whether the intended action is accomplished (e.g. 'touch the key in the pocket'). Their mutual control allows us to differentiate between active and passive occurrences (e.g. 'shaking with the bench' versus 'being shaken with the bench'). No doubt, to push or being pushed makes a difference which we feel noteworthy.

In an anthropomorphic way, we even unconsciously apply this difference tentatively to processes in general; what may lie in one's own reach and what not (what I assemble in the company, versus the company become disassembled being in debt). Power and materials, *c.eff.* and *c.mat.*, normally seem to be in our reach, suggesting active processes. Being selected and bound to a goal however, *c.form.* and *c.fin.*, also seem vital but in passive forms, inescapably fateful.

The circumstance that the sciences restricted themselves to what can be taken apart or put together can be no coincidence: the so-called positive facts in observable phenomena; i.e. *c.eff.* and *c.mat* To take as explanatory what can be reduced to its components is known as positivism and reductionism.

IV What urges the shift from forces to causes?

If one focuses on complex systems, the chance that tcausal explanation may be traced back to the four forces fades away. The reason for this can be described under four labels: (1) indeterminism, (2) phase transitions, (3) emergence, and (4) historicity.

Indeterminism

Relatively short chains of causes and effects may extend the phenomenon of indeterminism from the domain of microphysics into the macroworld. Assume a mathematically ideal billiards, but made of matter, with eight billiard balls, one meter apart from one another. It is to be expected, then, that the seventh ball can miss the eighth ball. This is because the indetermination of the position of the surface molecules, multiplied by itself seven times, surmounts the diameter of the balls

A simple empirical case is the laser. Stuffed with energy, the crystal will certainly emit a beam of light. Yet the direction of this emission cannot be predicted (HAKEN 1983); as a parliament of too many molecules is involved.

We have no doubt that nothing but the four forces can also be the cause of such phenomena. But even the most precise knowledge of them would not suffice for a precise foresight. Even in such simple cases we stand on more realistic grounds if we speak in terms of power, *c.eff.*, and material, *c.mat*

Phase transitions

The situation becomes more obvious if, in addition, we consider the observed phase transitions. Take the simplest case, such as freezing water; it could not be predicted which molecules would form the first crystal; which then coerces the orientation of the others.

Never could enough information be at hand to calculate, e.g., whether the distribution of the single hydrogen molecules of a cloud in a gravity field would be in the appropriate position to form a star or not. For the development of celestial bodies, we better take the cloud and the gravity field as *c.form.*, the hydrogen as *c.mat.*, and the motions which occure as *c.eff*.

Phase transitions become impressive in situations of higher complexity; such as the transition to life. The necessary molecules within the 'hot soup' are taken as *c.mat.*, their motion and energy content as *c.eff.*, the environment as *c.form.*; and the first self-sustaining programs of mutual support, (e.g.,between RNA chains, amino acids, and proteins) prepare what lateron will be unterstood as *c.fin.*

Here it last it should become clear that it would be wrong to lump together, to confuse, or to ignore the differentiation of the four causes. Each of them emerges from different sources, each acts very differently, each is indispensible for explanation, and only together they provide a sufficient explanation. In distance from any possibility to trace this causes down to the four forces, the four causes become indispensible holdfasts to sufficiently explain complex systems.

Emergence

If one goes on to the phenomena of emergence, the cases become even more convincing. Starting from a hierarchically layered system, we accept that the laws that direct lower layers reach through the higher ones. No doubt, cell physiology acts in every composer's brain, and the laws of chemical bindings in each of its cells! This may be taken as a truism.

Fewer considerations attracted the fact that the conditions of the superimposed layers act towards the lower ones as well. But one could not deny that the atmospheric conditions on this planet lay at the basis of, and are still undispensible for life on earth, and that human society contributed and still contributes to the emergence of composers.

However, in addition to the necessity of such a two-sided explanation, each newly inserted layer develops new systemic conditions which even in traces cannot be tracked back to the sub- or superimposed systems between which it originates. Even under the purely theoretical assumption that all the conditions of the neighboring systems were sufficiently known would this not be practiceable. Because the number of possible combinations of the parts and the conditions involved grows so extremely high, the probability of predicting the outcome is practically zero.

Moreover, the four causes give a guideline not to oversee or confuse conditions, with regard to grasp, from which side, and in what way they act, for the simple reason that they are all indispensible for sufficient explanation; and no way anymore exists to trace their contents back to the four forces.

Historicity

This leads us to the fourth perspective: historicity. That history never repeats itself might be another truism to state; but this is the point I have in mind. As we see it today, all complex systems, starting from celestial bodies, have history.

Historicity binds the three former phenomena together. For historicity starts with indeterminism reaching into the macroworld. It continues with the phenomena appearing in phase transition, i.e. that even under the most controlled conditions not all the characters of the new system can be predicted. It cumulates in the phenomenon of emergence as we must face the fact that in historical processes many emergent phases have been gone through. No possibility to fully reconstruct or repeat such processes is left over, then.

No doubt, even historical processes are grounded in nothing but the four forces. But no way is left to fully reconstruct the paths that they have taken. On the other hand, the four causes still form reliable patterns to match the two cognitive symmetries, so deeply rooted both in thinking and speech.

V How do the causes correspond to forces

One can hardly think of a complex process in our world that would not involve all four forces. This must be particularly the case with processes that are responsible for the composition, operation, and maintenance of complex systems.

As a consequence, we can expect all four forces to participate in all the four causes. Since matter will be involved, strong and weak interactions will act as well as electromagnetism; and gravity will play a role at least in the sense of keeping the systems bound to earth. What is different with respect to the four forces is—as we will see—the explanatory power each of the forces begets in each of the four causes.

Causa efficiens

The contributions of *c.eff.* are conceptually quite directly interconnected with forces. It is not difficult to trace capital for a building back to bustle, labor, metabolism, food, calories, chemical bounds, photosynthesis and eventually radiation. The same is the case for systems that take direct advantage from gravity, such as sedentary animals living in currents, or for rafts and water mills—ultimately, the whole water circulation of the planet.

Selection is also involved in *c.eff.*, which marks it off from simple interconnections. Remember that in every link in a food chain about 90% of the transmitted energy is lost. Similar concerns are known as the 'efficiency' of machines and systems in general (ODUM 1971). Theoretically the effort to build a gothic cathedral could be traced back over labor, food, photosynthesis, and radiation, to the number of photons required to get the building up; in practice this makes no sense.

Causa materialis and formalis

As far as *c.mat.* and *c.form.* are concerned, we may treat both of them together. They are similar with regard to a predominance of decision making pro-

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cesses, and, in a way, they are, as we will see, symmetrical in the qualities of their effects. For this purpose we here take 'selection' in its positive sense, as 'being winnowed', in contrast to 'elimination'. We must distinguish between pre- and post-decisions. If we talk about selection as we normally have in mind, viz., that a new system has been probed, and the environment selects or eliminates; this is post-decision or post-selectivity. However, systems may come into being with more or less appropriate constituents available; this I call pre-decision or preselectivity.

(1) C.mat., in this sense, is pre-selective, a pre-decisive process starting from subsystems, i.e., the constituents. We may distinguish five qualities of effects: (a) lack of components, (b) minimum conditions, and (c) conditions of profuseness, (d) effects of the kind of constituents, and (e) the constituents' origin. The most competitive typewriter fails (a) without paper. Whipped cream may be a delight in (b) small amounts, but in a pool of whipped cream (c), one would drown. Bridge building can be efficient with building blocks or with ropes (d). The result, however, would be an arcade versus a garland shape. And a shelter built in a rain forest, in a rock desert, or on the North pole, would, in shape, depend (e) on the materials at disposal.

(2) C.form. is post-selective, the post-decisive process, specified by the superimposed system, i.e. the environment; and the same five viewpoints can be applied. Water is the environment for a swimmer; the best swimmer would fail (a) if a holder for the water is missing, as in the rain or in a waterfall; a bathtub (b) would be below, the mid-Atlantic (c) beyond the maximum, a profuse condition, to perform the action for long.

The effect of a sieve, as the simplest post-decisive tool, depends (d) both on the mash size of the grid, the riddle, bolter, or filter, and it depends on the material to be selected or eliminated. If one, strangely, sieves hair (*c.mat.*), it depends how long they are before they pass through the mashes; if one sieves animals, this depends on how fast they cling to the grid. And if one shakes gravel endlessly on a fine steel grid, one will get silt and dust, until the grid breaks; rather than one receives gravel.

With regard to the origin of materials (e), one can maintain an igloo in a rain forest as well as an outdoor swimming pool on the South pole; it depends on the expenses of energy.

No doubt, transformation of power and/or energy transfer will play a role in whatever phenomena we spot in connection with *c.mat.* and *c.form* But if one considers who made and carried the materials, who made the bridge or shakes the sieve, one may agree that applying the four forces only would miss the point.

Causa finalis

C.fin. differs from the three other causes in only one point. Whereas one can hardly think of a complex system in which c.eff., c.mat. and c.form. are not involved, there are systems without *c.fin*.

C.fin. starts with the emergence of goal-seeking programs being successful in having been repeatedly and severely tested; and being in principle self-repeating, recursive, or iterative. Such programs are to be found in living beings, and as a basic component of artefacts; they are in great abundance. We do not think of programs in the inorganic world, because we do not see improvement and self-repetition, not even in long chains of chemical reactions. It might be that the universe is pulsing, repeatedly forming a new universe. It might bee that the 'life-cycle' of a star, after its collapse, is up to produce a new star. Even if this were the case, what could of improvement would lie behind this? What we have in mind are 'goal-seeking programs'. Such goals are set to maintain, i.e., to replicate and multiply, life, or, as one would expect, to satisfy and improve living conditions.

Such programs can be consciously designed, purposeful actions, intentions, a carpenter making a table, or plans put in a computer or a machine. But there are also many programs beyond consciousness; such as in a chicken's egg (to most probably produce a chicken), or in our liver (to cope with fat and alcohol excesses). Actually all conscious programs have unconscious ones as basic requirements/ outfits. And, as EE reveals, there are many programs in man that could be made conscious, but mainly act unconsciously.

All programs have history-sometimes a long history. Basic programs, such as DNA duplication, transcription and translation must be as old as life;

ontogenetic programs as old

as multicellularity: as Meta-

zoa. Consequently, they have e repetitive character. The Rupert Riedl, Konrad Lorenz-Institute for same seems to be the case in Evolution and Cognition Research, A-3422 all goal-seeking planning. Even a most inventive step-

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programming a machine, or a path to new discoveries—has the same two characters. First:, the novelty has, of course, an emergent character, due to new combinations and/or expansions of preformed programs from which it derives. But it is also built on preexisting programs that were handed down by tradition. Secondly, we expect that a successful program can be used over and over again.

Consequently *c.fin.* stands for 'repetitive, successful, goal-seeking programs with history'. They stand for what PITTENDRIGH (1958) called teleonomy, to contrast it with teleology, which has been used in too many ways. In probably all cases where *c.fin.* dominates, the three other causes will be involved, as well as all the four forces. But an attempt to trace the phenomenon of *c.fin.* back to the four forces, to *c.eff.*, or even to the other three causes, would bypass what is pertinent in the most exciting events on this planet.

VI Conclusion

The four physical forces and their action cannot be doubted; nor are they in any way altered by using four causes. There is also no evidence, and not even a necessity, to assume that the rationale of a changing world is divided into four different causes. The division has to do with our inherited outfit to perceive the phenomena of the mesocosmic dimensions of this world, and to compute the perceived data for life-supporting activity.

It cannot be doubted either that the four forces *in principle* allow a sufficient explanation of whatever process, or whatever history of a complex structure has to be explained. The point, however, is the 'in principle'. In practice, the chance of such an explanation fades away the more history, the more phase transitions complex systems have undergone. Tracing their conditions back to the four forces remains nothing but a figure of speech without explanatory value.

If, on the other hand, we appreciate constraints of the make-up of the human mind, the two cognitive symmetries that were given, i.e., the four ways of grasping the causes of complexity, we gain a much more reliable guide, viz., to avoid the fashionable fallacy of evading the core of complexity, to bypass the very conditions one which life and culture depend.

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Nietzsche's Naturalistic Epistemology and some Implications

bottom, Friedrich T NIETZSCHE'S (1844– 1900) epistemology is naturalistic; that is, knowledge for him is not something independent of life, but describable only in terms of biological creatures and their interactions with their environment and each other. Approaching epistemology from this direction, NIETZSCHE, like Konrad LORENZ (1977, 1982), Rupert RIEDL (1982), and Franz WUKETITS (1990), is able to wed the subject of human knowing to the

problem of survival. Indeed, as NIETZSCHE puts it, human knowledge "is to be regarded in a strict and narrow anthropocentric sense... The utility of preservation... stands as the motive behind the development of the organs of knowledge... [and] their observations suffice for our preservation" (WP¹ §480). In the following paper, I review the naturalistic qualities and some of the philosophical implications of NIETZSCHE's epistemology, concluding that he advances a number of ideas relevant to the concerns of the evolutionary epistemologist. Along the way I refer to many of NIETZSCHE's works, including *The Will to Power*, an incomplete collection of notes arranged and published after his death.

Introduction

"The problem of consciousness," explains NIETZSCHE, "steps before us only when we begin to understand to what extent we could do without it" (GS §354). Here, he says, our attention must turn to "physiology and the natural history of the animals" (GS §354) because the becoming-conscious of our

Abstract

The following paper examines the epistemological writings of the German philosopher Friedrich NI-ETZSCHE (1844–1900). The author argues that NI-ETZSCHE's epistemology can be best described as naturalistic and that it holds three important philosophical implications: that knowledge is selfreferential, that error is a condition of knowledge, and that there is a terra incognita of knowledge. The author then outlines six points on which NI-ETZSCHE's epistemology speaks to the concerns of the evolutionary epistemologist.

Key words

Associationism, consciousness, EE, living control system, path dependency. perceptions was "essential to us and to the entire organic process" (WP §505). Continuing along these lines, NIETZSCHE speculates that human consciousness was probaevolutionary bly an response to the problem of survival. "As the most endangered of animals," he hypothesizes, early man "required help, protection, he required his own kind, he had to express his needs, know how to make himself understood—and for that he first had need for 'con-

sciousness', that is to say, himself needs to 'know' what he lacks, to 'know' how he feels, to 'know' what he is thinking" (GS §354). Thus, he explains, "it seems to me... that consciousness evolved at all only under the pressure of need for communication—that it was from the very first necessary and useful only between man and man... and also evolved only in proportion to the degree of this usefulness. Consciousness is really only a connecting network between man and man—only as such did it have to evolve: the solitary and predatory man would not have needed it" (GS §354). Consciousness, he concludes, "is present only to the extent that consciousness is useful" (WP §505).

But what of the contents of consciousness? On this question NIETZSCHE continues with his naturalistic approach. In fact, he argues that an essential factor underlying the survival of the early human species was their capacity to *associate* sensory experiences, to identify the "similar," (WP §511) the "equal," (WP §510) and the "identical (HH §18, WP §521). Indeed, he explains, "countless creatures who reasoned differently from the way we now reason

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have perished... he, for example, who did not know how to discover the 'identical' sufficiently often in regard to food or to animals hostile to him... had a smaller probability of survival than he who in every case of similarity at once conjectured identity" (GS §111). Moreover, the capacity to associate sensory experiences conferred a selective advantage on early human beings because it improved their ability to interact with the world, or as NIETZSCHE puts it, the power to associate sensory events "labored in the service of our needs, namely of our need for security, for quick understanding on the basis of signs and sounds" (WP §513). The creatures who could simplify their sensory experiences in this way "had an advantage over those who saw everything in 'flux'... No living creature would have been preserved if the opposite [of the] tendency [to see in 'flux'] had not been cultivated with extraordinary vigor" (GS §111).

For NIETZSCHE, then, the capacity to associate sensory experiences was fundamental to early human survival. He goes on to speculate that the earliest human minds probably experienced the world much as modern minds experience dreams. As NI-ETZSCHE explains: In dreaming we are

taken back to a condition of imperfection in which everyone may have possessed in the primeval ages of mankind. Capricious and confused as [the dreaming mind] is, it continually confounds things on the basis of the most fleeting similarity... [it] recall[s] to us states of earlier mankind... Thus: in sleep and dreams we once again go through the curriculum of earlier humanity (HH §12).

But this was only the beginning. By way of associations, humans came to discover ways of assembling and organizing their tempest of sensory events into useful categories and patterns; "to impose upon chaos as much regularity and form as [their] practical needs require[d]... to subsume, to schematize, for the purpose of intelligibility and calculation" (WP §515). That is to say, the capacity for association allowed for the emergence of 'knowledge'increasingly complex webs of associations that enabled humans to pragmatically navigate through the world's storm of sensory information. Otherwise, as William JAMES (1940) points out, if humans were unable to order their sensory experiences, they would simply get each successive moment of experience as a sea-anemone on a reef receives whatever nourishment that washes by. But through ordering, we harness our sensory experiences and drive them to our pragmatic ends (p51). Thus understood, humans maintain themselves by gaining "power"

(WP §480) over the physical world. We grasp, explains NIETZSCHE, "a certain amount of reality in order to become master of it, in order to press it into service" (WP §480). In this sense, our knowledge is not oriented toward the world in itself, but toward gaining *control* over it, or as Olaf DIETTRICH (1994) has put it, our knowledge "is a process of conquering the world rather than discovering it" (p72).

NIETZSCHE recognizes, however, that his associationist model of knowledge must accommodate the fact that our sensory experiences do not occur all at once. Instead, we have past experiences and current experiences, and it is the past experiences that organize current ones into patterns that become the basis for their cognitive significance. Consequently, the knowledge process does not begin with the experiences at hand, but with our accumulated inventory of experiences, or more precisely, with our mnemonic archive. In other words, human knowledge is forged by the association of new sensory experiences-optical, acoustical, and otherwise-with past sensory experiences. "What appears is always something new, and it is only we, who are always comparing, who include the new, to the extent that it is familiar to the old" (WP §521). We "subsume a sense impression into an existing series" (WP §511)--"the same process every sense impression goes through" (WP §515)—"fitting new material into old schemas, making equal what is new" (WP §499). In this sense, knowledge is "nothing more than this: something strange [that can be] traced back to something already known by the senses" (GS §355).

Since sensory experience precedes knowledge in this way, NIETZSCHE concludes that knowledge is not self-evident, but something constructed. The attributes of a given sensory event, in other words, are not intrinsic qualities that are somehow communicated to the mind. Rather, knowledge is brought to things *before* they are comprehended; i.e., "before there is 'thought' there must have been 'invention', the construction of identical cases, of appearances of sameness, is more primitive than the knowledge of sameness" (WP §544). Or better yet, "there are no 'facts-in-themselves', for a sense must always be projected into them before they can be 'facts'" (WP §556). This is not to say, however, that we create the external world. As NIETZSCHE clearly explains:

What? And others even say that the external world is the work of our organs? But then our body, as part of the external world would be the work of our organs! But then our organs themselves would be—the work of our organs! It seems

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to me that this is a complete *reductio ad absurdum*, assuming that the concept of a *causa sui* is something fundamentally absurd. Consequently, the external world is *not* the work of our organs (BGE §15)?

The point NIETZSCHE is trying to make here, rather, is that the reason that the world we know seems wholly an orderly world is not the result of a translucent *logos*, but the result of the method by which we perceive it; i.e., "the world seems logical to us because *we have made it logical*" (WP §521, emphasis added). If *logos* were self-evident there would be no hesitation regarding meaning—the order of our ideas would simply conform to the order of things, doubt would be eliminated and we would possess knowledge for all eternity. On the contrary, NIETZSCHE does not view knowledge as a ubiquitous logic, but as an intelligibility arrived at through the domestication of sensory experiences. Knowledge is only the *aftermath* of this process.

This emphasis on the notion that we construct our objects of knowledge by organizing new and old sensory experiences, however, distinguishes NI-ETZSCHE's epistemology from that of, say, John LOCKE (1690). Indeed, for NIETZSCHE, we initiate knowledge by bringing meaning to our environment, but LOCKE holds an essentially instructionist theory of perception. LOCKE believes "that external objects furnish the mind with the ideas of sensible qualities" and "external objects convey into the mind what produces there... perceptions" (pp122-124 emphasis added). To put this distinction another way, the LOCKEAN view of the relationship between subject and object can be represented by a film projector that casts the external world onto our mind; whereas in NIETZSCHE's view of the subject and object relationship, the activity of knowing can be represented by a flashlight that illuminates aspects of the world around us. In either case the initiation of knowledge flows from opposite sources; for LOCKE an encountered sensory experience imposes knowledge on us, for NIETZSCHE, we impose knowledge on an encountered sensory experience. The implications of NI-ETZSCHE's associationist model of knowledge, however, do not end here. His model leads to three other important conclusions: that knowledge is selfreferential, that error is a condition of knowledge, and that there is a *terra incognita* of knowledge.

Self-Referentialism

As we have seen, NIETZSCHE contends that knowledge is a network of new and old sensory data in a

web of associations. This web, he maintains, involves past sensory information to which new sensory information is connected; i.e., sensory data has significance insofar as it shows a certain regularity in appearance to things we have experienced in the past. The implication of this process is that each sensory event is colored by experiences which are not part of the particular event that is occurring, but shaped by what exists within the network of one's prior experiences. As a result of this process, knowledge cannot be broken down into component sensory events. Each sensory experience's identity is defined not by itself as a discrete unit, but by its interrelations to other sensory events. They are linked with one another in such a way that they actually determine what each other is through their interconnections. Our sensory experiences thus are not singular in nature. Instead, each bit of sensory information is intertwined with the numerous other bits of sensory information that comprise our personal history. Ultimately, our knowledge is not a unity, but "something complicated, something that is a unit only as a word" (BGE §19) and where one part ends and another begins is undecidable; all experiences are embedded in a complex of associations to other experiences.

This contention that sensory experiences are not discrete, but always inherently plural, leads NI-ETZSCHE to conclude that they cannot be reduced to singular events. "If I remove," he writes, "all the relationships, all the properties... of a thing, the thing does not remain over" (WP §558), once "one removes other 'things', then a thing has no properties," no context and, therefore, no meaning (WP §557). For NIETZSCHE, then, there is no such thing as sensory data in itself. Rather, there is only sensory data in relations with other sensory data. The implication of this understanding is that sensory experiences cannot be analyzed irrespective of the other contents of the mind which contains them; that is, in order to describe a sensory experience all the way through, you must describe its relations to other bits of information, which in turn are related to further bits, and so on in an infinite regress. Logically, any truly complete description of a sensory experience would have to consider the comprehensive order that arises from each person's previous sensory experiences.

Conceiving of knowledge as a whole in this way implies that what we know at any moment about the external world is shaped by the order of the apparatus of association which has been built up by previous sensory experiences. That is to say, we interpret

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any new event in the environment in light of experience. Indeed, we are like historians confronted with a collection of documents which must be interpreted. Although the documents may suggest some hypotheses, the data must be organized in order to arrive at an explanation of the past that has some coherence. There is, however, an "intermediary sphere" (TL §1, quoted in GRANIER 1985, p192) between historian and document, subject and object, a sphere occupied by our previous experiences. Understanding, thus, is mediated by the specific experiences of the historian/observer.

It is important to recognize, however, that since every experience is uniquely conditioned by what precedes it in one's specific life, no sensory experience is uniform, but relative to one's experiential background. Knowledge, in other words, is biographical or, more figuratively, historically fingerprinted. Under this view, a question like "What is X?" has strict meaning only within relation to the perceiver's unique web of historical experiences. Accordingly, reports Karl JASPERS (1965), NIETZSCHE maintains that "all knowledge is an interpretation of being provided by a living and cognizing subject... [therefore] truth is not something independent, unconditioned, and absolutely universal. Rather it is inextricably involved with the being of the living subject and the world that he has constructed" (pp184–185). In other words, "man finds in things nothing but what he himself has imported into them" (WP §606, emphasis added) or, more boldly, "facts are precisely what there is not, only interpretations" (WP §481).

Conversely, we cannot interpret that to which our experiences do not provide us a connection. Indeed, we can only know such kinds of events that show a degree of regularity in their occurrence in relations with others *already* in our mind or, as NIETZSCHE puts it, "nobody can get more out of things than he already knows for what one lacks access to by experience one will have no ear" (EH §4:1). In short, a sensory event which has no relation to anything previously perceived cannot be comprehended. We sit within our historical net, explains NIETZSCHE, "and whatever we may catch in it, we can catch nothing at all except that which allows itself to be caught in precisely our net" (DB §73). That is to say, since it is possible to understand only what is similar to our own mind, it necessarily follows that we must be able to find all that we can understand in our own mind. In other words, a sensory experience foreign in all its aspects will be utterly incomprehensible; information is only intelligible when it can be associated

with that which is already familiar to us. Consequently, much of what we believe we know about the external world is, in fact, knowledge about ourselves, a disclosure of who we are historically. Indeed, "no matter how far a man may extend himself with his knowledge, no matter how objectively he may come to view himself, in the end it can yield to him nothing but his own biography" (HH, quoted in THIELE 1990, p28).

Self-referentialism thus holds a central position in NIETZSCHE's epistemology. To be sure, he admits that in his own case he has no right to claim to have understood his mentor: "I am far from believing that I have truly understood SCHOPENHAUER, rather it is only that through SCHOPENHAUER I have learned to understand myself a little better" (GW §7:140). Elsewhere, he concludes, "at the bottom of it there always lies 'what is that for *me*?'" (WP §556) and I assume "that it is known from the outset how very much these are after all only—*my* truths" (BGE §231).

Error as a Condition of Knowledge

Given the constructed quality of knowledge, NIETZSCHE maintains that what we perceive of the external world are never all of the properties that a particular object or event can be said to possess objectively, but are always only certain aspects that we associate with past experiences. In other words, our mind is not a strict catalogue of actual objects and events, but a collection of similarities that are "selected and gathered" (quoted in GRANIER 1985b, p135) with "the aim of making similar, equal" (WP §515). Thus understood, "the world with which we are concerned is false, i.e., it is not fact but a fallible approximation on the basis of a meager sum of observations" (WP §616). All our perceptions, in short, are a kind of "error" (WP §520) because they are simplified and assumption-laden.

Logic too depends on presuppositions with which nothing in the real world corresponds, for example on the presupposition that there are identical things, that the same thing is identical at different points of time... It is the same with *mathematics*, which would certainly not have come into existence if one had known from the beginning that there was in nature no exactly straight line, no real circle, no absolute magnitude (HH §11).

NIETZSCHE recognizes, however, that such 'error' is a condition of our knowledge. For him, the world is in "continual transition" (WP §520) and in a state of "continuous flux," (GS §11) and "becoming" (WP

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§520). Given this, we should understand that 'error' is necessary for us "to arrange a world for ourselves in which our existence is made possible... [to] create a world which is calculable, simplified, comprehensible, etc., for us" (WP §521). Indeed,

A world in a state of becoming could not, in a strict sense, be "comprehended" or "known;" only to the extent that the "comprehending" and "knowing" intellect encounters a coarse, already created world, fabricated out of mere appearances... has [it] preserved life—only to this extent is there anything like "knowledge" (WP §520).

In short, NIETZSCHE recognizes the primacy of practice over certainty in the construction of human knowledge. Indeed, he explicitly points out that "truth" is not the motive here, but rather "the utilitarian fact that only when we see things coarsely and made equal do they become calculable and usable to us" (WP §515). Or, to put this point more boldly, "truth is a kind of error without which a certain species of life [namely ours] could not live" (WP §493) and our webs of associations are "'truths' only in the sense that they are conditions of life for us" (WP §515). Many "of them proved useful and preservative of the species" (GS §110) and some became "very well acquired habit[s] of belief, so much a part of us that not to believe in [them] would destroy the [human] race" (WP §497).

It is not surprising, then, that NIETZSCHE contends that a "truth" is a "provisional assumption" (WP §497) that does not give us a picture of the world *Ding an* sich. Rather, "we have senses for only a selection of perceptions-those which we have to concern ourselves in order to preserve ourselves" (WP §505, emphasis added). NIETZSCHE realizes, however, that we cannot separate our means of perception from that which is being perceived; i.e., "we behold all things through the human head and cannot cut off this head" (HH §9). Indeed, there is no unmediated vantage point from which knowledge can be gained, for such a vantage point implies a possibility not granted to us-the possibility of transporting ourselves outside of our existence so as to behold the world. Instead, participation in existence is our only window unto the world. We cannot escape from our mind's historical lens so as to attain a presuppositionless or unsituated view of the world as a whole in itself. Our mind's perspective is the only basis for all our judgments and knowledge-"there is absolutely no escape, no backway or bypath into the real world" (DB §73)!

The belief that there is an objective viewpoint which gives an accurate account of the world, con-

cludes NIETZSCHE, thus must break down. No such world is accessible. Our view of the world is unavoidably mediated by the very means by which we perceive it or, as he analogizes, humans are like creatures with their backs to reality and a mirror before them. No matter how close to the edge of the mirror they go, their view of the whole of the reality behind them will always be obscured by their own eye. "Why does man not see things?" he asks, because "he is himself standing in the way" (DB §187).

That we cannot achieve an unmediated view of the world leads NIETZSCHE to conclude that the historical aim of philosophy to develop a transcendental metaphysics must be abandoned. The objective of philosophy cannot be the formulation of a metaphysical system, but rather the investigation of the limits of human knowledge. Moreover, such an investigation must be reflexive since, in the end, all philosophical inquiry is phenomenological inquiry. NIETZSCHE thus repudiates the belief that metaphysics can expose the essence or nature of things. Against positivism NI-ETZSCHE rejects the belief that there is available to us "immaculate perception" (TSZ, p233) or pure sensations which can contribute to an objective picture of the world. Instead, everything we observe is imbued with presuppositions. As NIETZSCHE explains, "the entire apparatus of knowledge is an apparatus for abstraction and simplification" (WP §503) and the picture we form of the world is abstracted from the infinite aspects which it contains and always "involves a great and thorough corruption, falsification, reduction to superficialities and generalization" (GS §354). There is thus no basis to believe that the representation of physical reality which our knowledge makes possible is an accurate representation of the world as it is. Each mind functions, rather, through a recognition of what is similar to that mind at the expense of what is particular to an item.

Thus, concludes NIETZSCHE, we must divest ourselves of the habitual assumption that all we have learned from experience must be true. Knowledge based entirely on experience may be entirely false; i.e., "however often 'the same form is attained', it does not mean that it *is* the same form" (WP §521). Moreover, as knowledge must always refer to selected experiences in association with other experiences, knowledge is based on the assumption that these associations *actually* exist. In short, NIETZSCHE questions the very authenticity of the image our mind presents to us because the contents of our knowledge are always selected and ordered. Hence, he argues, our knowledge of the world is always only conjectural and we should not dogmatize it. Hold all

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your positions open to criticism, says NIETZSCHE, even this one. Or, as he puts it:

"Everything is subjective," you say, but even this is an interpretation. The "subject" is not something given, it is something added and invented and projected behind what there is.— Finally, is it necessary to posit a interpreter behind the interpretation? Even this is invention, hypothesis (WP §481).

Terra Incognita of Knowledge

In NIETZSCHE's view, that which we call knowledge is generated from the association of new and old sensory experiences. The knowledge that arises from this activity has its root therein, and it emerges therefrom, but does not belong to it. Instead "we are a plurality that has imagined itself a unity" (WP §333). Our knowledge is the culmination of the workings of a complex web of associations that organizes our sensory experiences into useful patterns and which is implicit in everything to which we refer.

Yet, says NIETZSCHE, this organizing activity has its roots in the physiological activity of our brain. Indeed, he maintains that aesthetics and judgment are merely the "'subtlest nuances' of the *physis*" (GS §107) and that his objections to the music of Wagner, for example, "are physiological objections: why should I trouble to dress them up in aesthetic formulas? After all, aesthetics is nothing but a kind of applied physiology" (NCW p664). Yet NIETZSCHE is led to question just how much we can come to know about our own knowledge because we cannot know "how deep and wide the physis reaches" (GW §2:388). Indeed, he explains:

"[T]hat which becomes conscious is involved in causal relations which are *entirely withheld* from us" (WP §524, emphasis added) [and all] our moral judgments and valuations are only the images and fantasies of a physiological process unknown to us, a kind of convenient language to describe certain nerve stimuli. All our so-called consciousness is only more or less fantastic commentary upon an unconscious text, one which is perhaps unknowable yet felt (DB §119, quoted in BLONDEL 1985, p170).

In other words, as Michael HAAR (1985) summarizes NIETZSCHE's view, "the schematizing and assimilating activity of knowledge is not even the work of consciousness. This activity emerges already at the level of the body, and from there enters onto the conscious level" (p17). On physiological grounds, then, NIETZSCHE identifies a *terra incognita* of knowledge, for everything that is made conscious "belongs to the surface and skin—which, like every skin, betrays something but *conceals* even more" (BGE §32).

NIETZSCHE warns us, however, that just because the mind's "great principle activity is unknown" (GS §354), that it should not be characterized as inferior to conscious activity. He puts this point clearly when he explains that it is generally taken for granted that in some sense conscious experience constitutes the 'highest' level in the hierarchy of mental events, and that what is not conscious has remained unconscious because it has not yet risen to that level. But this is no justification dismissing them as inferior to conscious processes. For NIETZSCHE, it is a mistake to "regard the indistinct idea as a lower kind of idea than the distinct... that which removes itself from its consciousness and for that reason becomes obscure can on that account be perfectly clear in itself. Becoming obscure is a matter of perspective of consciousness" (WP §528).

The point NIETZSCHE is trying to emphasize is that the knowledge that emerges from the complex interaction of past and present sensory experiences seems never to be the outcome of a conscious process, not something at which we deliberately aim, but always a discovery of something which already guides its operation. "The nervous system," explains NI-ETZSCHE, "has a much more extensive domain; the world of consciousness is added to it. Consciousness plays no role in the total process of adaptation and systemization" (WP §526). If this activity were consciously accessible, we would know the very causes of our knowledge. But this, he says, is impossible because we cannot self-consciously calculate the activity to which all our conscious thoughts refer. In other words, to know our own knowledge would require that we should know more than we actually do, which is, of course, a non-sensicle statement. We instead function on the results of our mind's physioassociative activity. Our knowledge is not guided by particulars which are introspectively accessible, but which nevertheless guide it. As NIETZSCHE puts it: "Thoughts are signs of a play and struggle of affects: [and] they always are connected to their hidden roots" (GW §16:60).

In this regard, NIETZSCHE contends that what we experience as knowledge is a symptom of our mind's ordering of our "thousandfold complexity" (WP §523). Under this view, knowledge is not a beginning, but an "end" (WP §478), the last link of a chain, the verdict of underlying physio-associative activity. It is simply a "shadow" (GS §179) of cogni-

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tive activity that is elsewhere delineated; "for, to say it again: man like every living creature, thinks continually but does not know it; thinking which has become *conscious* is only the smallest part of it, let us say the most superficial part, the worst part" (GS §354). NIETZSCHE sees knowledge, then, not as *self*conscious, but as a phenomenon subordinate to an ordering and arranging self which is not conscious, i.e., it is not a "master," but a "slave" in relation to a "master" who is not conscious (see DELEUZE 1983, p39). "That a higher court rules over these things," concludes NIETZSCHE, "cannot be doubted" (WP §524).

This view leads NIETZSCHE to address what is called the "actor-action" presumption (see STRONG 1975, 63-72). The 'actor-action' presumption holds that actor A causes action B; i.e., actor A brings about action B. Such a distinction places the emphasis on the deliberate subject and implies that he/she has some meaningful discretion over action B. It implies that actor A has the ability to do otherwise and exercises an independence of action B. For NI-ETZSCHE, the most important consequence of the "actor-action" presumption is its overestimation of human reason. Under the "actor-action" presumption the subject is separated from his/her acts and given the status of a preeminent author. But in NI-ETZSCHE's view, an action is not qualitatively separable from the unconscious physio-associative process that already arranges, simplifies, schematizes and interprets the contents of consciousness. As a result, the reasoning 'I' is merely a fiction attributed to the actor whose movements are based upon the same physio-associative activity. To then say that actor A causes knowledge B is like saying the "lightening flashed;" it simply states two aspects of the same phenomenon and imposes a nonexistent causal relationship between them (WP §548). With regard to the issue of knowledge, then, NIETZSCHE concludes that there is no centralized agent at work. Indeed, we incorrectly believe "ourselves to be causal in the act of willing;" we incorrectly believe an action's causes are "to be sought in consciousness;" and, above all, we incorrectly believe "that the 'I' causes the thought" (WP, quoted in STRONG, 1975, pp70-71).

This view dramatically undermines the notion of a CARTESIAN subject. Indeed, according to NIETZSCHE, our knowledge is *not* guided by a 'declarative' or 'deliberate' self. Rather, it is mediated by a different system, one that emerges out of our physis. Our knowledge, in other words, does not necessarily imply the presence of a independent 'I' that designs and directs it. Instead, our knowledge is the by-product of an underlying plurality of physio-associative processes that often function to our maintenance. The conscious self and conscious sensations like "pleasure and displeasure" are hence not the prime movers behind our knowledge, rather, "they are value judgments of the second rank derived from a ruling value--'useful', 'harmful'" (WP §701). Our knowledge tends, in other words, to be far more arational; that is, in NIETZSCHE's view, our knowledge may appear vernünftähnlich or reason-like because it frequently works in our conservation, but its basis is not at all 'rational'. It is, instead, the outcome of a "tremendous multiplicity of events within an organism" (WP §674) whose distributed "interaction and struggle is the basis of our thoughts and consciousness in general" (WP §490).

Conclusion

Given our movements through time and space, NIETZSCHE maintains that "all our doing and knowing is not a succession of facts... but a continuous flux" (WHS §11). The things we know are but the result of the physio-associative activity of the mind as it orders experiences and is modified continuously by experience. In other words, the mind's contents and its relations are continually being updated with the introduction of each new experience. As he explains, "the power of the mind to absorb foreign elements reveals itself in the strong tendency to make the new like the old... to simplify the manifold... its purpose is the incorporation of new experiences, the adding of new material to old, its growth... Really, the mind is more like a stomach than anything else" (BGE §230).

NIETZSCHE is not explicit, however, in explaining what this process of 'growth' entails. He does maintain that "we ourselves keep growing, keep changing, we shed our old bark, we shed our skins every spring... we are no longer free to do only one particular thing, to be only one particular thing" (GS §371). Whether this sort of 'growth' implies the sort of 'blind variation and selective retention' the evolutionary epistemologist is interested in is not stated. Nevertheless, NIETZSCHE does advance a number of ideas relevant to the concerns of the evolutionary epistemologist.

First, NIETZSCHE agrees with the view that human knowledge is not something independent of reality: "To what extent our intellect is a consequence of conditions of existence—we would not have it if we did not *need* to have it, and we would not have it as

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it is if we did not need to have it *as it is*, if we could live *otherwise*" (WP §498). In other words, although existence may not be knowable to us in a perfect sense, it is nevertheless the fundamental determinant of how and what we know.

To grasp this point, we must turn to NIETZSCHE's concept of the "basic will to power" (WP §658). Under this view, the *cosmos* is not made up of either matter or energy, but the impermanent and everchanging condition of their interaction. Indeed, there exists "only dynamic quanta in a relation of tension to all other dynamic quanta" (WP §635). The 'world', he explains, is

a monster of energy, without beginning, without end; a firm, iron magnitude of force that does not grow bigger or smaller, that does not expand itself but only transforms itself... [It is] a sea of forces flowing and rushing together, eternally changing, eternally flooding back... with an ebb and a flood of its forms; out of the simplest forms striving toward the most complex, out of the stillest, most rigid, coldest forms toward the hottest, most turbulent, most self-contradictory, and then again returning home to the simple out of this abundance (WP §1067).

It is this complexifying-disintigrating force that NIETZSCHE calls the 'basic will to power', and because of it that the world's 'dynamic quanta' sometimes find themselves arranged into "complexes of events apparently durable in comparison to other complexes" (WP §552). NIETZSCHE is quick to point out, however, that these 'complexes' are not designed by an ordering entity. Indeed, he explains:

where a certain unity obtains in the grouping of things, one has always posited a *spirit* as the cause of this coordination... Why should the idea of a complex fact be one of the conditions of this fact?... We shall be on our guard against explaining purposiveness in terms of spirit: there is no ground whatever for ascribing to spirit the properties of organization and systemization (WP §526).

Instead, NIETZSCHE counts the capacity for undesigned complexity as a part of the 'basic will to power' and he maintains that it has incidentally resulted in the complex systems that we call living organisms; that is to say, life was the outcome of 'dynamic quanta' appropriately arranged (compare with PONNAMPERUMA/EIRICH 1990, KAUFFMAN 1996). Under this view, the 'will to power' is not something we *have* as living creatures, but something we *are*—"life simply *is* the will to power" (BGE §259) and "you yourselves are also this will to power—and nothing besides" (WP §1067). Moreover, all of your "organic functions," as well as "thinking, feeling, willing" can be "translated back to the basic will, the will to power—and understood as offshoots" (WP §658). Thus conceived, our knowledge is not something separate and distinct from the natural world, rather, it is an *instantiation* of it, or, following biologist Humberto MATURANA (1988), knowledge *in itself* entails a kind of knowing, it is a practical embodiment of information about the "domain of existence" and the "praxis of living" within it.

Second, NIETZSCHE agrees with the view that epistemology must take into account 'man as a knower'. Indeed, like Donald CAMPBELL (1974), NIETZSCHE seems to abandon the idea of "literal" truth, but recognizes the heuristic truth of a "hypothetical realism," the postulate of which is: there is an external world of objects and relations that exists independently of any knowing and perceiving organism, but that the knowing and perceiving organisms that inhabit this world have useful conjectural representations of it. Saying these representations 'work', however, is not the same as saying that they are 'true'. As NIETZSCHE explains: Many representations become

very well acquired habit[s] of belief, so much a part of us that not to believe in [them] would destroy the [human] race. But are they for that reason truths? What a conclusion! As if the preservation of man were a proof of truth (WP §497). Life is no argument. The conditions of life... include error (GS §121). All our organs of knowledge and our senses are developed only with regard to conditions of preservation and growth... [their truth] proves only their usefulness for life, proved by experience—not that something is true (WP §507).

In other words, although the products of our consciousness may 'fit' the external world and thereby improve our chances of survival, this does not mean that they 'match' the external world.

Third, NIETZSCHE argues that since our representations of the world are formed out of a need to eliminate the background noise of the world's 'flux' in order to make it comprehensible and useful to our preservation, it follows that our knowledge reaches only as far as our personal history permits. As a result, there are developmental constraints or 'boundary conditions' that are not externally defined, but which are accumulated in us as knowers; that is, our knowledge is 'path dependent'—future knowledge must somehow 'fit' within the genealogy which has come before. We find the same sort of limitation in

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biological evolution (WIMSATT 1986). Organisms do not adapt 'from scratch', but are constrained by their evolved genotype; i.e., acquired traits themselves help to define the parameters of future evolutionary trajectories. For example, as Oxford zoologist Richard DAWKINS (1996) has pointed out:

When land animals return to the water, why don't they recover the full apparatus of watery living? Why don't whales and sea-cows regrow gills and lose their lungs?... In evolution, ideal outcomes are not the only consideration. It also makes a difference where you start ... the ancestors of whales and sea-cows began, remember, close to an airbreathing [fitness] peak... All their internal details were geared to air-breathing assumptions. Perhaps they could have reformed them and come into line with the fishes, dusting off the embryonic vestiges of their ancient gills. But that would have meant a massive shake-up of their bodily infrastructure. It would have been equivalent to going down a deep valley... with the ultimate objective of climbing a slightly higher peak eventually (p132, emphasis added).

Fourth, NIETZSCHE rejects the idea that the development of our knowledge consists only of passively adapting or conforming to external circumstances. Rather, adaptation also includes processes wherein we achieve greater success by exerting control or 'power' over the external world. As he explains, "life is not the adaptation of inner circumstances to outer ones, but will to power, which, working from within, incorporates and subordinates more and more of that which is outside" (WP §681) and "the influence of 'external circumstances' is overestimated... the essential thing in the life process is precisely the tremendous shaping, form-creating force working from within which utilizes and exploits 'external circumstances'" (WP §647). Broadly speaking, this means that NIETZSCHE sees evolution as a process wherein the units of selection achieve greater success to the extent they are able to exercise 'power' over their environment better than their competitors. With this definition of adaptation, NIETZSCHE anticipates the evolutionary viewpoint summarized succinctly by Belgian cyberneticist Francis HEYLIGHEN

sense of an invariant system which changes its environment in order to cope with the strains it exerts. For example, I could adapt to an environment where there are a lot of wild animals by becoming stronger so that I could defend myself against any attack. But I might also change the environment by building a protective walls or poisoning the animals. Both are cases of adaptation, i.e., changing the relation between myself and the environment in order to enhance my chances of survival (p97).

Following this approach, NIETZSCHE maintains that our knowledge "works as a tool of power" (WP §480) or mechanism for controlling our sensory experiences. That is to say, it is not-as behaviorists arguethe environment that controls us as living organisms, but it is us as living organisms that control our environment through various cognitive representations of it. Knowledge, in this sense, is a "living control system" (POWERS 1973, CZIKO 1996) or a means whereby we exert power over our external world; i.e., knowledge is a natural life process involving interaction between an organism and its environment with relevance to the extension of its control.

Fifth, NIETZSCHE recognizes, like psychobiologist Henry PLOTKIN (1994), that knowledge "changes the knower and in knowing the knower alters the world that is known since knowing is always relative to a living system and never absolute" (p81). Indeed, according to NIETZSCHE, "coming to know means 'to place oneself in relation to something;' to feel oneself conditioned by something and oneself to condition it" (WP §555), and this conditioning presents the possibility of even newer knowledge---"to create new names and estimations and possibilities in order to create in the long term new 'things'" (GS §58). In other words, in knowing the world, we change the world, and this new world can come to change us, and so on. The same sort of 'feedback' phenomenon occurs in evolutionary biology. The most obvious example of this is the relationship between predator and prey (VAN VALEN 1973). When a prey improves its defense (i.e., rabbits run faster), this has a direct impact on its external environment because natural selection will favor those of its predators that have the best

(1990):

I use the word 'adaptation' here as well as in the sense of a system which changes its internal structure in order survive in an invariant environment, as in the

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countervailing offense (i.e., the fastest foxes). However, the differential improvement of the predator's offense (i.e., foxes running faster) will then have a direct impact on the prey because natural selection will then favor those of them

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that have the best countervailing defense (i.e., the fastest rabbits), and the cycle continues.

Lastly, unlike Karl POPPER (1983), NIETZSCHE does not conclude that our representations of the world will progressively converge upon the 'truth'. Indeed, appealing to Harvard University paleobiologist Stephen J. GOULD (1989), consider the following metaphor: Suppose we could clone an ecosystem many times over. We would not expect that after one million years the systems would look alike. In each case the organisms in the system would be subjected to unique contingencies and historical accidents. Consequently, there would be no convergence upon a single perfect form-each ecosystem would be *different*. In the case of knowledge, contingencies and historical accidents would have the same effect. As a result, our knowledge too is not leading to anywhere in particular. There is no telos. Evolution, in this regard, is not to be conceived as a means to an end, but rather as a "pathos" (WP §635), or fundamental tendency, present at every moment in the unfolding process in which life and mind exist-"and the process goes on" (WP §636). As NI-**ETZSCHE** explains:

What, after all, is "useful?" One must ask "useful in relation to what?" E.g., that which is useful for the long life of the individual might be unfavorable to its strength and splendor; that which preserves the individual might at the same time arrest and halt its evolution. On the other hand, a deficiency, a degeneration, can be of the highest utility in so far as it acts as a stimulant to other organs (WP §647). [Thus] "useful" in respect of acceleration of the tempo of evolution is a different kind of "useful" from that in respect of the greatest possible stability and durability of that which is evolved (WP §648).

In NIETZSCHE's view, in other words, evolution (cognitive or biological) is an essentially transformative process, but in no *specific* direction (see SCHACHT, p246–247). The only sort of 'utility' associated with it has to do with the *context* in which it operates. It does not refer to any end or 'final cause'. Accordingly, concludes NIETZSCHE, our knowledge "is a falsehood always changing but never getting near the truth: for there is no 'truth,'" no unchanging, static reality (WP §616).

Notes

1 Book and essay titles by NIETZSCHE are abbreviated throughout this paper as follows: *Beyond Good and Evil* BGE; *Daybreak* DB; *Ecce Homo* EH; *The Gay Science* GS; *On the Genealogy of Morals* GM; *Gesamelte Werke* GW; *Human, All* *Too Human* HH; "NIETZSCHE Contra Wagner" NCW; "On Truth and Lie in an Extra Moral Sense" TL; *Thus Spoke Zarathustra* TSZ; "The Wanderer and His Shadow" WHS; and *The Will to Power* WP. All emphasis is original to NIETZSCHE unless otherwise noted.

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The Glory, Decline and Return of Typus

On the Adoption and Usage of Concepts of Typus in Scientific Philosophy From 1800 to the Present

Part 1: Attitudes to "Typus"

Preliminary remark

It was written repeatedly that typologies have spread in science and philosophy from 1850 on up to now. However, there is neither a bibliography across the sciences nor a systematic comparative analysis of Typus concepts and benefits of using Typus, above all in the various sciences.

In 1995/96, a methodological study of Typus in urban architectural history was embarked upon. In the process some general trends were found from the source material which might be interesting for science history of the 19th and 20th century. These trends refer to diachronic–synchronic developments as well as structures of scientific cognition. As regards con-

Abstract

Different and competing concepts of the model, type or stereotype (henceforth referred to as "Typus" for clarity) emerged in biology between 1750 and 1850. Natural and artificial systems of classification were tried out by botanists and zoologists. Several of these "Typus" classifications proved to be mere fantasy. The widest approval has been accorded up to now to two concepts: the pattern, or "Bauplan", of related animal organisms and plants, called the "diagrammatic Typus" in botanics.

At present, typologies either play no role in the sciences or—as in many cases—have been taken back to the sector of primary heuristic. Always controversial, however, is whether they are epistemologically justified and efficient. Only zoology and botany are still working successfully with the "Bauplan". It has an extremely high predictive power and it concurs with other theories in biology to a great extent. This fact is unknown in other sciences..

The following text supports the theory that the biological typology was scarcely accepted beyond biology. It is expected that the discussion on the criteria of Typus in biology will help to develop basic terms for Typus in other sciences dealing with legalities concerning construction.

Key words

Typus, "Bauplan", classification, homology.

tent the writer was interested in transferring theories from one science to another, more exactly in the adoption of subject-specific problems and solutions in a science aiming at solving problems in other sciences. A concrete impression has emerged that the adoption of the Typus conception from the philosophy of science has created far more new problems in group of successful research morphologists. In theoretical biology which has developed recently it is mentioned that the methodological level of biology was low until 1950. It is remarkable that philosophy and the sciences were using terms for more than 100 years which were not part of their sector.

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several sciences instead of solving current problems. After a period of euphoria between 1900 and 1950 deep disillusionment has arised regarding the efficiency of typology concepts.

It is easy to realise that a group of philosophers dealing with the Typus in the 19th century had little knowledge about botany. They regarded the Typus as a rebel against the established class logic but it is possible that they did not know the homologytheorem of biology. This is because their discussion began in 1840 and the homology-analogy study of Richard OWENS was published in 1848. Besides that, competing and conflicting concepts of Typus in the biological sciences might in fact be the reason why other sciences did not learn very much from the

Period	Sector of usage and meaning				
World of Ancient Greece 500–150 BC	Bronze founding typoz = hollow form, impression of a hollow form typtein = beat				
	Philosophy T= archetype = picture = ideas of the things (PLATON)	<i>Arts</i> T= relief T= formless shape T= seal impression		<i>Literature</i> T = type of a comedy figure = a rough sketch (ARISTOPHANES)	<i>Medicine</i> T = regular course of a disease
Roman ancient world 150 BC–300 AC				Literature/Arts T = figure/picture (CICERO) T = ? TACITUS T = ? PLINIUS	<i>Medicine</i> T = unchanged
Christian Middle Ages 800–1500 AC	Philosophy Scholasticism New PLATONISM T = archetype = idea of the things (Universalia a re)		Philology Bible exegesis Typology: T = event in the Old Testament Anti-T = Christ		Medicine T = unchanged
Early Modern Age 1500–1800	 the language encyclopaedias of around 1600 include the word meanings of Ancient Greece worl again, the Christian typology is pushed into the background French everyday language type 				
	Pattern (1) T = picture/archetype Example = model carpet	t	Pattern (2) T = "Grund Example =	lform" (basic form) carpet pattern	

Figure 1: Etymology of the word typoz = typus = type = Typ(us) initial usage and development in meaning by 1800.

Source material and aim

Literature was analysed systematically and discursively. Bibliographies, encyclopaedias, monographs and surveys were examined in order to find methodological contributions. Those essays and subjects were selected which are always considered as the "essential" ones in literature. Thus, main and secondary discussion sectors emerged. From the beginning, one particular question was examined: the writer has known a lot about the problems of typology in biology for many years (GOETHE, REMANE, RIEDL, numerical taxonomy–homology) and now, he has had to pursue the discussion within biology in a wider perspective.

Concerning the structure of the study, the analysed source material is given and discussed in more or less formal-systematical periods. At the beginning, the etymology of the word "Typus" until 1800 is presented. At that time, the word became a specialist term. Afterwards, the origin of the term is introduced as well as its adoption to the sciences and philosophy. Finally, several general instructions regarding the future development of Typus in urban architectural history are given.

Initial usage

All encyclopaedias consulted emphasize the different meanings of Typus arising since its initial usage we know. It is undisputed that the word has its origin from the Latin word "Typus" and the Romanic-Germanic word "Type" as well as from the Greek word "Typoz". "Typoz" is normally connected with the Greek word "typtein" = beat. At the very beginning, the word "Typoz" was used in bronze founding. It could mean a hollow form, relief, impression of a hollow form, impression of a seal. Moreover, it was used for a hulking great shape (a statue being in its form, for example). Thus, it can be seen that the initial usage of "Typoz" was confined to the sector of shape and form. Above all, it described a reproduction, something that was shaped ("Typoz" as an impression of a seal) as weil as a picture, a model ("Typoz" as a hollow form). The encyclopaedias do not approve the devaluation of the word if it is used only for shape (reproduction) instead of being used for the whole thing and the positive valuation if it is used as a model (example) in contrast to the insignificant part of the thing.

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History of the word up to 1800

It can be proved that the Greek word "Typoz" already had different meanings in different areas of life. However, it did not achieve the status of a specialist term. The Greek philosophy uses "Typoz" in the sense of picture = model, PLATO considered ideas as the types of sensorial things. The medicine of the ancient world spoke of the regular (= typical) course of diseases. In literature, the term "typology" meant a means of style in order to demonstrate repeated obvious characteristics of figures in comedy. In classical Latin, CICERO and PLINIUS used "Typus" in the sense of figure and picture, and in historiography, TACITUS is supposed to have used the word. In the Middle Ages, the meaning according to PLATON in the sense of archetype, model was permanently existing. In biblical exegesis, the meaning of "Typus" as example in the sense of the Christian typology was prevailing during the whole Middle Ages: several selected events in the Old Testament (e.g., Moses' exaltation of the snake in desert) became types = examples and forerunners, and Jesus Christ's personality and his action were the antitype (antithesis) of those examples. Typus was connected to the "Antitypus": the "Antitypus" was its fulfilment and completion. Thus, the two terms are related both temporally and hierarchically.

In the 17th century, the word "type" arose in French for the first time. It had a strange double meaning due to the PLATONIC way of thinking, similar to the German word "Muster" = pattern (see figure 1).

Part II: Usage and spread of Typus in Science and Philosophy from 1750 on until the Present

1750 fo 1850

Biologists of the 18th and 19th century examined vegetable and animal organisms, living and extinct species and organism elements. From approx. 1750 to 1848 different methods and classification methods were tried out in order to find laws in formation. At the beginning, the biologists assumed that there was a simple and general pattern to the animal organisms (T1). From this assumption GOETHE developed a methodological procedure in order to distinguish orders of similar patterns which are related with the help of Typus (T2).



Figure 2: A closed field of divergent similarities, with the example of the hand skeleton on the left, of some living mammals and some fossilized mammals. The field is divided into orders according to the change of the similarities. Now in order to comprehend the definitive "Typus": the homologous feature skeleton is in the hierarchical order of basic homologies (calliper) in the same location in all species. It is always composed of three groups of features (correlation) in always the same order (connection). The unity of the Typus does not depend on the fact that the functions of the homologon change and the single features vary regarding number, size and extent.

As morphologists searched for patterns (Baupläne, T3), the analysing methods became more precise over a period of two generations. At first, it was observed that the position of the particles was significant (T5). Finally, the differential diagnosis was established (T6): similarities (analogies) were to be distinguished from family (homology) and functions were to be distinguished from structures. Reformulated from the perspective of the biological hermeneutic: The morphology presented a method to separate the common visual features from the common family features, whether similar or not, in order to separate the older phylogenetical visual assessment of build from rational assessment of build. Zoological families have concrete forms which can be ordered in a natural system (see figure 2). The ontological status of morphological types remained controversial for more than one century. Challenged by a numerical taxonomy, theoretical biologists had justified the results as well as the method epistemological sufficiently between 1950 and 1975.

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Figure 3: Diagrammatic Typus and individual forms of orchids (Monodria). Arounds the middle, different flowers of European orchids are shown (according to HEGI), in the middle the diagrammatic Typus which is basic to all flowers.

Botany followed another path. At first, an artificial division system was successfully tried out, though the researchers probably suspected that a natural one could be developed. GOETHE tried to solve that problem by the theory of a prototype (T2), but later, it was realized that natural families of plants can only be described by the "diagrammatic Typus". This is a graphic abstraction which cannot be demonstrated in its real forms (see figure 3). Besides that, phenomena of passing change were considered as very interesting by botanists. DE CANDOLLE (1813) dealt with the families of varities and special forms of the regular species and formed corresponding orders.

A representative was sought showing the common features of most visible or exemplary order (T4). This representative was called developed form or representative. The aim to find families could not be reached. It was only possible to clearly order similar passing phenomena.

1840 to 1900

The paths began to separate (compare figure 4). Many sciences were interested in Typus concepts long before 1840 according to CUVIER, GOETHE or v. Humboldt—the graph does not show this. In 1840, however, William WHEWELL caused a momentous philosophical debate as he introduced the Typus into the discussion as a contrast to inductive and deductive logic in the sciences. He understood Typus as a "pattern for a natural group: a species of a genus which strikingly shows all the characteristics of this genus. This species is not on the fringes but in the centre of a genus, and around it is a group of different, related species. Three years later, J. S. MILL reacted with a polemical reply: "Natural groups are divided according to Dr. WHEWELL through the "Typus" and not through definition. The class is noticed but not exactly confined; it is fixed but not exactly described; it is defined not by an outer border but by a central point; not by that what it excludes rigidly but mainly by that what it includes, by an example, not by a rule; briefly, we have a "Typus" instead of a definition as a leader." (MILL 1877, p155) MILL put forward that each class, whether natural or artificial, was defineable due to common features. Borderline cases could be ordered to a certain class as well if the class-forming characteristics were cleverly selected. The Typus was a pattern, an example showing all the characteristics of a group to a great extent. However, it should not repiace a definition but explain it. The Typus could be used as a means in order to identify the varieties of a class. But it was mainly a representative for assigning concrete objects to a class.

This controversy inspired a debate for the subsequent decades—one could even say for more than a century. The philosophers ERDMANN, SIEGWART and LOTZE agreed with WHEWELL, while the psychologist Wilhelm WUNDT (1880) took the role of mediator tolerating two kinds of Typus:

- 1. Typus as the most simple form of law concerning structure or composition.
- 2. Typus as a form representing the characteristics of an order of related forms most completely.

The focus of this debate was the "family" or relative Typus versus definition, varying features versus clinical classification, even finally intuitive feeling versus clear rationality. Biology did not face the problem in this way. The Typus can be considered neither as a purely phenomenological similarity nor as a representative of a rational construction. The Typus is a material form, regarded as a genetically fixed order of recursive causality.

Ever since Richard OWENS (1848) had separated homologies and analogies, there had been a strategy in order to link (intuitive) shape cognition and (ra-

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Period	Sector of usage and	d meaning				
Approx. 1750 to 1848	 Biology/Zoology/Botanics T1 = "un dessein primitif et general", "unité de plan" (BUFFON 1753) T2 = morphological type = law = idea "unite de type" "Ur-Form" (GOETHE from 1795) T3 = Bauplan (pattern) (Vicq D'AZYR 1805) (BLAINVILLE 1806) T4 = Vollform (= fully developed form) = representative = exemplary form = guide-line (A.P. de CANDOLLE 1813) T5 = Relation of elements (V. BAER, 1828) T6 = Typus = Homology (R. OWEN, 1848) 					
Approx. 1840 to 1900	PhilosopyT = a representative of severalsimilar objects who represents common featuresmost distinctively (WHEWELL 1840)T = a representative of a class of characteristics =explanation of a definition (J. ST. MILL 1843)T (natural sciences): ordinary typeT (cultural sciences): the individual, exemplary,outstanding (H. RICKERT 1920)			Science Palaontology (CUVIER) Geology (A. v. HUMBOLDT) Climate science (KÖPPEN) Typology theory of organic connections (GERHARDT) Physiology Crystal science Molecular biology Language history		
From approx. 1900 on	Political science C. Menger	Psychology E. Kretschmer C. G. Jung A. Jelinek	<i>Socioloqy</i> M. Weber A. Weber	History Otto Hintze A. Toynbee O. Spengler	Literature and Art Science Wölfflin G. Müller	
From 1950 on	<i>Biology</i> Bauplan (=pattern) Diagrammatic type Systematic type Generalised type Life-form type	Theology/ Phil Typology	osophy	<i>Sociology</i> Ideal type = extreme type = Utopia	<i>Cultural sciences</i> Text types (linguistics) Building type (archi- tecture)	

Figure 4: Usage and spread of Typ(us) in science and philosophy from approx.1750 until now

tional) scientific knowledge of the shape. A Typus is, irrespective of the degree of complexity, defined by the same number of comparable features in the same location; that means the Typus connects elements of a class logic ("same" number, "same" location of features) with the logical conditions of relational terms of order, like the same "number", the same "location", "comparable" features.

1900 to 1950

In 1900, the Typus, coming from the controversy in the philosophy of logic, began to spread in many disciplines. This summary is confined to two sectors, sociology (1) and psychology (2).

(1) In the 1880s, Christian MENGER introduced the Typus into what was called political science at that time; to be discovered by Max WEBER for social history. The historian Otto HINTZE (1962) picked it up

from WEBER for his history of the corporative constitutions in Europe. MENGER (1883) distinguished between real types and rigid types. In his opinion the real type was an abstractive basic form of real appearances, and the rigid type was fixed with rigid terms and laws regardless of its empirical connection to reality.

Taking the Typus definition of Karl Marx into consideration, Max WEBER (1922, p93) formed the ideal Typus out of the rigid Typus. The reason for his development of the ideal Typus was, according to WEBER, the fact that the usage of class terms and laws in historical explanations was either wrong or trivial. His ideal Typus was "a means for describing historical single appearances in order to form clearer connections without contradictions which seem to be objectively possible to our imagination and adequate to our knowledge." (WEBER 1922, p75) This explanation is shown in the works about historical

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constitutions by the historian Otto HINTZE (1962) which are still today considered as exemplary in the study of history. HINTZE created the idea of this type of constitution from all the cases of so-called corporative constitutions he knew, and distinguished between this type of constitution and the ideal type of democracy and of feudal system. He thus formed a concept for demonstrating the historical-systematical history of all constitutions. It was stated in the sociology of the time: "(ldeal-) Types are utopian and are perhaps not useable, but cannot, however, be disproved empirically." (HINTZE 1962, p113) After WEBER had established this ideal type, a discussion about the logical special status of ideal types started. Critics thought types should be divided either into classes (KEMPSKI 1951) or into the logic of order relations (HEMPEL 1936). Ideal types, however, could merely be described as theoretical models, most lacking in certain areas of application. The supporters considered constructed types as the only relevant generalizations in social sciences. Popper called them situation-logic explanations; they would be more than simplified but perhaps better than other explanations. J. E. HEYDE (1941), however, wrote that the ideal Typus would be ideal only due to the intensity of its characteristics which were without exception real.

(2) In psychology, the Typus became very significant from the research "Physique and Character" of KRETSCHMER, ACH, LERSCH, the JAENSCH Brothers as well as Eduard Spranger, Karl Jaspers and C. G. JUNG with different evaluations. These are only the most well-known researchers. Ernst KRETSCHMER (1961), probably the most significant personality of the project, emphasized repeatedly that his research works were purely scientific. In 1936, Carl HEMPEL and Paul OPPENHEIM checked the psychological typologies as to their logical status, irrespective of their contents. They came to the conclusion that types were either constructed according to the logical pattern of the traditional class logic of ARISTOTELE (genus proximum and differentia specifica) or were patterns showing groups with the same features of gradual change. Furthermore, these patterns could be explained properly by a new logical method for terms of reference which had been developed as relational logic in logistics.

In 1947, the American W. McDougall published an essay condemning psychological type systems: "The search for such types has no rational base and often opposes probabilities which lie at hand." (Mc-Dougall 1947, p24) Personalities like JUNG, KRETSCHMER or Spranger managed to help a hopeless and badly-phrased question to fame and glory merely because of their rhetorical elegance and their reputation in the sciences. As a reaction to that, Kurt STRUNZ (1952) examined the psychological typologies from a methodological point of view in 1950. The aim of the research was to find personality types. He noticed complexes of visible and cognitive features classified either as units, systems, groups or aggregates. As a result, he distinguished between two main methods of fixing Typus:

- 1. The empirical synthesis and its causal explanation
- 2. The psychological–constructive total classification.

We restrict ourselves here to a discussion of the former method. The base of the method are complexes of features existing in reality. The more common characteristics are distinguished from the occasional characteristics and general terms are established. Thus, an empirical general term, a class type, is achieved. At this stage, no exact empirical limit can be fixed according to STRUNZ. Afterwards, further features, beside the principle features, are identified, mostly by percentage calculation and correlatory examinations. These affinities which were discovered by purely statistic-empirical methods, were interpreted afterwards in causal analysis terms. The final result is, according to KRETSCHMER's terminology, "the maximum correlation concentration". STRUNZ meant the problem was whether this doctrine was verifiable in reality. A class term with less characteristics always included a number of individuals, and the concentration applied merely to a few real cases.

1950 to 1975

In the years 1951 and 1952 several famous representatives of the sciences and one philosopher were requested by the Swiss journal "Studium Generale" to judge the results of the "Typus" researches academically. The zoologist Adolf REMANE (1951) presented a brief description of the three main criteria and the three secondary criteria which define the "Typus" as homology. He also pointed out that the species "Typus" connected with the systematology of the natural system was a means of exceptionally high prognostical value. REMANE dealt with the "diagrammatic Typus" in detail in order to show by this example that the most difficult thing of all, not only in botany-would be to demonstrate the order which is of an inside similarity = homology. Besides those central Typus concepts, REMANE discussed the

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efficiencies and limitations of the life-form Typus and the generalised Typus (phylogenetically interpreted type order). The botanist Wilhelm TROLL (1951) in his report published soon afterwards discussed the general scientific intention of biomorphology. Biomorphology could be defined as science of shape. "As a biological discipline, it deals with the structure of living things and the great variety of forms in nature which exists as simultaneous variety. In comparison with that, we could speak of a successive variety regarding those phenomena which arise frequently in nature. Science is committed to finding the constants underlying processes in nature. Biomorphology examines the question of whether there is unity in the variety. Obviously, this unity must have the character of a shape as well. It is the unified shape of the variety of forms for which the term 'Typus' was established." (TROLL 1951, p376)

TROLL discussed in detail phenomena of overlapping of types as well as the fact that huge species groups (approx. 17 000 orchids, for example), can be reduced to the 15 general types as a group of Typus apostates maintained. TROLL considered the underlying forms of the order not as evolutionary epiphenomena, but the results of forming orders, and the way of forming was not yet understood sufficiently.

The sociologist Jürgen von KEMPSKI insisted on dispensing with typologies completely, and the historian Bernhard ZITTEL did not want to allow typologies with natural scientific character—he meant ordinary types—at all. In the heuristic sector, typological rough classifications had proved themselves (Middle Ages, Gothic, Revolution) but in other sectors no typologies could be used as far as the report of the history of unique events and of the actions of great personalities is concerned. It may be added that this opinion reflects Leopold RANKE's attitude but not Otto HINTZE and Arnold TOYNBEE.

Ernst KRETSCHMER and the previously mentioned Kurt STRUNZ pointed out the efficiency and the limitations of the psychological Typus research which had scarcely been criticised hitherto. The architecture historian Werner KNAPP discussed, using the example of castle research, that it was possible to write a historical typology of the Middle European castles without idealising or murdering the facts if sufficient sources were available and the different influencing factors were evaluated carefully. KNAPP's methodological principle: At first, it is necessary to realize that a building is an expression of some functional context in order to avoid overestimating some features or making them absolute or neglect-

ing them. He continued: "It must be established in which order of castle-building has developed in western world. The castle must be considered as an expression of this order, and its special forms which have changed in the single epochs of the complete development and their overlappings must be recognized as its pattern, its Typus due to their uniformity in shape. This Typus must be described simply. If it is possible to investigate such development of forms, it must also be possible to discover special forms from each epoch which reflect the order of the culture of each epoch in every respect. We can assume that such development of forms existed in each epoch because of the cultural heritage of the epochs. These special basic forms can rightly be called types." (KNAPP 1951, 219)

Significant to this type variety is the philosophical work of Joh. E. HEYDE (1952). His subject is the "Typologetik": the reflection to that what the Typus really is. HEYDE discussed mainly two subject complexes:

- 1. What distinguishes typologies from classifying terms?
- 2. Are types occurrences of the real world or are they order categories?

Both questions run not only through HEYDE's text but are topics arising repeatedly in typological literature. In this study and his previous study of 1940 he presents a thorough history of the word "Typus". The meaning of Typus = basic form, pattern, nature of the things was already well-known in the 17th century (see figure 1). Out of this meaning the biological Typus = "Bauplan" (=pattern) developed and afterwards, the botanic Typus = developed form, representative, model acquired through specific examinations of the so-called periphery of species. And the discussions in philosophy and among the sciences about Typus since 1840 had only dealt with the basic form and the developed form (see figure 5). It remains to be seen whether this opinion must be corrected by a historical-systematical examination of all typologies. In psychology, at least, we can consider it as justified in view of the fact that Wilhelm WUNDT (1883) allowed only these two Typus definitions and HEMPEL/OPPENHEIM (1936) reduced all typologies existing at that time to two logical basic forms: the classificational and the relational-Typus.

The question to what extent the "terms" classificational Typus and relational Typus refer to biological types of regular shape was not asked by HEYDE (1951). It is curious that he did not examine the basic form, the developed form and the "Bauplan" systematical comparatively in an ontological,

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Figure 5: Diagram of the historical development of the term according to Joh. E. HEYDE. In philosophy and some sciences the "basic form" (average) and the "developed form" (ideal type) have been recognized, but their status in the individual sciences are still controversial.

epistemological and methodological way as he knew much about the historical development of the term "Typus".

In 1953, August SEIFFERT dealt with the problem "Typus" again referring directly to the reports in "Studium Generale". He was well-informed about biological and chemical typologies as well as about the discussions on Typus of historians, sociologists and psychologists. He wanted to dig the debate out of the rut of well-worn ways of thinking in order to encourage new research. He developed a wide horizon of arguments from history of philosohy and pointed out that recognition of nominalism, realism and empirism had already developed in the Middle Ages and their claim to be radical was disproved by the history of adoption: each position implicitly uses the point of view it excludes explicitly. Against this background, he suggested a centre position: It should be kept open whether there were orders in nature or only in thinking and whether there were only single things or also universals. He concluded with some general theories:

- 1. In practical research you can look for and find peculiar fixed forms widely differing from classification which you can consider as independent in spite of being integrated in a network of conditions and causalities. Independent means independent in the real context and with regard to human understanding and usage. Those fixed forms are called "types".
- 2. From an epistemological point of view, you can realize that type research in the sciences and the fixing of types is always accompanied by a myste-

rious intuition probably existing in principle. This intuition also causes objectives of types (which are permanently changing, of course) during research.

- 3. The independence of the Typus becomes conspicuous and obvious. Unfortunately, types were often long pursued which finally proved to be a fantasy. That shows that the intuition is often deceptive. The sheer variety of competing and often paradoxical—hence incompatible typologies—is evidence for that.
- 4. Unusable typologies prove to be psychological standpoints or projections: such fantasies might be an encouragement for branding all types as subjective or fictitious;
- 5. The fact that it is not possible to fully unite the real thing with the "mental-theory" in ontological research also applies for types: A centre must be postulated, but it is abstract.
- 6. There are considerable difficulties in examining the types with their strange blurred and vague outlines. A suitable method must be developed.

SEIFFERT pointed out that the number of types and typologies had increased, especially in biology, chemistry and psychology, and he expected that these fixed forms would find increasing acceptance. The fact that HEMPEL/OPPENHEIM (1936) developed the classificational logic to a relational logic of type is positive proof for SEIFFERT that practical typeresearch is getting increasing methodological support.

SEIFFERT (1953) selected the "nimble family of gazelles" as his Typus example of biological Typus and one observes that he identified REMANE's description

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of the species type purely logically as classification. He only considered the variations within a species as types.

In 1973, 20 years later, the discussion about types was recharged by Gerhard TERTON (1973). He made a preliminary study for a planned systematical–comparative examination of type terms in the sciences with the aim of developing a general Typus methodology, analysing the logic of typological explanations as well as dealing with the relations of certain typologies to theories.

Against the background of SEIFFERT's study, it followed to question the efficiencies of the term "Typus" and its form within the sciences. TERTON then analysed the biological contributions about the morphological "Typus" and concluded that this Typus would be very significant as it had an exceptionally high prognostic importance in practical research and apart from crystalography, in no other science has such a high level of theoretical justification of typology been reached as in morphological biology" (TERTON 1973, p258). Neither the traditional logic of terms ("its epistemological thinking is too general"; "the underlying abstraction theory cannot accusately describe the actual processes of forming terms in the sciences", TER-TON 1973, p257) nor the new relational logic ("it only demonstrates that, concerning the ideal type, the typological term does not exclude the metrical term", TERTON 1973, p252) were suitable to comprehend the morphological Typus, in view of its features as well as in view of its classification in a term system.

Compared with the traditional logic of terms biology had, according to TERTON, rigorous demands as far as the characteristics belonging to the morphological Typus are concerned. Having been tried out over a long trial period with numerous roundabout ways, the method was accepted in biology. For the method of homologizing which required taking the main criteria and second criteria into consideration, it was necessary to clearly distinguish between functional and structural features. The claim that two compared structures corresponded homological, demanded a clear decision instead of a loosely-defined "more-or-less" Typus. The features were compared at a high level of abstraction, so that concrete differences in construction! such as height, width, length etc. did not have to be taken into account any longer

when establishing a class of homologies, but an exact location of the characteristics with regard to correlation and connection was required. Already included was the coupling of features demanded by HEMPEL/OPPENHEIM for the multi-dimensional Typus.

First examined was the species Typus which was to be understood as a Typus unit, that means as a unit class of many homologenous organism structures arising together. "From the logic of class it can be regarded as a unit class of homology classes." (TERTON 1973, p253)

Homologies arising in several species were ordered to a system and the border of this system were homology classes. This led to a hierarchically constructed type system which differed in its logical construction from other classification systems in the fact that the categories could not be fixed arbitrarily but were fixed exactly and were extraordinarily meaningful. The result of this was the high predictive power of the types: "If an individual of the examined sector belongs to any class of the homologenous characteristic 'Mi', it belongs, as a rule, to each other homology class of this sector." (TERTON 1973, p253).

Though it was not possible in some cases to describe a Typus with a so-called Typus formula, there could be no objection to its class-logical version. TERTON then discussed in detail the question of whether the biological type was only a visual idea, and he refuted this. Typus concepts which were established for order problems within species (anthropology, physiognomy and research of constitutions) resulted from basing a principle of selection which was not oriented to homologies and integrating empirical images by mere perception in the way that the features arising frequently were intensified while the other features became unclear. However, compared with the biological Typus it was only possible to fix that which could be seen directly.

Part III: Summary

In summarizing the discussion of more than hundred years with regard to the knowledge about Typus gained in biology and the tradition of the general comprehension of Typus existing in the scientific philosophical debate there can be found concrete indications to the fact that there were several different basic assumptions about the nature of Typus.

1. In biology, the order patterns of lawfully together

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Manfred Eickhölter, Buddenbrookhaus, Heinrich und Thomas Mann–Zentrum, Mengstr. 4, D-23552 Lübeck, Germany. arising features existing in nature are not intuitively regarded as types. There are no empirical visual patterns of similarities either. The perception of forms becomes

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cognition of forms when a distinction evaluating qualitatively between degree of similarity and sort of similarity is made. Part of this distinction is a rigid separation of functional and structural characteristics.

- 2. The biological Typus is no phenomen of ordered form arising in isolation but each Typus is part of a hierarchical system of types while being itself a whole which unifies a great number of types in a Typus unity.
- 3. Regarding the biological types, it cannot be justifies to distinguish between classes and types in the sense of a formal logic. The Typus is a class of homologies, and its features are on the one hand, defined very strictly and rigidly, and on other hand, form a construction of relationships and connections at many levels. Only against this background do the discussions about passing change within the sciences make sense.

Finally, it must be noted that the methodological research of Typus in theoretical biology was intensified after 1975 by the works of WAGNER and RIEDL. Then a theoretical explanation of evolution regarding the orders of Typus in nature was worked out. It pointed out that the biological Typus described those characteristics of form which could not be realized in other ways. At the same time, the research and establishment of the Typus in biology was methodologically reconstructed. And in terms of evolutionary recognition the theesis was at last put forward that the all-too human compulsion to find permanently existing forms was a cognitive achievement of adaptation of orders of forms in nature. This theory explains why the biologists managed to identify more than two million species in the short period of 200 years, and why many forms are incorrectly considered as types in biology.

Part IV: Perspective

As previously mentioned, the writer began this work with the intention of intervening in the debate about the Typus in the research of urban architectural history. It should be demonstrable by way of a few indications that it is necessary to discuss type terms in architectural history and urban history again. Bibliographical lists show that Typus terms are frequently used in this sector of research That is not surprising as respected introductions to the "Historische Hausforschung" (BEDAL 1978) or to "Die kleine Kunstgeschichte der mittelalterlichen Stadt" (MECKSEPER 1982, p107–108) demand typological preparation before any further analysis. Ernst SCHIR-MACHER (1987), however, complains that the term "Typus" has been discussed in the research of urban history for more than 100 years without any result. He tried to clarify the term in his well-reviewed book "Stadtvorstellungen". His aim is to see types not only as a heuristic too but as "real", "historical" types. Like the above-mentioned researcher of castles Werner KNAPP, SCHIRMACHER is convinced that types of construction are a functional expression of standardizing orders, "urban images", characterizing the image of a town for a period of time. However, the criteria of Typus introduced by SCHIRMACHER to the discussion are orientated towards the above-mentioned scientific-philosophical discussion and not towards the successful usage within one science. He uses terms like similarity and family without knowing which methodological function they have in biology. Thus, he might not be in a position to judge the chances of success the usage of the Typus could have in the study of the constructional order of towns.

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An Inquiry into Meno's Dilemma

Introduction

One very old problem in philosophy regards the discovery of new knowledge, that is, our ability to acquire knowledge that we do not already possess. This problem is known as MENO's dilemma after one of PLATO's dialogues where the subject was raised more than two millennia ago. Even though the problem of discovering new knowledge does not directly refer to first person experience, the

Abstract

Knowledge may be treated analogous to evolutionary processes. This view of knowledge has certain advantages compared to more traditional views. In particular, it allows the knowledge to be extended without the involvement of a separate agent. This means, in turn, that the problem of knowledge acquisition, known as MENO's dilemma, is avoided. The purpose of this paper is to discuss how an evolutionary process can accomplish this and to demonstrate the presence of an evolutionary process in the individual brain.

Key words

MENO's dilemma, evolution, learning, evolutionary epistemology, variation.

problem itself suggests that there is a *subject* (i.e., a first person) searching for knowledge. This is implicit in PLATO's formulation of the problem:

"How will you look for it, Socrates, when you do not know at all what it is? How will you aim to search for something you do not know at all? If you should meet with it, how will you know that this is the thing that you did not know?

"I know what you want to say, MENO. Do you realize what a debater's argument you are bringing up, that a man cannot search either for what he knows or for what he does not know? He cannot search for what he knows–since he knows it, there is no need to search–nor for what he does not know, for he does not know what to look for." (PLATO 1981).

In short, we may define the problem this way:

MENO's dilemma: We cannot learn what we do not already know because we are unable not only to search for it, but also to recognize it should we stumble on to it.

Formulated this way, learning does indeed appear paradoxical. I believe the reason for this appearance stems from the presence of an active subject searching for the knowledge. It seems as if the subject has a choice of either searching for knowledge or to refrain from doing so. If it does not search, it will not find. But if it does choose to search, there is no wise method of going about the business. "In going beyond what is already known, one cannot but go blindly. If one can go wisely, this indicates already achieved wisdom of some general sort" (CAMP-BELL 1974, p57). Reasoning, therefore, which is a wise method of obtaining seemingly new knowl-

edge, is nothing but an exploration of what we already implicitly know. The discovery of truly new knowledge needs another explanation.

In this paper I wish to challenge the active role of the subject in the quest for knowledge. Instead of putting all the burden on the subject alone in the search for knowledge, I will suggest a participation also from the environment in which the unknown resides. In doing this, we must give up the distinction between the subject and the knowledge that it is traditionally assigned to possess. That is, the subject and the knowledge are to be treated as two inseparable aspects of the same thing. Thus, we do not need to abolish the idea of a subject, only its separation from its knowledge.

The benefit of adopting this kind of view on the subject and the knowledge, is that it allows an evolutionary approach to MENO's dilemma. That is, evolution considered not primarily as a theory for the emergence of the species, but as "a *metaphysical research program*–a possible framework for testable scientific theories" (POPPER 1976, p168). Using this framework, I will argue that the brain (or the mind) can avoid MENO's dilemma in the same way as biological evolution avoids the neces-

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sity of a pre-existent creator. That is, to utilize noise or a variation mechanism to make new discoveries. This is in line with BATESON's statement that noise is "the only possible source of *new* patterns" (BATESON 1967, p416). To stress the similarity between the two problems concerning the origin of new knowledge and the origin of new species, let us invent a second dilemma that we may call "DARWIN's dilemma."

Darwin's dilemma. An evolving structure cannot adapt to an unknown environment, because it does not know what to adapt to.

The strategy of the paper is to start from DARWIN's dilemma and the problem of the origin of the species and then step by step move towards the problem of knowledge discovery as it applies to the brain. The path to be taken is as follows:

- 1. Biological evolution escapes DARWIN's dilemma.
- 2. Biological evolution is a knowledge process.
- 3. Thus, evolution escapes MENO's dilemma.
- 4. To deal with the individual knowledge process, we need a faster source of variation.
- 5. Spontaneous neural activity provides this source.
- 6. In this way, the brain escapes MENO's dilemma.

From Darwin to Meno

Before we begin, it is necessary to say a few words about the concept of knowledge. This concept has been subject to debate ever since the dawn of philosophy. I do not wish to enter this debate in this paper but merely follow the tradition of evolutionary epistemology founded by POPPER and CAMPBELL (see RADNITZKY and BARTLEY 1987). What we will be concerned with is merely the *discovery* of knowledge. That is, how it is possible for a human being to acquire new knowledge. In doing so, we will assume that knowledge has a neural correlate, that an act of knowing corresponds to some neural activity. The actual experience of knowing, from a first person point of view, will not be addressed. Nor will the issue of whether knowing requires an act of belief, or not.

By the concept "knowledge" we will understand simply that which allows a more sensible behavior in a certain situation or environment. What is sometimes referred to as "useless" knowledge (e.g., the ruling years of a nation's kings) will not be addressed since MENO's dilemma does not apply to this kind of knowledge (it is a matter of remembering rather than discovering). In the approach to be taken, we need to give up the idea that knowledge must be possessed by a subject that is separated from what it knows. Some philosophers might find this disturbing, but I wish to stress that whether the subject is viewed in this way or not is a choice of definition. The very purpose of this paper is to show that to avoid the separation of the subject from its knowledge is advantageous. This does not mean, however, that the idea that the knowledge is experienced from a first person point of view is surrendered. What is usually referred to as the "subject" may still exist as the manifestation of the knowledge (PALLBO 1997a).

Biological evolution avoids Darwin's dilemma

In one way, it is trivial that biological evolution avoids DARWIN's dilemma. After all, the very idea of evolution was a response to this problem. It will serve our purpose well, however, to investigate, a bit closer, the way in which biological evolution operates. Not least should we try to isolate the crucial qualities of the process.

First of all, we should note that there does not exist any method or algorithm that allows us to discover the features of a totally unknown environment. The reason is of course, as is stated in the dilemma, that we do not know what to look for. Only if we have a God eye's view of the world would we be able to come up with such a method. That is, once we know what we are looking for, it is easy to design a "search" that would have brought us from our previous state of ignorance into our new state of knowledge. In such a design, however, we would implant implicit knowledge of what we are looking for, i.e., we would use our *a posteriori* knowledge in the design of something supposed to be *a priori*. Therefore, such a method would be disqualified as a solution to our dilemma. We need a mechanism that extends our knowledge without any future references.

Secondly, it is important to note that evolution does not *search* for new adaptations, evolution may *find* adaptations but does so *without* searching. In the process of evolution, the evolving structure is constantly subject to variations. Those variations are produced without any goal in mind–they simply occur. Some, or even most, of them will not make much sense, so, in a way, we could say that evolution is producing solutions to problems that do not exist. But such solutions to non-existent problems (or adaptations to non-existent environments) do not necessarily remain for very long because they do not

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invigorate or strengthen the structure. Every now and then, however, the variation will create an adaptive advantage, thereby it will invigorate the structure, and therefore make it more likely to be sustainable. The evolution of this structure did not look for any particular adaptation, however. For this reason, the production of variations cannot be said to constitute a search. Thus, finding does not necessitate searching. This understanding is an important key in our approach to MENO's dilemma. Later, we will try to replicate this non-searching in a model of the brain.

Finally, we should notice that in evolution there are continuous rounds of variation-then-selection. The blind creation of variations is always based on the latest level of adaptation rather than being produced from some original starting point. Without such an arrangement, only the complexity that can be reached in one act of variation could be achieved. With a progressive evolution, on the other hand, small variations can add up to very complex and powerful adaptations. This means, however, that the variations that are produced must increase in complexity to follow this development. This is a problem of great significance that we will return to below when we discuss the requirement of a new and faster source of variation.

Biological evolution is a knowledge process

Throughout the process of evolution, what will be a successful adaptation or not is to a large extent determined by the environment. Only adaptations which do not clash with the environment will persist in existence. Therefore, those adaptations that are successful will reflect features from the environment. One example of this is the motion and the shape of fish which reflect the hydrodynamics properties of water (LORENZ 1977). Another example is the camouflage color of a bug that reflects the texture of its habitat. Such reflections of the properties of the surrounding environment can be said to express knowledge about the external reality because it improves the species ability to live in that environment. In some way, the bug species "knows" how the texture of the background looks like. Likewise, as CAMPBELL puts it, "[t]he shape of a horse's hoof certainly expresses "knowledge" of the steppe in a very odd and partial language, and in an end product mixed with "knowledge" of other contingencies" (1974, p87). Neither the horse or the bug, nor the corresponding species need to know that they know this, however. The knowledge is there and it will be utilized in the successive development since the mechanism of variation acts directly on this body of knowledge.

One must, however, be very careful with these kinds of metaphors, not to interpret them too literally. There is no Mr. Bug involved in the knowing process. Neither is there any agent that successively changes the coloring of the bug to better protect it from predators. Instead, it is through the survival and reproduction of individual bugs that the knowledge is introduced, maintained and, not least, embodied in that particular species. There is no separation between any subject and object in evolution. There is nothing beyond the evolving structure. Furthermore, this structure is not a passive medium that is manipulated from external sources. On the contrary, it is active in its own creation and transformation. However, it is not only the structure that is active in its creation, but also the environment. Knowledge can be said to already reside in the environment, and, through the process of evolution, this knowledge becomes (partly) imprinted on the species. The species thus becomes "a negative of reality, like a photographic negative, or the plaster cast of a coin" (LORENZ 1977, p23). This way, by utilizing the actual environment directly in the evolutionary process, the process does not need to maintain any secondary model or a replica of reality.

Evolution escapes Meno's dilemma

If evolution is, as argued above, absorbing knowledge about and from the environment, we must conclude that evolution escapes MENO's dilemma. Prior to the absorption the species does not already possess this knowledge, nor does it utilize any general knowledge as a guide. Simply, evolution manages to reveal knowledge that was previously unknown to it. This is what MENO's dilemma is about.

Socrates and MENO, however, were not discussing the evolutionary process of a species, but the knowledge process in the individual mind. Biological evolution is too slow to account for the knowledge acquisition of the individual. It suffers from what LORENZ called "the generational dead time," which refers to the time required to introduce and test a new variation (LORENZ 1977). In the human species, this is done only once per generation. Any event that does not last longer than this dead time will, in a way, be invisible to evolution. Therefore, if we want to cling to the

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original problem that Socrates and MENO were discussing, we need, somehow, to break through the generational dead time.

A faster source of variation

One possibility of achieving a faster evolutionary process is to introduce a new source of variation that operates at a faster pace. This is the method we will apply in this paper. More precisely, the spontaneous neural background activity will be advocated to provide this source. Before discussing the details of this specific source of variation, however, we need to dwell into the requirements of a variation mechanism in general.

Usually, in evolutionary models, it is the mechanism of selection and not the mechanism of variation that receives the most, if not all, of the attention. The reason for this inattention on behalf of the variation mechanism is probably that it is regarded as a trivial accomplishment to supply one. On the contrary, I believe this to be very hard. The mechanism of variation must, in an advanced structure, produce relatively complicated variations. Clearly, not any source of noise would be able to accomplish that.

A source of variation must furthermore be able to increase the complexity of its productions whenever the evolving structure grows more complex. This is because evolution is progressive in its nature. Any new modification of the evolving structure is made from its current state rather than repeatedly from some initial point. If the latter case were true, then we could not expect any complex structures to emerge. The level of complexity that could be reached in such a situation would be determined by how far one act of variation could stretch the structure. In a progressive evolutionary process, on the other hand, with an endless repetition of variation-then-selection, there seems to be no upper limit of an evolutionary process. The already achieved knowledge is in general preserved and continuously extended.

The problem that we face considering the variation mechanism, is that it does not only have to follow the progression, it must in fact *lead* it–variation is the path finder of evolution. This would certainly introduce difficulties if the mechanism of variation would have to reflect the complexity of the evolving structure. That is, if the variation mechanism must be continuously updated to be kept in line with the increasing complexity. To avoid this difficulty, it would be preferred if the mechanism can retain its simplicity throughout the development of the structure. This would mean that while keeping the actual *mechanism* of variation constant, the *effects* of its operation should progress with the structure. The variation mechanism can accomplish this by utilizing the evolving structure *per se* in the process, i.e., by letting the structure itself supply the required complexity.

Consider the case of the reproduction of DNA in biological evolution. This process relies on physical phenomena that do not guarantee perfection. This imperfection serves as a main source of variation in biological evolution. But while this actual source of imperfections operates in the same way regardless of the complexity of the genes, its effects do not. If a mutation appears in a complex DNA, it is likely to bear complex consequences, while a change in a simple DNA is more likely to bear simple consequences. The complexity is carried by the evolving structure while the variation mechanism remains simplistic. This way, biological evolution may progress, fueled by the very same mechanism throughout many stages of its development. The structure and complexity of DNA changes, and by basing all new variation on the latest structure, a progressive path of evolution is obtained.

This brings up an important remark concerning the level on which the variation operates. It was argued above that the variation mechanism in biological evolution does not (in one act of variation) generate a series of new hypothetical species but only slight modifications of the present one. It would be unable to produce anything more complex than that because it is unaware of what it produces. If the mechanism of variation was aware of what it produces, then, certainly, it would be able to design more complex modifications of the current structure. That would, however, require a very complex mechanism that contains general knowledge of the world. For a simple mechanism, on the other hand, the blind acts of modifications are carried out in complete ignorance. In this case, it is more wise to make only small changes to the structure to reduce the risk of maladapted creations. And with these small changes, we are more likely to obtain a progression in the path of evolution. That is, we would be more likely to preserve the knowledge achieved thus far. It would be a good idea for the bug to stay buglike in its variations rather than trying to accomplish something totally different like a butterfly in just one act of variation.

What we can conclude from the above, is that, to promote simplicity, the variation mechanism should operate on a level of complexity that is *lower*

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than the level on which the knowledge is expressed. The variation did not directly produce the knowledge of the steppe as it is expressed in the hoof of the horse. It simply produced small variations, and through its exposure to the steppe, the hoof obtained its form.

In summary, what we need to consider in a new and faster source of variation is that: (a) it should be simple; (b) the effect of the variation must increase in complexity to stay in line with the evolving structure; (c) it should retain its simplicity throughout the development of the structure; and (d) it should operate at a level of complexity that is lower than the knowledge that it expresses.

Spontaneous neural activity provides a new source of variation

Our next step is to demonstrate the presence of an evolutionary process in the brain. This process is enabled by a new source of variation, namely the spontaneous activity of the neurons. That is, the background activity or the "noise" found in most neural tissue. In this section, we will see how this second source of variation can give rise to neural activity that is not a mere nuisance, but that actually make some sense (cf. FERSTER 1996; refer to PALLBO 1997b for more details). Before we dwell into that, however, we should identify the evolutionary process in the brain so that we know what we are talking about. The structure that is evolving in the brain is the compound of the neural activity and the abstract space defined by the connections. These two together reflect the experience of the system with the environment, and, through their encounters with the environment, are constantly transformed. In a way, this compound becomes the negative of reality that was mentioned above. The components involved in this evolutionary process are:

Neural activity. The activity of the neurons in the brain constitutes the active instantiation of the evolutionary process in the brain. In biological evolution, it corresponds to the phenotype.

Neural connections. These enable the preservation of experience. As in all neural networks, the architecture of connections and the connection strengths express the "knowledge" possessed by the system. In biological evolution, this would correspond to the genes.

The propagation of activity. The propagation of neural activity from one moment to the next is chan-

neled by the connections between the neurons. This allows the previous experience that is preserved in those connections to considerably affect the transformation of neural activity. That is, there exists a relation from past experiences to the current activity.

The plasticity in the connections. The strength of a connection between two neurons are affected by the extent to which this channel is used and how it is used. The modification of a connection is locally determined by the connection itself and the behavior of the two neurons that are connected. This allows the current activity to affect the memory of the system.

Sensors. These enable a selectional pressure to be imposed upon the system. Only the spontaneous activity that harmonizes with the imposed activity as well as the connection network, will survive and influence the succeeding activity. Spontaneous activity that does not meet this demand, will be too weak to have any influence. In this way, a mechanism of selection is introduced in the evolutionary process.

Before we investigate whether our new source of variation meets the demands put on it, we should say something more about how it operates. Let us start by a comparison to a related study made by Gerald EDELMAN. He proposed his "Neural DARWIN-ISM" as a theory for how the mind functions (1987). The idea is that the ontogenetic development of the brain, i.e., the growth of the brain in the individual, provides the source of variations. This is due to the non-specific connections made between neurons at this stage. Later, when the individual is exposed to the world, the more useful of these connections are "selected" while others will fade or disappear. Thus, in his model, *selection* is operating at a much higher pace than in biological evolution. This is good, because this is required to break through the generational dead time. But the variation is still produced only once in a generation. This lack of a constant variation was commented by CALVIN (1988) in a review of EDELMAN's work. "The dance evolutionary biologists call the 'DARWINIAN two-step,' randomness-then-selection continuing back and forth for many rounds to increasingly shape up nonrandomlooking results, usually cannot be seen in EDELMAN's examples of neural DARWINISM" (p1802). Anyhow, EDELMAN's approach constitutes a major advancement in this field of study.

To accomplish the DARWINIAN two-step, we need to provide a constant source of variation. At best, the

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variation should be produced "on the fly" while a perception is created. This is what the spontaneous activity of the neurons enables. A perception can grow out of this noise when it is being supported by the stimuli and the current connection architecture in the brain. That is, the spontaneous activity that harmonizes with the present situation will sustain as a meaningful activity, while spontaneous activity that clashes with it, will disappear without affecting the neural activity to any great extent.

To see how this can work, let us consider the case of motion detection. In (PALLBO 1997c), it was shown how the utilization of noise can simplify the computation of direction detection. The idea is that the sense data is imposed upon the (cortical) detector neurons at a sub-threshold level. That is, the neurons are excited by the visual stimuli, but not enough to cause them to fire. Similarly, the background activity is kept at a low level to prevent it from causing something like an epileptic seizure. At some points, the spontaneous activity will correlate with the imposed sense stimuli (cf.WIESENFELD and MOSS 1995). When this happens, the activity in those neurons will propagate to neighboring neurons. If these neighboring neurons receive a sub-threshold stimulation as well, they will in turn propagate the activity further. This way, the activity that is signaling a detection of a motion will spread throughout the network. The architecture of the lateral connections in this network will assure that the spread of activity only can take place in the preferred direction of the detectors. That is, the activity of a rightward motion will propagate rightward among the rightward detectors as long as it continuously moves in that direction. If the direction changes, or the motion halts, then the activity will fail to propagate any further because the neurons on the right do not get excitation from the sensors any more (refer to PALLBO 1994 and 1997c for more details).

By using noise, the model of motion detection is considerably simplified. In fact, the computation of motion is never performed in the model. Instead, the detection emerges as a result from the selectional pressure imposed from the moving object. This means that the motion in the external world is made part of the cognitive process. By utilizing the external world in this manner, the need for the computational process to *search* for the motion in the external world is abolished. Instead of the search, the motion in the external world imprints itself in the brain by stabilizing the neural activity. Without a moving object, there is no stabilizer and therefore nothing but the spontaneous noisy background activity in the detectors.

The same arguments can be repeated for any kind of perception in the brain. There is no fundamental principle in the way that the motion detector operates that makes it exclusive to motion detection. What makes it a motion detector, rather than something else, is the connection architecture. Since this architecture allows activity to spread only in one direction from each neuron, it expresses knowledge about motion detection in a certain way. This knowledge, however, does not necessarily have to be innate. With the addition of some kind of HEBBIAN modifications of the neural connections (HEBB 1949), it can be a result of the brain's exposure to motion. The kind of architecture that will emerge depends on the stimuli. If the stimuli is motion, then the architecture will support this kind of detection, but with other forms of stimuli, the result would be different. In fact, this kind of exposure is very essential for the development of visual perception. During fetal development, the retina in the eye is generating waves of activity that is imposed upon the visual cortex. If this activity is suppressed (e.g., by artificial means) then the visual cortex will not develop the motion detectors that we usually find there (MEISTER et al. 1991; KATZ 1993; WONG et al. 1995; see also GILBERT and WIESEL 1992). Inversely, if the visual nerve that usually goes from the retina to the visual cortex is redirected into the auditory cortex, then the auditory cortex will be exposed to visual stimuli instead of auditory. In experiments done with such redirections, it has been found that, in a redirected brain, the neurons in the auditory cortex develop a selectivity that is typical of the visual cortex in normal brains (SUR et al. 1990; ROE et al. 1992).

Considering the results with redirected optic nerves, it appears that the cortex will adapt to whatever kind of stimuli it is exposed to. This will probably be true, not only for *external* stimuli, but also for any stimuli that is generated *internally* in the brain. This means that the model outlined above will apply to neural activity of the brain in general. That is, that not only the neural correlates of perception, but also the neural correlates of any kind of thought, can grow out of noise.

We should now return to settle the issue whether this new source of variation, the spontaneous activity, fulfills the requirement in the previous section. These were that (a) it should be simple; (b) the effect of the variation must increase in complexity to stay in line with the evolving structure; (c) it should retain its simplicity throughout the development of

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the structure; and (d) it should operate at a level of complexity that is lower than the knowledge that it expresses.

What regards the first issue, simplicity, it is quite evident that spontaneous activity satisfies this demand. All that is needed is the noisy background activity of the neurons. That is simple indeed. The second issue, the requirement of an increase of complexity in the products of the variation, is satisfied too. What the activation of a neuron will mean to the rest of the system depends on its relation to other neurons. In short, the connection architecture and not the activity per se determines the complexity that a certain activity expresses. Therefore, in a less complex system, the relations will be less complex, while in a more complex system the relations will be more complex whereby the complexity that an activity expresses also will be higher. Just like the mechanism of variation in biological evolution, spontaneous activity makes use of the evolving structure to obtain its degree of complexity. The actual mechanism of variation, on the other hand, remains simple as before. Furthermore, the mechanism will remain the same throughout the evolutionary process which meets the third criterion.

Finally, according to the fourth issue, the spontaneous activity should operate at a lower level than the knowledge it gives expression to. This is accomplished too, because the spontaneous activity is operating at the individual neuron level, while the knowledge it expresses is dependent on the system in its totality. Furthermore, any individual neural activity is in itself insignificant, it does not contain any knowledge. As we saw in the model of motion detection, there is nothing particular to motion in the activity of the neurons. Motion detection was a *system* property.

The brain escapes Meno's dilemma

We have already argued above that evolution is a knowledge process. To demonstrate that the evolutionary process corresponds to the knowledge process that we usually assign to the individual, we need to make three assumptions. They are in short:

Thoughts correlate with neural activity. We need to assume that our thoughts, as an active expression of knowledge, correspond to some activity of the neurons in the brain. Fortunately, this is not a controversial statement today, at least not within the cognitive science community. The question of how these activities are experienced as thoughts remains, however, a

controversial issue. But there is no need to address that problem here. It is sufficient to accept that neural activity correlates, somehow, with our thoughts.

The content of a thought is determined by its relation to other potential thoughts. Further, we will assume that what knowledge a certain neural activity expresses, i.e., the content of a thought, is given by its relation to other potential thoughts. This relation is determined by the connection network. The neural correlate of a thought, i.e., the neural activity pattern, can be arbitrary. But it is arbitrary only in isolation from other neural activity patterns. A thought does not contain its own interpretation per se. Rather, all potential thoughts, and their relations, form together a conceptual space in which the content of a thought is given through its position (GÄRDENFORS 1996). For instance, the color green obtains its meaning by its relation to other colors, like red and blue. If the color space were inverted so that the neural activity patterns that correlates with the colors would change but the relation with the other colors would remain intact, there would be no way of telling the difference (VARELA et al. 1991). In this paper, however, we are ignoring the issue of how the contents achieve a first person experience.

The knowledge of the mind correlates with the neural connection network. Finally, we need to assume that the knowledge possessed by the mind correlates with the neural connection network of the brain. That is, the connections between the neurons, and the strengths of those connections, correlate with the knowledge. This is the case in all neural networks and should therefore not be a controversial issue. The connection network architecture determines how the neural activity will propagate in the system. Thereby, it decides what patterns of activity that will be supported and what patterns will be suppressed. In this way, the knowledge that is resident in the system will affect the process of neural activity, i.e., the thoughts.

If these assumptions are accepted, there is a way for the knowing individual to extend its knowledge without falling prey to MENO's dilemma. The reason is mainly that there is no need to *manage* the knowledge in an evolutionary system. Neither is there any need to *search* for that new knowledge, nor is there, therefore, any need of a subject, a first person, that looks for new knowledge. The knowledge itself is animated and is continuously growing. If we want to speak about a subject, the subject

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must be taken to be this animated knowledge–not anything beyond it. We have to be careful, because as soon as the subject is separated from the knowledge, as soon as the knowledge becomes a passive object managed by the subject, MENO's dilemma will trouble us anew.

Without the problem of managing the data, we are not restricted to follow a fixed procedure or algorithm. On the contrary, our faster variation mechanism allows the creation of thoughts, and thereby knowledge, to escape the boundaries characteristic of formal procedures. Whereas a rigid formal algorithm must implicitly contain general knowledge about how to approach the environment *a priori*, the utilization of noise avoids this problem.

When we are confronted with a situation, a perception will grow from the spontaneous activity from those activities that harmonize with the environment as well as the preserved knowledge expressed in the connections. This way, we create a thought of something we already know. But the spontaneous activity may also create something novel. It may create some activity that clashes with neither the environment nor the preserved knowledge but that is still "unknown" to the system. Whenever this happens, this activity will affect the connections and extend the knowledge preserved by the system. Later on, this preserved knowledge will make it easier for the same knowledge to be reactivated because the connections will support it. In an immature system, the knowledge that will be discovered this way will be of a rather simple character. But as the system grows more mature, more and more advanced features of the environment can be discovered.

Interestingly, the knowledge already absorbed by the system may also become a hindrance. This may happen because the present knowledge may favor an interpretation of the stimuli that is different from the one that would be more suitable at a given occasion. This phenomenon is called interference and was investigated in STROOP's now classical experiments (1935). He made a list of color words written in ink of various colors where the ink color and the color name did not match. When STROOP asked his subject to read the names of the colors, they scored only 5.6% worse compared to a test trial with

the names written all in black ink. When he asked them to name the colors of the ink used in the different words, the subjects scored much worse. Compared to a test trial of naming the colors of ink blobs, the score was 74.3% worse. This experiment shows very clearly how knowledge already present in our minds interferes with our perception of this task. But, more important, it also shows that the present knowledge is very active in the creation of a thought, even when we deliberately do not want it to be so.

Discussion

In this paper it has been asserted that MENO's dilemma is due to the separation of the knowledge from the subject. When the knowledge is considered to be passive in its nature, it needs an agent-a manager-to become useful. This means that the problem of knowledge is localized to the subject. While the passivity of knowledge will simplify our models of knowledge, it will to a great extent complicate the issue when we address the subject. As an illustration to this, consider three bodies revolving around each other under mutual influence of gravity. This system is so complex that the solutions to the differential equations that describe these movements are understood only in special cases (NORTON 1995). The physical system, however, is not operated by means of these equations. The bodies themselves embodies their own solution and there is nothing beyond these bodies in the system. Therefore, there are no problems regarding management in this system.

To avoid such complications of management, we took an evolutionary approach to knowledge in this paper. This does not allow a solution to MENO's dilemma, but it does allow a more sane approach-to avoid it. To accomplish this, we did not only consider evolution as a knowledge process, but also, inversely, the knowing process of the individual as an evolutionary process occurring in the brain. The benefit of this approach is that it allows an explanation of how knowledge can be activated and how it can extend itself without the need of an external subject. The explanation of this is basically a parallel to the explanation of how the species can emerge without the involvement of a god. Both utilize a creative mechanism that produces variations of the structure. With the selectional pressure from the environment, only those variations that harmonize

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Robert Pallbo, Lund University Cognitive Science, Kungshuset, S–222 22 Lund, Sweden. Email: robert.pallbo@fil.lu.s with the environment will persist. As a result, the variations that *do* persist come to reflect conditions from the environment. That is, they embody knowledge of the environment.

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An interesting consequence of the model is that it assumes that the mind is chaotic in its nature while the environment is (relatively) stable. If the environment would not contain phenomenon that are more stable than the mind, then it could not offer any selectional pressure to the mind. As a result, the mind would remain chaotic. When the environment is more stable than the mind, on the other hand, it is precisely the chaotic nature of the mind that will allow it to harmonize with the environment. Most models of thinking, however, view the situation the other way around. The mind is considered to be ordered while the world is chaotic. Only by an effort of the ordered mind does it manage to make any sense out of the chaotic input it receives. In the model presented in this paper, on the other hand, there is actually no effort involved at all. The chaotic mind is simply exposed to some ordered phenomena, and, as a result, these phenomena will imprint themselves in the brain. Both the phenomena as well as the brain remain unaware of this process, however. But that is not a bug, it is a feature.

In conclusion one may rise the question about what problems the approach taken in this paper can solve that cannot be dealt with by traditional methods. Due to the nature of the question, however, a straight forward answer cannot be given. If we are asked to present a specific problem that this approach solves, then this problem will be known a priori. This, in turn, means that it is easy to design a traditional solution. But then we need a designer and we need to know the problem beforehand. MENO's dilemma, on the other hand, specifically addresses that which we do not already know. Therefore, in this paper, an approach has been taken that assumes neither the need of a designer nor the *a priori* knowledge of the problems we are going to be presented to.

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Activational Hormonal Influences on Abilities and Attitudes

Implications for Evolutionary Theory

has been widely observed in humans and animals that males tend to be more successful in spatial tasks than females, and several theories have been advanced regarding the evolutionary origins of these differences (See GAULIN 1992; GAULIN and HOFFMAN 1988; SHERRY and HAMPSON 1997; SIL-VERMAN and PHILLIPS 1997. for reviews). In one of these theories, pertaining primarily to the human case, SILVERMAN and EALS (1992) (See also EALS and SILVERMAN 1994) conjectured that spatial sex differences originated with division of labor between sexes; that is, the disparate spatial attributes required by males for hunting and females for foraging during the "environment of evolutionary adaptedness" (TOOBY and DEVORE 1987). Previous writers had noted the correspondence of spatial measures

Abstract

Based on theories from evolutionary psychology, three performance measures (Mental Rotations, Object Memory and Object Location) and one attitudinal measure (Sexual versus Emotional Jealousy) were examined for both sex differences and possible activational hormonal influences in a between-subjects design with 209 university undergraduate volunteers. All four measures exhibited the expected sex differences. Among the 120 female subjects, two indirect measures of current estrogen dose (based on stage of menstrual cycle) were both significantly related to response patterns on the Mental Rotations and Jealousy tests, but unrelated to response patterns on either Object test. On the former two tests presumed estrogenic effects were consistent with theory in that higher doses were associated with more "feminine" response patterns. In addition, Mental Rotations and Jealousy responses were very highly correlated among women, but not among men, a finding also consistent with a presumed cyclic estrogenic effect among women. The data were considered as additional support for the evolutionary bases of these sex differences. The issue was also discussed of whether activational hormonal effects, as found in the present study, bore some evolutionarily adaptive function independent of organizational hormonal effects.

Key words

Cognitive performance, jealousy, estrogen, spatial ability.

cifically, these would entail the ability to rapidly learn the contents of object arrays and the spatial relationships of the objects in an array to one another, whether through incidental or direct learning.

The investigators tested their hypotheses in several ways. In one, developed for group administration, subjects were presented with drawings of common objects in an array and asked to examine them for one minute. In another, subjects were left alone for two minutes in a small room, containing a variety of work related and personal objects. In some conditions they were instructed to try to memorize the objects in the room and their locations; in others they were led to believe that they were merely waiting for the experimenter to arrange materials for the upcoming

which show a male bias, such as mental rotations, to hunting skills (e.g., JARDINE and MARTIN 1983). SIL-VERMAN and EALS extended this premise, however, to posit that if the disposition for the development of these attributes evolved in the male in conjunction with hunting, females would have developed parallel spatial specializations associated with foraging. Speresearch session. Dependent measures for all methods consisted of recall of objects and their locations, and, for all methods, females' scores consistently exceeded those of males at statistically significant levels. One objective of the present study was to attempt to replicate these findings, using SILVERMAN and EALS' group test.

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Regarding proximate mechanisms mediating evolutionarily based spatial sex differences, various sources of evidence suggest that they are shaped by the organizational effects of sex hormones (for reviews see NyBORG 1984; GAULIN and HOFFMAN 1988; KIMURA and HAMPSON 1993). Recently, several studies have suggested that sexually dimorphic cognitive abilities, particularly spatial skills, are subject to activational hormone effects as well (HAMPSON 1990a; GOUCHIE and KIMURA 1991; SILVERMAN and PHILLIPS 1993). For example, SILVERMAN and PHILLIPS (1993) described four studies of the relationship between menstrual-cycle phase and three-dimensional mental-rotations performance, using both between- and withinsubjects designs. All of these studies showed significant increases in mean mental-rotations scores during the menstrual phase, when estrogen levels are typically at a minimum. Tasks that lacked a significant spatial component showed no relationship to menstrual-cycle phase.

Inasmuch as most of SILVERMAN and PHILLIPS' data derived from one Canadian university population, a second purpose of the present study was to attempt a replication of their observed menstrual-cycle effects with a different population. A further goal was to assess whether performance on the SILVERMAN and EALS' object and location memory tasks were related to menstrual-cycle phase. Based on the previous finding that estrogen improves performance on tests that typically show a female advantage (HAMPSON 1990a 1990b, HAMP-SON and KIMURA 1988 1994), we expected decreased scores during the menstrual (low estrogen) phase of the cycle; that is, we expected the inverse of the effect previously observed on mental rotations tasks.

Certain other psychological dimensions also exhibit marked sexual dimorphism. Many of these have been argued to be adaptive consequences of sex differences in reproductive tactics (BUSS and SCHMIDT 1992; BAILEY et al. 1994). We selected one such dimension, sexual versus emotional jealousy. Males show more of the former than females, which is assumed to be an adaptive manifestation of paternal uncertainty. Females exhibit more of the latter than males, presumably based on their larger investment in individual offspring and consequent larger requirement for a secure, pair-bonding relationship. (BUSS et al. 1992) Our final objective was to attempt to replicate reported sex differences on this dimension, and to examine whether the responses of female subjects were related to menstrual-cycle phase.

Methods

Subjects

A total of 209 student volunteers (120 women, 89 men) were recruited from two undergraduate anthropology classes. Their ages ranged from 17 to 64 years. To maintain a relatively homogeneous sample and one that was comparable in age to prior studies of this type, subjects over 30 years of age (n = 5) were excluded from the analytical phase. This left a sample of 118 women and 86 men for analysis; mean age for this sample was 20.3 years. One further subject (a woman) left the Mental Rotations Test blank and is hence excluded from analyses involving that test.

Instruments

A short battery of tests was organized into two booklets, one for female and one for male subjects (see below). The batteries were group-administered and included the VANDENBERG and KUSE (1978) Mental Rotations Test, the Object Memory and Object Location Tests developed by SILVERMAN and EALS (1992), a forced-choice Jealousy item similar to that pioneered by BUSS et al. (1992), and a series of questions designed to elicit information about hormonal status. Order of presentation in the test booklets was 1) Jealousy, 2) Object Memory, 3) Object Location, 4) Mental Rotations, 5) Hormonal Status. Only the Jealousy and Hormonal Status sections differed by sex of subject. The initial page of the test booklets explained that the study was part of a "research program on the effects of hormones on abilities and attitudes."

The Mental Rotations Test is well known. It presents two-dimensional depictions of three-dimensional stimulus objects, and evaluates the subject's ability to recognize each stimulus object when seen from novel, i.e., rotated, perspectives. There are 20 such items on the test, each of which has two correct answers. This test typically yields a pronounced sex difference with males scoring significantly higher than females. We used a standard, timed presentation of this test, including the normal two pages of tutorial and practice items.

As designed by SILVERMAN and EALS, (1992) The Object Memory and Object Location Tests are intended to be given together, and we adhered to this presentation. The subjects were first shown a sheet of 8.5"x11" paper with an array of simple line drawings of 27 common objects. The subjects

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were asked to examine the sheet for one minute and then to turn to the following blank page. With the initial stimulus page thus out of view, the subjects were told that the next page was similar to the one they had previously examined but that several objects had been added to the original array. They were asked to cross out all the added items and told that they would be given one point for each added item crossed out (correct response) and have one point subtracted for each original item crossed out (incorrect response). They were given one minute to complete this Object Memory Test and again asked to turn to a blank page. Again with all arrays out of view, subjects were told that the next page was similar to the first (unaugmented) array but that some of the objects would be in a different location. They were asked to circle each object that was in its original location and cross out any object that had been moved. Subjects were allowed one minute to complete this Object Location Task and given one point for each correct answer.

To avoid ambiguity we decided to word the forced-choice jealousy item differently for female and male subjects (cf. BUSS et al. 1992). In the text that follows words in italics appeared only in booklets distributed to females; words in parentheses appeared only in booklets distributed to males.

Suppose you are in a serious relationship with a *man* (woman). Which of the following scenarios would be worse?

- A. You discover that you partner is having sex with another *woman* (man).
- B. You discover that your partner is involved in a deep emotional relationship with another *woman* (man).

Of course both are bad; PLEASE CIRCLE THE ONE WHICH IS WORSE.

Thus female subjects were asked to imagine their male partner's involvement—either sexual or emotional—with another woman, and male subjects were asked to imagine their female partner's involvement with another man. This can be argued to be inappropriate wording for homosexual subjects. Because the frequency of homosexuality is low, perhaps two percent of the male population and still lower among females (HAMER and COPELAND 190/), we felt that it would be better to tolerate the small number of possibly aberrant data points that homosexuals might generate than to risk an unknown number of invalid answers that a complex or ambiguous wording scheme might have elicited from the numerically preponderant heterosexual subjects.

Hormonal status is a key independent variable in this study. For females, hormonal status was assessed via a detailed menstrual questionnaire, intended to assign each female's test date to a particular day in her monthly cycle. Male subjects responded to questions about steroid use.

Estimation of Hormone Levels

As noted above, previous studies (HAMPSON 1990a; GOUCHIE and KIMURA 1991; SILVERMAN and PHIL-LIPS 1993) have demonstrated that women's performance on certain spatial tests, including mental-rotation tasks, is significantly elevated during their menstrual periods. A number of researchers have explicitly attributed this to the fact that estrogen depresses spatial performance and is at a cyclic low during menstrual flow. We used two separate techniques to generate proxy measures of estrogen status. The first was the simple method used previously by others (e.g., SILVER-MAN and PHILLIPS 1993): simply grouping female subjects by whether or not they were menstruating at the time of testing.

The second attempts a more sensitive analysis by assigning an estimated estrogen value to each woman for the cycle day on which she was tested. This required a determination of her current position in her monthly cycle and the assumption that she exhibited the mean estrogen value for that cycle day, based on standard gynecological sources. Two widely cited sources were used (TAYMOR et al. 1972; SPEROFF et al. 1989) and their estimates for each cycle day were averaged to produce a composite day-by-day estrogen profile.

These standard sources are based on large samples and therefore reflect the mean cycle length of 28 days, a value from which many individual women, including many of our subjects, deviate. How should hormonal values be assigned for women whose cycles deviate from the 28-day mean? Most of the individual variation in cycle length is due to variation in the preovulatory phase, the postovulatory phase showing considerably less variance (VOLLMAN 1977). For this reason we elected to assign women to cycle days on the basis of the date of expected onset of next menses, rather than date of onset of last menses; this appropriately forced all the variation in cycle length into the preovulatory phase.

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This method is subject to various possible sources of error. One is the risk of overgeneralizing these hormonal estimates to women with abnormally long cycles. The longer a woman's cycle, the less precisely her unique hormone profile will fit the population-wide means on which our estimation technique is based. Thus, women with menstrual cycles longer than 40 days (n = 9) were excluded from these analyses. A second problem is that, inevitably,

Test	Adjusted Group Means (s.e.)			Effect of Age			
	Female	Male	F	p	Reg. Coeff.	Т	р
Object	17.62	16.25	9.99	0.002	0.030	0.30	0.76
Memory	(0.28)	(0.33)					
Object	21.90	20.68	7.13	0.008	0.011	0.10	0.92
Location	(0.29)	(0.34)					
Mental	8.13	14.83	32.85	0.0001	0.051	0.19	0.85
Rotation	(0.75)	(0.88)					

Table 1: Spatial test performance of men and women, controlled for age.

some of our subjects were using oral contraceptives at the time of our study, thereby altering to some degree their personal hormone profiles. The hormonal formulation of these contraceptives varies, but a significant proportion of contraceptives now prescribed are of the "tri-phasic" type, which attempts to mimic a normal hormone profile (UPTON 1980). Thus, we elected to ignore this possible source of error because classifying all women, regardless of contraceptive use, according to population means is systematically conservative with respect to all the hypotheses tested here. Our general method is to ask whether hormone levels are correlated with test outcomes. Any random error introduced into the estimation of hormone levels should weaken observed correlations between hormone levels and test outcomes.

Hormonal estimates were constructed for estrogen because prior direct and indirect evidence suggests that it affects spatial performance. To provide a control, the same methods were used to construct estimates for progesterone, a hormone for which no effects on cognition have been indicated (HAMPSON 1990b).

Analysis

Because each subject was tested only once, we chose to approach the analysis as a between-subjects design. Our general approach was to treat hormonal status as the independent variable and performance on each of the tests—Mental Rotation, Object Memory, Object Location and Jealousy—as dependent variables in separate analyses, to explore the extent to which each varied with hormonal status. In some cases we also examined the correlation between performance on certain pairs of tests.

Results

Sex Differences

As expected, there were baseline sex differences on all four tests included in the battery. Unfortunately, there was also a statistically significant sex difference in age in our sample ($F_{1,202} = 5.63$, p = 0.019) with males averaging approximately 0.7 years older than females. For this reason we analyzed sex differences on the three quantitative tests, Mental Rotations, Object Memory and Object Location, using analysis of covariance, with test score as the dependent variable, sex as a grouping variable and age as a covariate. Table 1 shows that females performed significantly better than males on the Object Memory and Object Location tasks and significantly worse on Mental Rotations. For none of these tests was age a significant covariate (Table 1).

There was also a significant sex difference on the jealousy item: 30 of 116 women (26%) and 35 of 84 men (42%) answered that sexual infidelity was worse than emotional infidelity (Chi² = 5.55, p = 0.019). Again it should be considered whether this effect is due to the mean age difference between male and female subjects. This question was addressed in a logistic regression format where response on the jealousy item was treated as the dependent variable and age and sex were independent variables. Sex of subject explained a significant proportion of the variance in jealousy responses, and after that portion of the variance to the variance was removed, age made only an insignificant contribution to the regression equation (F_{1,197} = 1.49, p = 0.224).

In summary, all four of the measures examined in this study—Object Memory, Object Location, Mental Rotations and Sexual versus Emotional Jealousy—ex-

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Test	Adjusted Group Means (s.e.)				Effect of Age		
	No Period	Period	F	p	Reg.Coeff.	Т	p
Object	17.41	18.29	2.50	0.116	-0.076	-0.68	0.50
Memory	(0.26)	(0.49)					
Object	22.01	21.49	0.78	0.378	0.091	0.76	0.45
Location	(0.28)	(0.52)					
Object	7.29	10.98	5.84	0.017	-0.183	-0.59	0.56
Location	(0.71)	(1.34)					

Table 2: Spatial test performance of menstruating and nonmenstruating women,controlled for age.

			Spati	ial Test		
Hormone	Object M	femory	Object I	ocation	Mental R	otation
	r	р	r	р	r	р
Estrogen	-0.079	0.421	0.138	0.157	-0.203	0.037
Progesterone	-0.042	0.671	0.012	0.902	-0.052	0.595

Table 3: Correlations between estimated hormone levels and performance on spatial tests.

hibited significant sex differences, and all these sex differences are in the direction previously reported by others (BAILEY et al. 1994; BUSS et al. 1992; SILVERMAN and EALS 1992; EALS and SILVERMAN 1994; SILVERMAN and PHILLIPS 1993; VANDENBERG and KUSE 1978).

Possible Hormonal Effects: Menstrual Status

If sexually dimorphic abilities and attitudes have a hormonal basis they might be expected to fluctuate with hormonal status. To parallel previous studies we began by examining the extent to which each of the four sexually dimorphic measures varied among women as a function of menstrual status. Estrogen is at a cyclic low during menstruation and estrogen is thought to "feminize" performance. For our battery that yields the predictions that menstruating women would have had depressed scores on Object Memory and Object Location, elevated scores on Mental Rotation, and been more sexually jealous.

Approximately 22% of the female subjects were menstruating during the test battery and this subset of women happened to be significantly older (by 0.95 years) than their nonmenstruating counterparts ($F_{1,116} = 4.49$, p = 0.036). For this reason we again used analysis of covariance to control for age when

evaluating group differences. There were no significant differences between menstruating and nonmenstruating women on either the Object Memory or Object Location Tasks. Surprisingly, the Object Memory score of menstruating women were marginally significantly elevated (rather than depressed). However, as in previous studies, menstruating women performed significantly better on Mental Rotations. Again, in no case did age explain a significant proportion of the variance in performance (Table 2).

On the Jealousy item, 11 of 26 menstruating women (42%) and 19 of 90 nonmenstruating women (21%) answered that sexual infidelity was worse than emotional infidelity (Chi² = 4.73, p = 0.030). This significant effect could not have been due to the mean age difference between menstruating and nonmenstruating subjects, because logistic regression showed that while menstrual status

was a significant predictor of response to the Jealousy item, age was not ($F_{1,113} = 0.43$, p = 0.513).

Possible Hormonal Effects: Estrogen and Progesterone Estimates

As detailed under Methods, estrogen and progesterone levels at the time of testing were estimated for each female subject. Because Object Memory and Object Location scores did not vary systematically with menstrual status, it is not surprising that neither was significantly correlated with estimated estrogen level. However, Mental Rotation scores were significantly negatively related to estimated estrogen levels (Table 3), a finding that accords well with the proposal that the higher Mental Rotation scores of menstruating women is due to their low estrogen levels. As expected, none of the three cognitive measures were related to estimated progesterone levels. Likewise, responses to the Jealousy item were significantly related to estimated estrogen levels but not to estimated progesterone levels (Table 4).

In summary, none of the cognitive or attitudinal measures were significantly related to estimated progesterone levels. This was an expected outcome; progesterone was considered merely as a control. On the other hand, Mental Rotations scores and Jeal-

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ousy responses were both related to estimated estrogen levels, and both were related in the same direction; lower estrogen levels had a "masculinizing" effect, producing higher Mental Rotations scores and an increased likelihood of judging sexual infidelity to be worse than emotional infidelity.

Intercorrelations Among Variables

The results outlined above are consistent with the idea that at least some sexually dimorphic abilities and attitudes fluctuate systematically in relation to cyclic changes in hormone levels. In the present study we have no direct hormone assay, relying instead on proxy measures such as the discrete event of menstruation or hormone estimates based on population means. Within the limitations of our study, one

other approach is possible. For each subject, Mental Rotation performance and Jealousy responses were assessed on the same day. If they fluctuate in relation to the same underlying variable, then these two measures would be expected to be intercorrelated. Of course, in this between-subjects design, any such association might be due not to cyclic phenomena within subjects but to baseline differences between subjects (e.g., overall "masculinization" or "feminization"). To control for this possibility, Table 5 compares the association between Mental Rotations score and Jealousy response for men and women; only the latter show a significant relationship. The Mental Rotations scores of those women (n = 30)who rated sexual infidelity as more threatening were so elevated that, based on post-hoc SCHEFFE tests in a two-way (sex by Jealousy-response) ANOVA, they were statistically indistinguishable from those of men (p > 0.1), but they were highly significantly different (p < 0.01) from those of other women.

Hormonal Effects in Men

Although our principal focus is cyclic hormonal effects, there is a quasi-experimental hormone manipulation available for analysis among our male subjects, six of whom reported using anabolic steroids. Of course these six men represent a self-

Hormone	Sexual Infidelity Worse	Emotional Infidelity Worse		
	Mean (s.e.)	Mean (s.e.)	F	p^{a}
Estrogen (pg/ml)	131.0 (16.2)	165.3 (9.9)	3.15	0.039
Progesterone (ng/ml)	2.08 (0.60)	3.13 (0.42)	1.77	0.187

a. test for estrogen effect is one-tailed due to the directional prediction.

Table 4: Estimated hormone levels for subjects giving different answers to the jealousy item.

	Sexual Infidelity Worse	Emotional Infidelity Worse		
	Mean (s.e.)	Mean (s.e.)	F	p
Men	16.0 (1.6)	14.0 (1.4)	0.88	0.352
Women	12.1 (1.0)	6.6 (0.7)	16.21	0.0001

Table 5: Sex-specific mental rotation scores for subjects giving different answers to the jealousy item.

selected sample and may differ from the normal male population in many ways (e.g., perhaps being less scholastically and more athletically inclined) but we do have the opportunity to examine their performance across a series of tests to partially control for such possible self-selection biases.

Evidence exists that, like estrogens, high doses of androgens feminize performance (e.g., SHUTE et al. 1983; WILLIAMS et al. 1990; GOUCHIE and KIMURA 1991). Given this expectation, one-tailed tests were used. Overall, steroid users do not show depressed performance, scoring better than nonusers on two of the three tests (Table 6). However, on all three tests they exhibit "feminized" performance, scoring higher on the Object Memory and Object Location, and lower on the Mental Rotations. Two of these three effects are at least marginally significant, a rather remarkable outcome considering the very small subsample of steroid users.

On the Jealousy item 2 of 6 steroid users (33%) and 33 of 78 of nonusers (42%) answered that sexual infidelity was worse than emotional infidelity. This result is in the predicted direction (steroid users' answers more "feminized"), but it does not approach statistical significance. With such a small subsample of steroid users, only if all six had answered that emotional infidelity was worse, would the difference have been statistically significant (by Likelihood-Ratio Test).

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Test	Test Me			
	Steroid Users	Steroid Nonusers	F	p^{a}
Object Memory	17.67 (0.62)	16.16 (0.41)	4.13	0.035
Object Location	21.67 (1.05)	20.61 (0.43)	0.43	0.257
Mental Rotation	8.83 (3.71)	15.30 (1.06)	2.61	0.055

a. where LEVENE's Test indicated unequal variances, the WELCH Test for equality of means was used.

Table 6: Test-specific performance of men as a function of steroid use.

Summary and Discussion

In support of prior studies (BAILEY et al. 1994; BUSS et al. 1992; SILVERMAN and EALS 1992; EALS and SIL-VERMAN 1994; SILVERMAN and PHILLIPS 1993), and the evolutionary theories to which these relate, all four dependent variables examined in this study—Mental Rotations, Object Memory, Object Location and the forced-choice Jealousy item showed significant sex differences in the predicted direction. The "feminine" pattern is lower scores on Mental Rotations, higher scores on Object Memory and Object Location and an emphasis on emotional as opposed to sexual jealousy; the "masculine" pattern is the reverse.

The present findings pertaining to hormonal bases of these sex differences also support prior studies. Among women, presumed estrogenic effects, assessed via comparisons of menstruating versus nonmenstruating women and via estimates of estrogen levels at the time of testing, were consistent. Regardless of the assessment method, "low-estrogen" women exhibited significantly more "masculine" performance on the Mental Rotations and Jealousy measures. Possible progesterone effects, not predicted and examined solely as a control, were reliably absent. Predictions of a positive relationship of estrogen level and object and location recall, however, were not confirmed.

Among women, but not among men, Mental Rotations scores and jealousy responses were strongly correlated, more "masculine" scores on the former

being strongly associated with more "masculine" responses on the latter. We tentatively interpret this positive association as further evidence for the correlated, estrogenic influences on these two very different psychological traits, but

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our between-subjects design does not allow exclusion of the possibility that the correlation is due to individual than differences rather cyclic changes. The latter interpretation would require that our sample of menstruating women was, bv chance, much more masculine in overall psychological makeup than was our sample of nonmenstruating women.

Among men, steroid users exhibited more "feminine" patterns than

nonusers on all four dependent variables; two of these differences approached or exceeded conventional significance levels. These results accord well with prior work suggesting a negative association between androgen levels and performance on tasks that typically favor men (GOUCHIE and KIMURA 1991). Such effects are not limited to those who artificially manipulate their hormone levels. Androgen levels cycle seasonally in normal men, becoming higher in the autumn and lower in the spring, and here also an inverse relationship with spatial performance has been reported (KIMURA and HAMPSON 1994).

One question raised by the present research is: Are cyclic changes in cognitive and attitudinal variables themselves adaptations, or are they mere side effects of evolved organizational (developmental) responsiveness to sex hormones, as suggested by SILVERMAN and PHILLIPS (1993). Sexual selection is a form of disruptive selection where different phenotypes are favored in the two sexes, and it has been extensively argued that such adaptive sex differences can include cognitive and psychological traits (BUSS and SCHMIDT 1992; GAULIN 1992; SILVERMAN and EALS 1992; BAILEY et al. 1994). Given that there can be very few genetic differences between the sexes (confined to the ychromosome or dosage effects of x-linked genes), any such sexual differentiation must rest largely on "environmental" cues, such as circulating sex hormones. Thus the developmental responsiveness of mammalian bodies to sex hormones is not surprising, and their subsequent use in orchestrating cyclic reproductive patterns is similarly logical. But once they

> have produced their adaptive developmental effects why should they exert ongoing cyclic influences in the cognitive and psychological domains?

> Post-hoc adaptive explanations can be constructed quite easily for some of the cyclic ef-

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fects observed in this (and other) studies. For example, it might have been advantageous to hominid females to be more sexually jealous during their menstrual periods, the only time when they overtly signalled their infertility. But it is not so transparent why an increase in spatial ability would have conferred an advantage on menstruating females (or conversely why a decrease in spatial ability would have been advantageous to ovulating females).

One evolutionary based notion may pertain, however. BEVER, BOCIRNES, GORDON, ILARDI, LAMEN-DOLA, MICCHELUCCI and SCHENKMAN (1993) proposed that decreased general spatial test performance in females relative to males is a concomitant of their demonstrated lesser sensitivity to bodily cues. This, in turn, serves to render them more tolerant of pain and, hence, better able to sustain the ordeal of childbirth. Inasmuch as estrogen levels undergo a marked rise prior to childbirth, this theory can readily explain the adaptive basis for inverse activational effects on spatial behavior. Support for these notions

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issues from WOODFIELD's (1984) study, showing reduced spatial test performance in parturitional women several days before childbirth, when estrogen levels are at peak, compared to several days after, when they have returned to normal state.

An additional question raised by this study is: Why was the female advantage on the Object Memory and Object Location tasks not enhanced by high estrogen levels? One possibility is that the relatively small effect size for the sex difference on these variables (compared to that for Mental Rotations; see Table 1) indicates only a weak hormonal influence. This idea is rendered less credible by the fact that the menstrual effects for these two variables are in opposite directions in our sample (Table 2), with Object Location showing nearly significant reductions in the high estrogen phase. It may be that estrogen interferes with spatial cognition in general, regardless of whether the task shows an initial male or female bias, although additional evidence would be required to substantiate this view.

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Zusammenfassungen der Artikel in deutscher Sprache

William Wimsatt Functional Organisation, Functional Analogy, and Functional Inference

Der Funktionszusammenhang (functional organisation) ist die Architektur des Phänotyps. Dies ist der Argumentationsbereich des adaptiven Designs, im Zusammenhang mit jenem der Selektionstheorien, auf allen Ebenen von der Gestaltung des genetischen Codes über zelluläre und physiologische Funktionszusammenhänge bis zur Populationsdynamik. Der Aufbau, die Erhaltung, die Analyse und Instandsetzung des Funktionszusammenhanges (functional organisation) erhellen unsere Sinnzusammenhänge von der Biologie über "software engineering" bis zum Entwurf von Gesetzen.

Die logischen Voraussetzungen für funktionelle Zuschreibungen ermöglichen die Analyse der Struktur von funktionellen Hierarchien. Es werden entscheidende Variablen der funktionellen Zuschreibung erarbeitet: der Funktionsgegenstand (I) und sein Verhalten (B), welches sich in einem Funktionssystem (S) in seiner jeweiligen Umgebung (E) artikuliert, um einen Zweck (P) entsprechend einer Menge von Kausaltheorien (T) zu erfüllen. Die Variable P wird durch eine Selektionstheorie oder Selektionstheorien spezifiziert. Diese ergibt bzw. ergeben die Struktur für den Funktionszusammenhang (functional organisation), welcher als "objektbegründeter Baum" (im graphentheoretischen Sinn) beginnt, und durch eine Untersuchung dahingehend erarbeitet und beeinflußt wird, um der Ontologie für Funktionen, die auf natürlichem Verhalten basieren, besser zu entsprechen. Parallelbahnen und Funktionskreisläufe müssen hinzugefügt werden, so daß die "Hierarchie" nicht länger als Baumstruktur zu fassen ist.

Die idealen Strukturen, wie sie von der konzeptuellen Analyse nahegelegt werden, stehen jenem heuristischen Instrument entgegen, wie sie in der Praxis der Funktionsanalytiker benutzt werden. Diese Struktur dient als Grundlage, um Funktionsanalogien und Äquivalenzbeziehungen zuzuordnen und zu bewerten. Sie kann das erforschende Aufarbeiten von funktionell organisierten Strukturen anleiten.

Problemlösungsstrategien und biologische Anpassungen teilen spezifische Merkmale, wobei auch deren funktionelle Struktur inbegriffen ist. Die Analyse von Heuristiken bezieht sich dabei auf den Bezug von Theorie und Praxis, hilft die Kraft von verschiedenen Denkmustern zu verstehen und warnt vor Verzerrungen und Versagen.

Wolfgang Lalouschek Brain and Language. Beyond the Left Cerebral Hemisphere

Die sprachliche Kommunikation ist eine der wesentlichsten Fähigkeiten des Menschen. Die entscheidende Rolle der linken Großhirnhemisphäre für Sprachgenerierung und Sprachverständnis ist seit langem bekannt. Dementsprechend konzentrierten sich die meisten Untersuchungen bezüglich der neuralen Grundlagen der Sprache auf die linke Hemisphäre. Im Gegensatz dazu sollen im vorliegenden Artikel die Beiträge anderer Hirnregionen zur Sprachfunktion dargelegt werden, die üblicherweise in diesem Zusammenhang weniger Beachtung finden. Zu diesen gehören verschiedene subkortikale Kernregionen, nämlich die Basalganglien und der Thalamus. Während erstere vor allem mit der Bewegungskoordination in Verbindung gebracht werden, wird als Hauptaufgabe des Thalamus die Vermittlung sensorischer Informationen zur Großhirnrinde gesehen. Kortiko-subkortikale Schaltkreise scheinen jedoch auch für die Sprachfunktion von großer Bedeutung zu sein. Mögliche Mechanismen subkortikaler Beteiligung an Sprachgenerierung- und verständnis im Rahmen exzitatorischer und inhibitorischer Regelkreise, unter anderem zur Regulierung der kortikalen Aktivität und zum semantischen Monitoring, werden dargelegt. Auch das Kleinhirn wird vor allem in Zusammenhang mit der Bewegungskoordination betrachtet. Neuere Untersuchungen deuten jedoch auf eine mögliche Funktion des Kleinhirns im Rahmen kognitiver Prozesse, dar-

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unter auch der Sprachproduktion, hin. In diesem Zusammenhang erscheinen Erkenntnisse zur Evolution des Kleinhirns und seiner Verbindungen im Rahmen der Hominidenevolution von besonderer Bedeutung, die ebenfalls vorgestellt werden. Schließlich liefert auch die rechte Hemisphäre wesentliche Beiträge zur Kommunikation, insbesondere im Hinblick auf den emotionalen Gehalt sprachlicher Mitteilungen sowie auf bestimmte Formen nonverbaler Kommunikation.

Rupert Riedl From Four Forces Back to Four Causes

Das Paradigma der Physik erlaubt die zureichende Erklärung aller Phänomene auf der Grundlage von vier Wechselwirkungen (Kernkräfte, schwache Wechselwirkungen, Elektromagnetismus und Gravitation). Jedoch führte, die in letzter Zeit intensivierte Beschäftigung mit komplexen Systemen zu einer Renaissance des Denkens in vier Ursachen (Kräfte, Material- und Formursachen, Zwecke) – eine Sichtweise, die auf ARISTOTELES zurückgeht.

Es handelt sich jedoch dabei nicht um einen Rückfall in frühe Weltbilder, sondern vielmehr um eine methodische Notwendigkeit – dies aus folgenden Gründen:

(1) Das Anwachsen von Phasenübergängen in der Entwicklung komplexer Systeme, namentlich von Organismen und Artefakten, macht es zunehmend unbefriedigend deren Strukturen und Funktionen auf jene vier physikalischen Wechselwirkungen zurückzuführen.

(2) Das "Vier Ursachen Konzept" dagegen ist besser geeignet derartige Phasenübergänge vollständig zu erfassen. Dieses hat sich jedoch, bald nach Aristoteles, in zwei widersprechende Paradigmen zertrennt, indem jeweils eine der vier Ursachen zureichende Erklärungen erlauben sollte: indem die Naturwissenschaft nur mit den Kräften, die Geisteswissenschaft mit den Zwecken operierte.

Heute kommt eine wachsende Zahl von Autoren auf des "Vier Ursachen Konzept" zurück, wobei eine Vielzahl, zum Teil widersprechender Ergebnisse zu verzeichnen sind. Dabei scheint kein Grund zu der Annahme zu bestehen, daß die Welt selbst in vier Ursachen geteilt wäre (ARISTOTELES Phys. 12, 185a 12–14). Nun aber läßt uns die Evolutionäre Erkenntnistheorie erwarten, daß es Schranken und Symmetrien unserer kognitiven Ausstattung sind, welche uns Ursachen in vierfacher Weise begreiflich macht. Faßt man das ins Auge, so wird man nicht nur das "Vier Ursachen Konzept" gerechtfertigt finden, man wird auch um seine scheinbaren Widersprüche herumkommen und es als Anleitung verwenden, um nicht weiterhin in die Falle unerlaubter Simplifizierung zu gehen.

Gary T. Dempsey Nietzsche's Naturalistic Epistemology and some Implications

Der Artikel befaßt sich mit den erkenntnistheoretischen Implikationen der Schriften von Friedrich NIETZSCHE (1844–1900). Es wird behauptet, daß NIETZSCHES Erkenntnistheorie als naturalisierte Erkenntnistheorie charakterisiert werden kann, weil er Erkenntnisvorgänge in enger Beziehung zu Lebensprozessen interpretiert. Erkenntnisvorgänge werden in organismischen Begriffen gefaßt, wobei den Interaktionen mit der Umwelt wie mit den Artgenossen zentraler Stellenwert eingeräumt wird.

Es wird aufgezeigt, daß Nietzsches naturalisierte Epistemologie drei wichtige philosophische Implikationen aufweist.

1. Wissen ist selbstreferentiell

2. Irrtum ist eine Voraussetzung von Wissen

3. Es gibt eine "terra incognita" des Wissens

All diese Implikationen erweisen sich als Folge der konstruktivistischen Qualitäten NIETZSCHES naturalisierter Epistemologie, wobei NIETZSCHE nicht müde wird zu betonen, daß Wissen nicht "selbst evident" ist, sondern als Resultat konstruktiver Akte erscheint. Die Eigenschaften eines sensorischen Ereignisses kommen diesem selbst nicht direkt zu (sie sind keine "intrinsischen Qualitäten" dieses Ereignisses, die dem "Geist" direkt übermittelt würden). Eher treten wir jeweils mit einem bestimmten "Vorauswissen" an die Dinge heran, bevor diese "begriffen" werden.

Weiters werden sechs Punkte angeführt, welche die Beziehungen zwischen NIETZSCHES Erkenntnistheorie und der EE betreffen:

- 1. Menschliche Erkenntnis ist nicht unabhängig von der Realität.
- 2. Erkenntnistheorie muß den Menschen als "Wissenden" mit einbeziehen.
- 3. Unser Wissen reicht nur so weit wie es unsere persönliche Geschichte zuläßt.
- 4. Die Entwicklung von Wissen besteht nicht nur in einer passiven Anpassung an die externen Bedingungen.

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- 5. Wissen verändert den Wissenden und das Wissen über den Wissenden verändert die Welt über die man etwas weiß.
- 6. Unser Wissen über die Welt verläuft nicht progressiv in Richtung "Wahrheit".

Manfred Eickhölter The Glory, Decline and Return of Typus

Zwischen 1750 und 1850 n. Chr. sind in der Biologie konkurrierende Typuskonzepte entstanden. Botaniker und Zoologen erprobten natürliche und künstliche Klassifikationssysteme. Manche Typologien erwiesen sich als Hirngespinste. Weitestgehende Zustimmung fanden bis heute zwei Konzepte: Der, Bauplan" tierischer und verwandter pflanzlicher Organismen. Der Bauplan in der Botanik wird als "diagrammatischer Typus" bezeichnet. Beide Konzepte referieren auf unterschiedlich darzustellende, in der Sache aber einheitliche Gestaltgesetzlichkeiten in der Natur. Ab 1840 wird der Typus am Beispiel eines botanischen Konzeptes von A. P. DE CANDOLLE in der Logik diskutier (WHEWELL). Als Ergebnis etablieren sich zwei Typenbegriffe: die "Grundform" und die "Vollform". In der Folgezeit breiten sich Typenlehren epidemisch in Philosophien und Wissenschaften aus. Allein in der Psychologie wurden bis 1970 ca. 4000 Typenstudien gezählt. HEMPEL/OPPEN-HEIM mustern 1936 und 1968 die soziologischen und psychologischen Typenbegriffe durch und kommen zu dem Ergebnis, daß nur zwei Typen zulässig sind: der klassifikatorische Typus (Grundform) und der relationale Typus (Idealtypus). Gegenwärtig sind Typologien aus vielen Wissenschaften entweder wieder verschwunden oder in den Bereich primärer Heuristik zurückgedrängt Immer aber sind sie umstritten hinsichtlich ihrer erkenntnistheoretischen Rechtfertigung und ihrer Leistungsfähigkeit. Nur zoologische und botanische Biologie arbeiten noch immer erfolgreich mit dem Typus als Bauplan. Er besitzt einen extrem hohen prognostischen Wert, und er stimmt mit weiteren theoretischen Annahmen in der Biologie in einem Maße überein, wie dies für keinen zweiten Typusbegriff zutrifft. Der folgende Text argumentiert für die These, daß die biologische Typologik außerhalb der Biologie kaum rezipiert worden ist. Die in der Logik und in den Wissenschaften zwischen 1840 und 1936 erarbeiteten entstandenen Typusbegriffe sind vermutlich mit den Kriterien für den biologischen "Bauplan" gar nicht verglichen worden. 1973 erscheint die erste Studie, die darauf aufmerksam macht, daß die biologischen Homologiekriterien einen Typusbegriff ermöglichen, der sich weder mit dem klassifikatorischen, noch mit dem relationalen Typus beschreiben läßt. Der Bauplan ist keine Minimalfestlegung gemeinsam auftretender Häufigkeitsmerkmale und auch kein mehr oder weniger an Typus. Es wird erwartet, daß eine Auseinandersetzung mit den strengen Typuskriterien und dem Typussystem der Biologie fundierte Typusbegriffe auch in anderen Wissenschaften ermöglichen wird, in denen die Frage nach Gestaltgesetzlichkeiten zu beantworten ist.

Robert Pallbo An Inquiry into Meno's Dilemma

Die Arbeit bezieht sich auf ein altehrwürdiges Problem der Philosophiegeschichte welches unter der Bezeichnung "MENOS Dilemma" in den PLATONI-SCHEN Dialogen auftritt. Es geht dabei um den Erwerb neuen Wissens und die damit verbundene Frage wie dieser Erwerb vor sich geht. PLATON geht dabei von der Annahme aus, daß wir bereits immer über ein enstprechendes "Vorauswissen" verfügen, welches den Erwerb "neuer" Erkenntnis immer bloß als "Wiedererinnerung" an bereits Gewußtes erscheinen läßt. Am Beispiel der Gerechtigkeit versucht PLATON dies insoferne deutlich zu machen, als er behauptet, daß jegliches Urteil über "gerecht" bzw. "ungerecht" bereits ein intuitiv gegebenes Wissen um Gerechtigkeit voraussetzt.

In dieser Art und Weise formuliert legt das "Dilemma" die Existenz eines Subjekts, welches die Suche vollzieht nahe. Dabei wird dieses suchende Subjekt als von dem Wissen welches es erwirbt bzw. besitzt getrennt gedacht. Diese Sichtweise wird bestritten und die Behauptung aufgestellt, daß diese Trennung nicht nötig ist. Die Vorstellung des menschlichen Geistes als eines evolutionären Prozesses macht es möglich ein Agens zu vermeiden, welches nach Wissen strebt und Wissen strukturiert. In der gleichen Weise vermeidet das Konzept der biologischen Evolution ein Agens oder einen Gott, welcher die Arten erschuf.

Der Artikel skizziert mögliche Mechanismen, welche bei der Erzeugung von neuen Gedanken eine Rolle spielen. Ein besonderes Augenmerk wird dabei Variationsmechanismen zugesprochen. Im Verlauf der Evolution müssen dabei diese Mechanismen zunehmend komplexe Strukturen entwickeln, ohne dabei selbst zu komplex zu werden.

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Es wird davon ausgegangen, daß dieser basale Variationsmechanismus in der spontanen und stochastischen neuronalen Hintergrundaktivität des Gehirns gründet, die in den meisten Gehirnabschnitten vorgefunden wird.

Einen anderen zentralen Faktor stellt die Selektion dar. Diese wirkt über die sensorischen Eindrücke auf die Spontanaktivät des Gehirns ein und führt zur Aufrechterhaltung und Stabiliserung bzw. Unterdrückung bestimmter interner, spontaner Aktivitätsmuster. Die hier vertretene Sichtweise von Kognition geht davon aus, daß Ordnungsmuster des menschlichen Geistes durch ein Zusammenwirken dieser Mechanismen entstehen, wobei die "Umwelt" einen aktiven Bestandteil des kognitiven Prozesses darstellt. Indem der menschliche Geist nicht mehr als aktiv Suchender konzipiert wird, der einer unbekannten Umwelt gegenübertritt, an der gelernt werden soll, wird das MENO'sche Dilemma vermieden.

Steven J. C. Gaulin, Irwin Silverman, Krista Phillips, Christine Reiber Activational Hormonal Influences on Abilities and Attitudes: Implications for Evolutionary Theory

Diese Studie basiert teilweise auf SILVERMANS und EALS Theorie, nach der das bessere Abschneiden männlicher Versuchspersonen bei räumlichen Tests auf der Arbeitsteilung zwischen Jägern und Sammlern im Verlauf der Evolution der Hominiden zurückzuführen ist, sowie dem Nachweis, daß Frauen Männer in räumlichen Tests übertreffen indem sie deren Rolle als Nahrungsbeschaffer nachahmen.

Das beinhaltet die Wiedererinnerung an Objekte sowie deren Anordnung im Raum. SILVERMANS und EALS Test wurde an 120 weiblichen und 89 männlichen Studenten durchgeführt. Der Test beinhaltete auch Aufgaben welche die Fähigkeit zur dreidimensionalen mentalen Rotation überprüften, wobei zahlreiche Studien ein besseres Abschneiden der männlichen Versuchspersonen nachwiesen. Schließlich wurde noch ein Einstellungswert in die Untersuchung miteinbezogen, der geschlechtsspezifische Unterschiede – wie sie anhand Buss evolutionärer Theorie postuliert wurden – aufzeigte. Es ging dabei um die Frage ob die Versuchspersonen eher zu sexueller oder emotionaler Eifersucht tendieren, wobei erwartet wurde, daß weibliche Versuchspersonen eher zur emotionalen Eifersucht tendieren, während die männlichen Versuchspersonen stärker zu sexueller Eifersucht neigen.

Die erwarteten geschlechtsspezifischen Unterschiede wurde bei allen vier untersuchten Eigenschaften in statistisch signifikanten Ergebnissen vorgefunden. Bei den weiblichen Versuchspersonen wurde darüberhinaus eine signifikante Korrelation zwischen den Ergebnissen bei den Aufgaben zur mentalen Rotationen und den gemessenen Eifersuchtswerten festgestellt – was bei den männlichen Versuchspersonen nicht zutraf.

Bei den weiblichen Versuchspersonen bestand eine signifikante Beziehung zwischen der Höhe des Östrogenwertes (im Zusammenhang mit der Menstruationsphase) zu den Ergebnissen der Rotationsaufgaben und der Eifersuchtstests, wobei höhere Östrogenwerte stärker mit den erwarteten weiblichen Meßergebnissen übereinstimmten, dabei also schlechtere Ergebnisse bei dem Rotationstest erzielt wurden und stärkere Tendenzen in Richtung emotionaler Eifersucht – versus sexueller Eifersucht – gegeben waren.

Entgegen den Erwartungen bestand keine positive Korrelation zwischen Östrogenwert und den Meßergebnissen der räumlichen Tests. Es wurden sogar Korrelationen mit Signifikanzniveau, welche in die gegenteilige Richtung weisen festgestellt.

Betrachtet man die Ergebnisse in ihrer Gesamtheit so unterstützen sie die Annahme einer evolutionären Basis von Geschlechtsunterschieden. Es wurde auch die Frage erörtert ob die in der Studie festgestellten aktivierenden Effekte hormoneller Einflüsse evolutionär adaptive Funktionen beinhalten, die unabhängig von organisatorischen Effekten gegeben wären.