If schemas are considered as units representing and interacting with the world, then it may be possible to represent human thought and behavior with an adaptive schema network. Fortunately, certain functions and their corresponding subnetworks are clearly hierarchical. Schemas located at the same level would interact by competition and cooperation, whereas schemas at different levels would act by maintaining the proper sequence. For example, motor control – from the motor cortex to the cerebellum, brain stem, spinal cord, and muscle servo – is clearly hierarchical. [See Stein: "What Muscle Variable(s) Does the Nervous System Control in Limb Movements?" *BBS* 5(4) 1982.] Visual information processing in the frog and the toad is another example. On the other hand, in each level of the hierarchy there are many concurrent activities, for instance, the information processing in retina. The brain accordingly processes and integrates information in both sequential and parallel distributed ways. Arbib's operations on schemas reflect this feature, such as compounding (several schemas compounded are a more powerful new schema), generalization, instantiation, and activation (data-driven or goal-driven).

I think it is important to find more methods for representing schemas. Semantic networks are successfully used to represent structured, well-learned knowledge in artificial intelligence, but they are suitable only for the analytic mode of thinking that breaks the object up into parts and then combines them. Dividing the whole into parts may lose much information. There is another mode ("imagistic thinking") whose function is to process the object as a whole (Qian 1983). This helps to abstract similarities among objects, as used in analogies and logical inference. What schema representation will facilitate such analogies and abstraction in this mode of thinking? It is likely to be quite different from those that are suitable to the analytic mode.

The ideal of using the schema concept as a bridge is in an early stage of development and perhaps somewhat premature, but worthy of further exploration.

Schemas: Not yet an interlingua for the brain sciences

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Arbib argues for the utility of schemas as intermediate representations between neural-level analysis and overall brain behavior. He presents examples on how to move from schemas to testable models of neural circuitry, proposing that schemas can provide a meeting ground for the behavioral and brain sciences. Unfortunately, there is no reason to believe that there is only one requisite level of intermediate representation. I agree that there is a need for a common language of discourse, and for stating theories. Only with a common formalism can theories from one discipline contribute to development in another. In other sciences – for example, physics – the language of mathematics forms this common formalism. However, schemas, as Arbib uses them, do not yet provide this interlingua for the brain sciences.

The key problem with Arbib's schemas (as well as with Minsky's frames [Minsky 1975], Schank's scripts [Schank & Abelson 1977], and the other versions of packaged knowledge) is that a syntax and semantics for the formalism are not provided. Arbib only provides a set of four constraints on the characteristics of schemas. It is difficult to argue against the utility of representation or of knowledge packages in general; they have so many advantages and these have appeared in many papers (a partial summary appears in Tsotsos 1984). There is a need, however, to introduce the concept to other domains because, although it is perhaps well accepted in the computer science community, it may not be so well accepted in other disciplines. The designer of a representation must determine how appropriate the representation is for the problem domain, and must give it a syntax and formal semantics so that others may adopt it as well. A common formalism is useful only if other researchers use it, if one can compare different representations of the same problem solution, and if one can distinguish correct from incorrect representations. Throughout most of the target article, Arbib uses schemas in an informal manner; a great many questions arise about schemas in general and about the representation of specific constructs. The general questions are:

How are schemas mapped onto an implementation for testing purposes? If they are part of a programming language, is there a compiler or interpreter for the language?

How can schemas proposed for the same behavior be compared without simulation?

What class of behaviors does a given schema represent? How can one tell whether that class of behavior is the desired one, and contains no other unwanted behaviors?

How can one tell whether a given schema formalism is sufficient for all the behaviors of interest, and only those?

The specific questions are:

Is a schema a procedural or declarative entity? Does this matter?

Are the arrows between units in a schema control lines, data lines, or do they represent some other relationship?

What are the primitives of schemas? Is it possible to define a set of primitives so that all behaviors of interest can be represented using combinations of these primitives?

In motor tasks, temporal issues are of particular importance. How is time represented in the schemas?

How are motion qualities, such as "grasp gently" or "grasp aggressively," represented?

What are the subtleties of vision that the language of Arbib's Appendix B cannot capture, in addition to not handling control concepts? Are they not critical?

It is claimed that the "FORALL" construct is novel; it appears previously at least in my own work, and definitely in my 1984 contribution to Arbib and Hanson's edited volume (Tsotsos 1987). In fact, the work of Weymouth described in Arbib's Appendix A is very similar to the representation and control framework for vision I had put forward (Tsotsos 1980) and further refined (Tsotsos 1985; 1987). In fairness, however, my own work does not provide answers to most of the questions I have raised here (and no one else's does either, I believe).

The problems with schemalike and semantic net representations were first noted in seminal papers by Hayes (1974) and by Woods (1975). Further analyses are found in papers by Brachman and by Hayes (Brachman 1983; Hayes 1979). These papers launched a very different emphasis in the knowledge representation research community: that of providing representational formalisms with a logical semantics. (See Mylopoulos and Levesque [1984] and Levesque [1987] for overviews of knowledge representation research and Brachman and Levesque [1985] for a collection of key papers in the field.)

The need for a common language of discourse among the artificial intelligence, cognitive psychology, and neuroscience communities is critical, particularly with the current burgeoning interest in many centers in interdisciplinary research. In this regard, Arbib is completely right, and I regard his proposal of schemas as a very reasonable starting point. The questions I have raised are not so much criticisms of Arbib's work but indications that much more work remains to be done. It is appropriate to examine the success of mathematics as a common language for the physical sciences. Mathematics has an agreed-upon formal semantics. A family of compatible representational tools is provided, each suitable and appropriate for a different problem task (arithmetic, algebra, geometry, topology, probability, and statistics, calculus, analysis, set and number theory, logic, combinatorics, and others). I feel it is a mistake to expect that a single representational formalism can be found that will be sufficient and appropriate for all levels of analysis of the brain. I believe that a family of representations is required in much the same way as a family of representations and tools has been found to be appropriate for the physical sciences. The members of this family must be compatible with one another in the same way that the different languages of mathematics are compatible. Schemas or their derivatives are certain to play a role at some level of representation.

Schemata and representational constraints

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The target article gives me the impression that the author is telling the wrong story at the right time. Arbib acknowledges a point not generally accepted by neurologists – that a functional explanation of a neurological system is necessary in order to capture the relevant aspects of its structure. He therefore accepts a cognitivist type of description as intermediate between the behavioral level and the neurological one. Cognitivism identifies functional description with formal description. The formal descriptions involve computations on representations. Since "the eventual goal, of course, is for functional and structural analyses to be rendered congruent," it comes as no surprise that Arbib seeks to combine neural circuitry into more complex ensembles called "schemata." However, he does not wish to accept the computational level of explanation as a separate level, because "different animals (or different subsystems of a given animal) may make different use of visual cues that cannot be discovered until 'implementational details' are taken into account." If the role for computational notions is merely a heuristic one, they will ultimately be eliminated from the theory. It seems, however, that they are meant to stay, since "For many behaviors, analysis at the level of single neurons may be superfluous." Arbib therefore seems willing to have the best of both worlds, without even mentioning the conceptual problems involved in identifying the computational level of explanation with a certain type of neurological hardware.

But let us dream of such an account with all these problems solved. Basic steps in the computations are carried out by the underlying neurological system. We assume that its "implementational details" are simply "facts" about the neurological hardware. We assume that these facts indeed constrain our elementary computational steps. Then why do schemata need to play the role of the elementary computational processes provided by the neurological hardware? They seem far from elementary processes.

If this is the point Arbib wishes to make, my critique boils down to the following: The question of which processes are carried out depends on the question of which representations can be stored and retrieved. Constraints on representations are therefore at least as fundamental as those on processes. An interesting theoretical possibility lies in imposing neurological constraints that have consequences for representations. Satisfaction of these constraints would be a prerequisite for the ascription of meaning to representations in a cognitive explanation. This means that the constraints themselves should be formulated independently of any semantic domain. Any domain-dependent formulation of the representational properties is beside the point. Schemata are, alas, domain-dependent entities, and can therefore not be expected to serve as a basis for understanding the neurological constraints on computational theories.

Schema theory: A new approach?

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As his books have demonstrated, Arbib is familiar with theories no matter how complicated they are. One therefore wonders what impels him to construct models – for example, models of simple toads—that do simple things in simple experiments. Many physicists have avoided doing just this and have immigrated into brain research (e.g., from thermodynamics) with the aid of complicated equations. On the basis of similarities between "physical" and "neuronal" structures, they have then attempted to explain or at least to imitate brains. These attempts have been "successful": They have led many scientists to think about information processing in terms of a kind of neuronal "architecture" and thereby to solve problems such as pattern recognition or optimization algorithms that are useful in technical applications. However, such work has repeatedly arrived at the conclusion that almost nothing about the brain can be explained in this way. Given the conditions of the real world, the equations in physics are approximations that are frequently inadequate when boundary conditions alter. Physics is certainly essential for the understanding of biology, but in its conceptual methods rather than in uncritical use of its equations. The result is that there are many formulas in brain research today, but few concepts in which theories can take root.

In my opinion, Arbib's target article presents a concept that could be seminal. It is justified eloquently and illustrated with many examples. In order to comment on it – insofar as I have understood it – I would like to "distort" it a little as seen from my own point of view: To describe brain functions, a certain level of abstraction is necessary. Arbib chooses basic schemata, which have to be constructible in neuronal architecture that is technically measurable. As a help in finding and defining basic schemata, I think they should be interpreted as "basic situations." These consist of sensory or motor cues or both. To perceive and handle these situations – for example, prey-detecting and prey-catching – is the task of the animal. By combining basic situations, more complex behavior can be generated. The spatiotemporal behavior of layered feedback structures can be used to encode and combine such representations, as Arbib shows for the stereo problem. This approach assumes that brains are not universal computers but rather systems that must solve concrete problems in a specific environment, that must be capable of evolution, and that can be constructed reliably under the constraints of ontogeny. The development of *one* new schema allows a large number of combinations with already existing ones, and the requisite *hierarchies* are easy to construct. Compared with the algorithmic approach, this concept, based on the structure of the relevant task and neuronal networks, has the advantage of using experiments that can be currently implemented as well as computer simulated.

This approach of course also involves problems not solved in the target article:

1. How can basic schemata or basic situations be defined and systematically detected?
2. What strategy is used to couple the basic schemata when a target function has to be defined?
3. Is there a general and workable data format for such systems?
4. Is self-organization possible?

I think it is worth proceeding in the direction Arbib has outlined. The path is difficult, but it is the one physics has used since the beginning of experimentation.

The computer simulations tend to conceal a conceptual detail I think should be reexamined: the columnar structure of neuronal networks. This has not been detected neuroanatomically