Brain and Mind **3:** 229–242, 2002.

Decomposing the Mind-Brain: A Long-Term Pursuit

WILLIAM BECHTEL
Department of Philosophy, University of California, San Diego, La Jolla, CA 92093, USA

(Accepted: 26 April 2002)

Abstract. This paper defends cognitive neuroscience's project of developing mechanistic explanations of cognitive processes through decomposition and localization against objections raised by William Uttal in *The New Phrenology*. The key issue between Uttal and researchers pursuing cognitive neuroscience is that Uttal bets against the possibility of decomposing mental operations into component elementary operations which are localized in distinct brain regions. The paper argues that it is through advancing and revising what are likely to be overly simplistic and incorrect decompositions that the goals of cognitive neuroscience are likely to be achieved.

Key words: cognitive neuroscience, decomposition, localization

Many of the major advances in the functional disciplines of the life sciences have stemmed from the discovery of ways of decomposing a phenomenon of interest. The goal of such research is to find functional components that map onto the system's structural components. From these one can try to assemble a mechanistic model of how the system produces the phenomenon of interest. Discovering such functional components in natural biological systems is never easy. In normally operating systems the identity of the components is concealed by their smooth coordination in performing the system's overall activities. Often this smooth coordination involves non-linear interactions of large numbers of components. In the various biological disciplines there have been skeptics who, recognizing the shortcomings of the initially proposed decompositions and the hopelessness of providing an adequate explanation of the overall function with just those components, advocated abandoning the quest to discover an underlying mechanism. William Uttal is in distinguished company of such scientists as Louis Pasteur and Xavier Bichat when he raises such concerns about current pursuits in cognitive psychology and cognitive neuroscience. But in other biological disciplines it was those scientists who put forward the first crude hypotheses about component functions that inaugurated the inquiries that have led to the by now quite well established accounts of such phenomena as oxidative phosphorylation and protein synthesis in living cells. I will argue that despite the clear shortcomings of both current research tools and current proposed decompositions, it is those researchers boldly attempting to decompose mental function that are preparing the route to eventually understanding the mind-brain.

Uttal's critique of the current state of cognitive neuroscience is extremely probing and should be taken very seriously, especially by those who would propose utilizing current theories for applied or therapeutic functions. The techniques, such as analysis of deficits stemming from lesions or imaging of cognitive performance, are fraught with numerous difficulties that render current proposals of localized mental functions far from reliable. Uttal has done a great service by cataloguing some of the most serious challenges confronting the localization of function. The fact that current research is plagued by these difficulties means that the results of current investigations in cognitive neuroscience should be viewed as early hypotheses as to what the component operations are and how they map onto component structures, not definitive answers. But whereas Uttal sees these difficulties as raising potentially impenetrable barriers to a cognitive neuroscience based on localization of function, I will argue that they resemble the early stages in other investigations in the life sciences that have now yielded relatively mature models of mechanisms. Although it is possible that the mind-brain will be a system unlike these others and that the prospects of successfully decomposing the mind-brain are futile, we do not yet have reason to abandon the quest.

At the core of Uttal's negative assessment of the prospects of cognitive neuroscience is a critique of psychological decomposition. This is, indeed, the decisive issue. If the cognitive system is not functionally decomposable, then current projects in cognitive neuroscience are bankrupt. Thus, I will begin by examining Uttal's arguments against psychological decomposition.

1. Psychological Decomposition

Uttal contends that the psychological processes into which cognitive theories propose to decompose the mind have the status of hypothetical constructs: "Many mental entities turn out on close inspection to be hypothetical constructs whose reality is impossible to validate because of the intrinsic inaccessibility of mental processes" (p. 15). I am not sure exactly what Uttal thinks is significant about the claim, which he advances more than once, that proposed mental entities (or processes) are hypothetical constructs. Any proposal to decompose an overall activity of a system into component operations is hypothetical. The proposals to decompose physiological processes such as fermentation and oxidative phosphorylation into biochemical reactions were hypothetical, as were the proposals to decompose chemical compounds into atoms. Many of the constructs that figured in early theories of these processes turned out not to exist (witness phlogiston) or not to figure in the process (methylglyoxal). The question is whether cognitive theorists can hope to find evidence supporting, refuting, or, most importantly, prompting revisions in the proposed decompositions.

Uttal further misrepresents most cognitive psychologist's attitude towards these constructs: "The main difference between behaviorism and cognitive mentalism is their respective attitudes toward the question of the *accessibility* of mental processes. The behaviorists asserted that mind is not directly accessible, whereas the cognitivists asserted that it is" (p. 95). But cognitivists no more than behaviorists think that the component operations of the mind are directly accessible. Cognitive psychologists, like behaviorists, rely on behavioral measures. The component operations they hypothesize are at best indirectly accessible and the challenge cognitivists confront is to generate sufficient indirect evidence to constrain the possible decompositions. This is why measures such as reaction times and error patterns are so frequently appealed to in studies in cognitive psychology. If the pattern of reaction times discovered is not compatible with a particular proposed decomposition of a cognitive task, then that decomposition needs to be revised. Reaction times and other features that can be measured behaviorally provide only relatively weak constraints on possible decompositions. The prospect that the tools coming from neuroscience may provide additional constraints is one of the reasons many cognitivists have been enticed into the collaborative endeavor of cognitive neuroscience. Whether and how neuroscience tools can provide such constraint is a topic to which I will return.

Before proceeding further, though, it is important to differentiate two types of decomposition that are not usually distinguished – phenomenal decomposition and mechanistic decomposition. Uttal is skeptical of both, at least when they are linked to attempts to localize what is decomposed to brain areas, but only one of these is appropriately related to the localization project. A phenomenal decomposition differentiates the different phenomena a system exhibits and that need explanation. A cell may transform energy, make proteins, dispose of waste, divide, etc. A person might engage in different mental activities – perceiving, reasoning, language use, decision-making, etc. The attempt of faculty psychology to differentiate different faculties of mind is an exercise in phenomenal decomposition. Such a decomposition is a useful prolegomena to developing a mechanistic account of the operation of the mind insofar as it leads us to focus carefully on what are the activities of the mind which we seek to explain. But in itself it offers no explanation and is not generally a good basis from which to map to the underlying substrate (although in some cases, such as that of the cell, different components are responsible for different activities). First, as Uttal notes, there are likely to be a host of different processes involved in any of these activities. Hence, in the case of the brain, these activities are likely to draw upon widely separated areas of the brain and about all we can say is that much of the brain is involved in language use or much of the brain is involved in problem solving. Second, there here is no reason to expect that a distinct set of processes is invoked in each of these activities. Generating verbal descriptions and encoding memories may invoke the same set of processes (see Gabrieli *et al*., 1998 for suggestive evidence along these lines). Finally, and perhaps most crucially, such decomposition does not provide explanation. After we assign a cognitive ability to a faculty, we do not know anything more about how it was performed. For explanation, mechanistic decomposition is required.

Mechanistic decomposition of an activity involves identifying component processes which contribute to the overall performance and are recruited when the activity is performed. In the case of cell physiology, a particular biochemical reaction may contribute to an overall activity such as fermentation (moreover, it might also be invoked in carrying out other cellular activities) and it is by identifying these reactions and the way in which they figure in the overall process that biochemists began to explain processes like fermentation. Cognitive psychology's quest (as well as that of neuropsychology) has typically been to identify such component processes underlying processes like language use or reasoning. The invocation of neuroimaging is typically directed towards both identifying component processes and identifying them with brain processes (in a manner I will sketch latter, it does not just assume a decomposition into component operations, but can contribute to the discovery of the most fruitful decomposition).

What is critical here is that the goal of cognitive neuroscience is the decomposition and localization into the component processes that figure in mental tasks, not the localization of the tasks themselves. Petersen and Fiez, for example, stress that neuroimaging should be seeking to identify *elementary operations*, not tasks. There is, admittedly, a strong, fallible, assumption that the system is divided into components performing different elementary operations. Petersen and Fiez make this assumption clear:

> The areas involved in performing a particular task are distributed in different locations in the brain, but the processing involved in task performance is not diffusely distributed among them. Each area makes a specific contribution to the performance of the task, and the contribution is determined by where the area resides within its richly connected, parallel, distributed hierarchy (Petersen and Fiez, 1993).

It is indeed possible that task performance is diffusely distributed in that areas do not make specific contributions to a task, but some more general overall contribution. It is for this reason that Richardson and I (Bechtel and Richardson, 1993) characterize both decomposition and localization as heuristics – they are based on fallible assumptions about how the system is put together.

The issue between Uttal and those advocating pursuit of mechanistic explanations of mind is clearly drawn: Uttal bets against the contention that the mind can be decomposed into component or elementary operations which in turn can be localized in brain areas whereas the mechanist bets that it can. But we need to clarify this wager further. Uttal suggests that the correct decomposition must be arrived at before localizing operations in the brain. But, as I shall argue in subsequent sections, progress in other sciences has resulted from beginning with tentative decompositions and letting the localization process guide revisions of the initial decomposition. Accordingly we can view the current proposals for decomposition of mental abilities into elementary operations as tentative hypotheses that are subject to major revision as the project of cognitive neuroscience proceeds. In particular, discovery of non-linear modes of organization are likely to result in major revisions to hypotheses about what components do. The wager thus is not about whether the initial decomposition correctly characterizes the component operations, but whether there are component operations to be found and whether the sort of research programme cognitive neuroscience is pursuing can hope to find them.

Before moving on to analyzing the project of actually attempting to find component or elementary operations, we need to get a bit clearer about what they might be. In machines that we build engineers typically begin with a repertoire of components they already have available and their effort is directed at putting them together in an appropriate way to accomplish the task at hand. Likewise, scientists trying to unravel the workings of a mechanism typically start with a repertoire of possible component operations (Machamer *et al*., 2000 refer to these as a *store*). For example, in trying to build models of biochemical pathways, biochemists work with known types of biochemical processes – oxidations, reductions, phosphorylations, etc. The challenges are to figure out possible ways through which these processes might be linked together to accomplish the overall task and then determine whether those are the ways the system in question does it. But the repertoire of possible component operations is not predefined. It must be discovered.

It is also important to be clear about how elementary the operations in question are to be. In one sense, work in basic physics spells out elementary operations in terms of which all phenomena in nature are comprised. But this is typically not the level at which we find the operations that are to figure in the explanation of a given phenomenon. To explain how a car generates locomotion we do not jump immediately to quantum mechanics. Rather, we appeal to parts at one level of decomposition down from the whole car – the engine, drive shaft, axles, etc. Each of them makes a contribution which we can understand in light of the goal of generating locomotion – transforming chemical energy to mechanical energy, etc. We might then want to explain how the engine works by taking it apart into the next level of parts – the cylinders, pistons, rods, etc. What is needed for mechanistic explanations then are the parts a level of organization below the level of inquiry.

Identifying the parts and what they do at the appropriate level of organization may not be easy. Researchers in physiological chemistry working between 1850 and 1900 knew about the elemental chemical composition of living organisms. But this was not the level needed to explain biochemical reactions. Processes at an intermediate level – oxidations, reductions, phosphorylations, etc. – had to be identified before explanations could be advanced. That is, the repertoire had to be created. This involved both conceptualizing the entities and assigning them names and required difficult theoretical work. Previously I have argued that cognitive psychology is likely to be in the same predicament as physiological chemistry before the useful repertoire of basic operations was developed (Bechtel, 1994).

There is vocabulary to characterize the cognitive activities of agents and vocabulary for characterizing the activities of neurons and, to a more limited degree, ensembles of neurons. But the vocabulary to characterize the component operations at the level out of which cognitive activities are constructed has yet to be developed. Researchers often try to construct a set of operations out of which an activity might be comprised. For example, remembering is characterized as requiring encoding, storage, and retrieval. But these activities are specified in terms of memory and while they may be real, probably do not characterize the level of elementary operations out of which memory, problem solving, language use, etc., are comprised. The fact that we are confronting a missing level of organization about which we cannot yet say much, however, is not a reason to despair. In the face of the same problem, physiological chemists continued their experimental and theoretical work, characterizing the intervening operations in fermentation, for example, as intermediate fermentations. Through this work much was learned which facilitated the discovery of the elementary operations relevant to explaining physiological processes, and we can hope for similar development in the case of mental processes.

2. Using Localization to Guide Decomposition

As I indicated at the outset, in the history of the life sciences those who rejected mechanistic explanations often correctly pointed to the clear inadequacies of then current conceptions of the mechanism responsible for a given phenomenon, but it was those who advanced the proposals that prepared the way for future advancement. A common first step was in fact to try to localize an activity defined by a phenomenal decomposition. For example, researchers sought the locus of the generation of animal heat in a specific organ or of fermentation in a specific enzyme. But efforts to evaluate these proposal empirically frequently yielded the discovery that multiple components were involved and led researchers to begin proposing mechanistic decompositions. Typically they began by envisaging a linear mode of organization and breaking the overall process into a series of stepwise operations. Sometimes researchers have even had good theoretical reasons to offer a linear model. Fermentation breaks a complex molecule into simpler molecules and in 1900 it seemed plausible that each step would involve a particular cleavage of the complex structure until the final products (e.g., alcohol, lactic acid) were obtained.

In trying to develop a model of fermentation, researchers had to satisfy a number of constraints. First, the individual chemical reactions had to be ones that had already been identified as actually occurring. Second, the intermediate products had to be ones that could be independently identified, ideally by isolating them in the reacting media. The goal was to satisfy these constraints by proposing a series of operations on sugar that would yield alcohol. Even the first coherent proposal, that by Carl Neuberg, sacrificed linearity since the simplest reaction schema he could envisage utilized products produced later in the pathway in reactions earlier in the pathway. Empirical challenges such as this are one of the major factors that lead scientists to identify non-linearities in physical systems. Neuberg's schema, however, faced a serious empirical problem – one of the hypothesized intermediates, methylgloxal, could not be injected into the reacting system so as to generate alcohol as it should if it were an intermediate. Another important failure was that his schema could not account for the need to continually add phosphates to the experimental setup.

Research in the 1920s and 1930s led to a reconceptualization of the fermentation process. First, it was discovered that a phosphorylated compound, adenosine triphosphate (ATP), was both needed to initiate the fermentation process and was produced by the process. Second, it was recognized that the critical intermediates in the pathway were phosphorylated. With these clues, a model of the reaction pathway was developed that met the above constraints. But the changes were in fact major. The introduction of the roles played by ATP and by a coenzyme NAD resulted in the recognition of even more non-linearities in the process. Moreover, the goal of fermentation came into focus – it served to transfer potential energy from glusose to ATP, which could be used in cellular work. In light of this, one of the critical roles of the non-linearities became apparent – the availability of ADP, from which ATP could be created, regulated the fermentation process so that fermentation occurred only when energy was required to make ATP for use in energy consuming processes performed by other cell components (which would break it down to ADP) (for a more complete account of these developments, see Bechtel and Richardson, 1993).

My point in presenting this quick historical narrative is that hypothetical decompositions of complex processes, even when they are wildly inaccurate, can provide a critical foundation for discovery of more adequate models of mechanisms. What was critical was that the original hypothesis made claims about what operations should be localizeable in the experimental setup. One could then investigate whether components performing such operations existed. In this case research could not reveal them but led to the discovery of other components for which the initial scheme did not provide a role. This in turn led to reconceptualizing the process and discovery of different component operations. Numerous other examples from other parts of biology as well as chemistry can be offered in which localizationist research initiated with overly simple and erroneous decompositions spurred discoveries that pointed to revisions that ultimately yielded far more adequate decompositions. The one thing that is necessary in an overly simple initial hypothesis is that it suggest a line of research that can reveal its own inadequacies and hence point to profitable revisions.

The implication of this for work on cognition is that attempts to localize hypothesized mental operations in the brain prepares the ground for revisions in the decomposition of a particular mental activity. Thus, we need to view the project of decomposing cognitive activity as a long-term one that is carried out in conjunction with attempts at localization. It is not something that we can expect to be adequately worked out before localization.

3. The Heuristic Value of Identity Claims

As Uttal emphasizes, underlying the program in cognitive neuroscience is a commitment to psychoneural identity claims – claims to the effect that an entity identified functionally in a cognitive analysis is identical to an entity identified structurally in neurophysiological terms. Psychoneural identity claims lie at the center of one of the traditional solutions to the mind-body problem – the mind-brain identity theory. After a period of popularity in the 1960s, identity theory was largely eclipsed in philosophy of mind by a position known as *functionalism* (in what follows I will speak of *functionalism without identity*, since, as Lycan (1987) makes clear, the basic commitments of functionalism are also compatible with adopting the identity thesis). Functionalism without identity (Fodor, 1975; Putnam, 1967) agreed with the identity theory that mental processes could be decomposed into component operations, but rejected the claim that these operations could be identified with particular brain processes. One of the major arguments for a functionalism without identity is the claim that cognitive processes are multiply realizable. More recently a number of philosophers, myself included (Bechtel and Mundale, 1999), have contended that multiple realizability claims are not nearly as telling as they were once thought to be and that psychoneural identity claims are plausible. But it is important to appreciate more fully the status of such identity claims.

Often the establishment of identity claims is taken to be the end of research. Following the lead of Wimsatt (1975), McCauley and I have argued that identity claims are made early in a research program and serve as heuristic for further research (Bechtel and McCauley, 1999; McCauley and Bechtel, 2001). A major component of McCauley's and my motivation in emphasizing the heuristic character of identity claims was to counter objections to identity claims that one could never prove more than a correlation between the entities characterized in the two vocabularies (Kim, 1964). No evidence could ever demonstrate that they were not just correlated but were identical. We contend that identity claims in other sciences are not proven in this sense. Rather, identity claims are advanced and if they bear fruit in terms of patterns of discovery in each of the two initially independent theoretical frameworks, they are woven into the fabric of the science. The request to prove the identity never arises. In response to Uttal, however, there is a related point to emphasize. Identity claims are strong and falsifiable claims. If x is identical to y, what one theoretical framework says of x, the other theoretical framework must say (in the appropriate vocabulary) of y. Such a claim about y may not be true. But the heuristic is to attempt to show that it is true using the tools established to investigate y. If it is, the theoretical understanding of y is expanded in the process.

Wimsatt describes what happened after investigators proposed that genes were identical with components of chromosomes. One of the discoveries about genes were that they were linked in that some genes were inherited together. But the relationship was not perfect and researchers were able to work out frequencies at which alleles at given sites would be inherited together. This suggested that linked genes were on the same chromosome and that failure of perfect linkage would be due to crossing over of the corresponding parts of the paired chromosomes during gamete formation. It seemed natural to assume that genes whose linkage was greater would be closer to each other on the chromosome than those whose linkage was less. This provided a basis to try to map the location of genes on chromosomes. But a problem arose. On this scenario, for genes located in the order A, B, and C on the chromosome, the frequency of crossover between A and C ought to be a sum of the frequency of crossover between A and B and between B and C. But it was less. This suggested the possibility of two or more crossovers. This might seem like an ad hoc rescue of the identity claim, but in fact it advanced an empirical hypothesis. Although the hypothesis might well have been false, in this case cytological evidence suggested it was true.

A major aspect of the heuristic power of an identity claim arises when the initial attempt to map what is claimed of *x* onto what is claimed of *y* fails, but is close enough to support a revised claim about *y*. If the theoretical framework characterizing *y* is revised in this way, one must now consider a revision in the theoretical characterization of *x*. If, using the tools for investigating *x* support the new claims about *x*, the heuristic has again proven its value. This iteration of revising *x* and *y* may recur over many cycles. It may also fail and the claims about *x* and *y* may end up diverging. Then the identity claim must be surrendered. But when it does work, it provides a powerful way to learn more about *x* and *y*.

We can now see how psychoneural identity claims can guide cognitive neuroscience. Cognitive psychologists, typically restricted during the 1970s and 1980s to using behavioral measures such as error patterns and reaction times, attempted to test various processing models. Uttal notes that these models were generally underconstrained by these data. (He also suggests that such models are in principle always undercontrained by any possible data. But if that were a serious worry, then not only the attempt to develop models of cognitive activity but also the attempt to explain any phenomena in the life sciences and any functionally characterized parts of the physical sciences would also be undermined. Moreover, the fact that it is generally extremely difficult to develop a model of a mechanism that satisfies even a minimally rich set of data indicates that the in principle availability of an infinite set of models may not pose any practical concerns.) This is one reason to try to identify these cognitively characterized processes with particular brain activities. Now we can consider the relationship between the activity both as characterized and examined with cognitive tools but also as characterized and studied with neural tools. The cognitive theories now make claims about the organization of neural activity and the truth of these claims becomes an additional constraint on processing models. If we find that the brain activity identified with two cognitively identified processes that are linked in cognitive models are not linked in terms of brain activity or neuroanatomy, then a revision in the cognitive model may be required. But the benefits are not just one-sided. An account of the cognitive processing in a given task can guide the search for neural relations not previously anticipated.

Allowing for such cycles of revision may lead some, including Uttal, to suspect a disastrous circularity. In discussing attempts to map psychological characterizations onto brain characterizations, Uttal, echoing claims of van Orden *et al.* (2001), suggests that such research is bound to generate confirmations. This can only happen, however, if the investigatory tools utilized in developing the two theoretical frameworks are not themselves sufficiently advanced to yield potentially falsifying data. One of the major reasons for trying to identify cognitively characterized processes with neurally characterized processes is to introduce a new set of tools for constraining, and potentially falsifying or forcing revision in psychological processing models. There are a number of ways this can happen in addition to the one suggested in the previous paragraph. If, for example, two cognitive models suggest that the same component operation is invoked in two different tasks, and on the basis of neuroimaging with one of the tasks we identify that operation with a particular brain region, then we should expect the same region to be active in the other task. If it is not, then a revision in our cognitive account may be required. (The revision might be made elsewhere. We might challenge the imaging results, for example. There are a number of practical problems confronting any research technique, such as fMRI. Uttal has identified many of these. But this is characteristic of the introduction of new techniques in other sciences. The route from experimental results to evaluation of mechanistic models is always vexed, but over time procedures generally do get developed that enable reasonable appraisals of the models themselves.)

Admittedly, this strategy of identifying cognitive operations and brain processes has so far yielded the greatest results in sensory processing areas for which Uttal is willing to admit the validity of decomposition and localization. But it is worth noting just how this strategy has succeeded in that case. Researchers did not start out with a detailed decomposition of seeing or hearing into component operations and simply map these onto brain processes. Rather, researchers started with very primitive proposals as to the decomposition, identified possible brain regions involved in sensory analysis, and then probed deeper into what those areas did. For example, the pioneering work of Hubel and Wiesel (1962, 1968) on vision was guided by the idea that Brodmann's area 17, known to be involved in visual perception on the basis of lesion studies, contained the same sort of center-surround cells as Kuffler found in the retina and LGN. Discovering instead that most cells that responded seemed to be responsive to bars of light led them to focus a new question: "How this information is used at later stages in the visual path is far from clear, and represents one of the most tantalizing problems for the future" (Hubel and

Wiesel, 1968, p. 242). The development of hypotheses about areas beyond area 17 was guided by lesion and single cell recording research. The single cell recording work generally tried to identify types of stimuli that would drive individual cells. The simplest interpretation of such results is that a cell is a feature detector for the feature exhibited by the stimuli that elicit a response. But like most simple interpretations, this one is being revised. It now seems likely, for example, that cells carry information even when they are not firing maximally, suggesting that we should view them as filters, not detectors (van Essen and Gallant, 1994). The complexity of the wiring pattern is also becoming apparent, further complicating any simple model of the mechanism. Despite these complexities, though, work on the visual processing system has quickly become an exemplar for cognitive neuroscience (Bechtel, 2001).

Extending from sensory processing to more central cognition is not likely to be easy and may, as Uttal suspects, prove impossible. In the next section I will discuss one of Uttal's major reasons for suspecting that such extension into more central cognition will not succeed.

4. The Challenges of a Highly Integrated System

At the foundation of Uttal's contention that decomposition of cognitive functions will fail is the conception of the brain as a complex dynamical system. In the next to last chapter of *Discovering Complexity* Richardson and I raised the question of whether there were kinds of systems in which mechanistic analysis would fail, and we concluded that it would fail in systems in which the components did not perform operations that were distinct from one another and for which it was the interactions of components within the system that was chiefly responsible for generating its behavior. Interactions of components are particularly important when they are non-linear. Uttal notes evidence that the brain is just such a system characterized by non-linear interactivity of simple processing units.

There is no doubt that the brain is a highly interactive non-linear complex system, perhaps the most complex system that science will encounter. But the question is whether the complexity is of a sort that will defeat the heuristics of decomposition and localization, or merely confront them with major challenges. It is too early in the development of cognitive neuroscience to answer that question definitively. But there are some reasons to be optimistic about the prospects of mechanistic analysis. The primary reason is that the strategy of decomposition and localization is compatible with discovering great amounts of interactivity that gives rise to complex dynamics. In fact, it is precisely when we have learned the parts of a system and their interactions that we are in position to apply tools of dynamical analysis. Thus, Fred Keijzer has recently argued, applying dynamics to biochemistry

> only works because we have a theory of chemical reactions and enzymes which specifies the parameters to be included in the mathematical model.

> The same goes for the interactions between an organism and its environment which amount to behavioral regularities .... [F]irst the relevant parameters which govern those regularities will have to be discovered. DST needs to be supplemented by a theory of the implementing substrate which specifies the relevant parameters and variables for the generation of behavior (Keijzer, 2001, p. 187).

To appreciate how traditional mechanistic analysis via decomposition and localization prepares the ground for dynamic modeling, consider again the neural case for which Uttal admits that mechanistic explanation might succeed – sensory processing. van Essen and his collaborators have identified over thirty different brain areas in visual processing as well as a complex pattern of interconnectivity. Approximately one third of all the possible pair-wise connections between these areas are realized in the monkey brain. For most feedforward projections there are corresponding feedback projections. There are also a large number of collateral projections. This is a highly interactive system and any account that simply tries to describe the stagewise processing of stimuli is likely to prove inadequate. The visual system is one ripe for the invocation of dynamical modeling techniques. But it is also salient that there are reasonably good hypotheses at this juncture as to what sorts of processing these various areas are involved in – for example, V4 is involved in shape and color processing and MT is involved in motion processing. What exactly are these areas doing? Clearly V4 is not *the color processing area* and MT is not *the motion processing area*. They only contribute to color and motion processing as a result of their interactions with a host of other areas. But at the same time, there are some reasonable hypotheses emerging as to what each area contributes to these types of processing. For example, the broad inhibitory connections in V4 suggest how this area can compute color constancy (of course, only given inputs provided from earlier areas and probably as constrained by backwards projections from later processing areas). The exact contribution may not be easy to specify in our current vocabulary. As I noted above, generally it is a major accomplishment in a science to develop the vocabulary to describe what a given set of components is doing. Moreover, our understanding of the contribution of the different areas is likely to emerge in conjunction with our understanding of the interactions between these areas and, in particular, the dynamics that arise between these areas.

One of the more common objections to attempting to fix the contribution of individual brain areas stems from discoveries about plasticity in the brain. These discoveries are not only very important, but in one respect make the challenge for cognitive neuroscience even harder since they indicate that the mechanism is constantly undergoing change. But they do not undermine the usefulness of trying to determine what individual brain areas do. In fact, it is when researchers begin to formulate hypotheses about the contributions of particular areas that they are in a position to discovery the plasticity and to incorporate it in their theories. For example, it was once relatively detailed maps of inputs to sensory areas were developed that the changes in these maps with altered input were identified.

An even more compelling case of how initial proposals of processing pathways prepares the ground for the discovery of plasticity is found in the follow up to one of the celebrated early neuroimaging studies. In the late 1980s Petersen *et al*. (1988) used the verb-generate task to attempt to identify some of the critical brain areas that contribute to language processing. In part because they were using PET, which allowed researchers only a limited number of trials with the subjects in the scanner, they generally had the subjects practice the tasks before going into the scanner. But does practice change the way the brain handles the task? Neuroimaging was able to provide part of the answer. Raichle *et al*. (1994) discovered that different neural pathways are active when subjects are imaged while still naive in performing the verb-generate task than when they have practiced it. Such results serve to raise new questions about the differences in the operations invoked in performing a task before and after practice, but the point here is that far from plasticity defeating the value of psychophsyical identity claims and the usefulness of techniques like neuroimaging, it is through identity claims and neuroimaging that we can discover and begin to understand the plasticity exhibited by the brain.

5. Conclusion

Uttal has raised a number of important issues about current practice in cognitive neuroscience. At the center of his critique, though, is the question of whether cognitive activities can be decomposed into component operations. He contends that they likely cannot. If he is right, the quest for mechanistic models is doomed and the sciences of the mind-brain will be radically different than other biological sciences. I have argued to the contrary, though, that there is reason to be optimistic. This requires taking a different perspective on current proposals for functionally decomposing cognitive activities than Uttal adopts. In particular, initial proposals for decomposition must be viewed as fallible first proposals which are likely to be heavily revised in the course of research. Second, the point of psychoneural identity claims is heuristic. By identifying component operations with neural components one pursues a discovery heuristic which provides additional constraints for evaluating and especially for revising the initially proposed decomposition. In part Uttal's negative assessment of the project of decomposing the mind is based on the recognition that the mind-brain is a complex non-linear system. While it may turn out to be the sort of complex non-linear system that defeats mechanistic explanation, in many cases complex non-linear systems are compatible with mechanistic analysis and there is reason to think the mind-brain is such a case. If so, then we should expect to revise the functional decomposition and alter accounts of the mechanism in conjunction with appreciating the dynamics within the system. Searching for mechanisms and modeling their dynamics will be mutually reinforcing, not in opposition.

References

Bechtel, W., 1994: Levels of description and explanaiton in cognitive science, *Minds and Machines* **4**, 1–25.

Bechtel, W., 2001: Decomposing and localizing vision: An exemplar for cognitive neuroscience, in W. Bechtel, P. Mandik, J. Mundale and R.S. Stufflebeam (eds), *Philosophy and the Nurosciences: A Reader*. Basil Blackwell, Oxford, pp. 225–249.

Bechtel, W., and McCauley, R.N., 1999: Heuristic identity theory (or back to the future): The mind-body problem against the background of research strategies in Cognitive Neuroscience, in M. Hahn and S. C. Stoness (eds), *Proceedings of the 21st Meeting of the Cognitive Science Society*, Lawrence Erlbaum Associates, Mahwah, NJ, pp. 67–72.

Bechtel, W., and Mundale, J., 1999: Multiple realizability revisited: Linking cognitive and neural states, *Philosophy of Science* **66**, 175–207.

Bechtel, W., and Richardson, R.C., 1993: *Discovering Complexity: Decomposition and Localization as Scientific Research Strategies*, Princeton University Press, Princeton, NJ.

Fodor, J.A., 1975: *The Language of Thought*, Crowell, New York.

Gabrieli, J.D.E., Poldrack, R.A., and Desmond, J.E., 1998: The role of left prefrontal cortex in language and memory, *Proceedings of the National Academy of Sciences, USA* **95**, 906–913.

Hubel, D.H., and Wiesel, T.N., 1962: Receptive fields, binocular interaction and functional architecture in the cat's visual cortex, *Journal of Physiology (London)* **160**, 106–154.

Hubel, D.H., and Wiesel, T.N., 1968: Receptive fields and functional architecture of monkey striate cortex, *Journal of Physiology (London)* **195**, 215–243.

Kim, J., 1964: Inference, explanation and prediction, *Journal of Philosophy* **61**, 360–368.

Lycan, W.G., 1987: *Consciousness*, MIT Press, Cambridge, MA.

Machamer, P., Darden, L., and Craver, C., 2000: Thinking about mechanisms, *Philosophy of Science* **67**, 1–25.

McCauley, R.N., and Bechtel, W., 2001: Explanatory pluralism and heuristic identity theory, *Theory and Psychology* **11**(6), 736–760.

Petersen, S.E., and Fiez, J.A., 1993: The processing of single words studied with positron emission tomography, *Annual Review of Neuroscience* **16**, 509–530.

Petersen, S.E., Fox, P.T., Posner, M.I., Mintun, M., and Raichle, M.E., 1988: Positron emission tomographic studies of the cortical anatomy of single-word prodessing, *Nature* **331**(18), 585–588.

Putnam, H., 1967: Psychological predicates, in W.H. Capitan and D.D. Merrill (eds), *Art, Mind and Religion*. University of Pittsburgh Press, Pittsburgh.

Raichle, M.E., Fiez, J.A., Videen, T.O., MacLeod, M.-A. K., Pardo, J.V., Fox, P.T., and Peterson, S.E., 1994: Practice-related changes in human brain functional anatomy during nonmotor learning, *Cerebral Cortex* **4**, 8–26.

van Essen, D.C., and Gallant, J.L., 1994: Neural mechanisms of form and motion processing in the primate visual system, *Neuron* **13**, 1–10.

Van Orden, G.C., Pennington, B.F., and Stone, G.O., 2001: What do double dissociations prove? Inductive methods and isolable systems, *Cognitive Science* **25**, 111–172.

Wimsatt, W.C., 1975: Reductionism, levels of organization, and the mind-body problem, in G. Globus, G. Maxwell and I. Savodnik (eds), *Brain and Consciousness*, Plenum, New York, pp. 205–267.