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Précis of *The Modularity of Mind*

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Abstract: *The Modularity of Mind* proposes an alternative to the "New Look" or "interactionist" view of cognitive architecture that has dominated several decades of cognitive science. Whereas interactionism stresses the continuity of perceptual and cognitive processes, modularity theory argues for their distinctness. It is argued, in particular, that the apparent plausibility of New Look theorizing derives from the failure to distinguish between the (correct) claim that perceptual processes are *inferential* and the (dubious) claim that they are *unencapsulated*, that is, that they are arbitrarily sensitive to the organism's beliefs and desires. In fact, according to modularity theory, perceptual processes are computationally isolated from much of the background knowledge to which cognitive processes have access. The postulation of autonomous, domain-specific psychological mechanisms underlying perceptual integration connects modularity theory with the tradition of faculty psychology, in particular, with the work of Franz Joseph Gall. Some of these historical affinities, and some of the relations between faculty psychology and Cartesianism, are discussed in the book.

Keywords: Cartesianism; cognition; faculty psychology; interactionism; language; modularity; neuropsychology; perception; phrenology

Everybody knows that something is wrong. But it is uniquely the achievement of contemporary philosophy – indeed, it is uniquely the achievement of contemporary *analytical* philosophy – to have figured out just what it is. What is wrong is that not enough distinctions are being made. If only we made all the distinctions that there are, then we should all be as happy as kings. (Kings are notoriously *very* happy.)

The Modularity of Mind (henceforth *Modularity*) is a monograph much in the spirit of that diagnosis. I wanted to argue there (and will likewise argue here) that modern Cognitivism failed, early on, to notice a certain important distinction: roughly, a distinction between two ways in which computational processes can be "smart." Because it missed this distinction, Cognitivism failed to consider some models of mental architecture for which a degree of empirical support can be marshaled, models that may, indeed, turn out to be true. If these models *are* true, then standard accounts of the nature of cognition and perception – and of the relations between them – are seriously misled, with consequences that can be felt all the way from artificial intelligence to epistemology. That was my story, and I am going to stick to it.

"What," you will ask, "was this missed distinction; who missed it; and how did missing it lead to these horrendous consequences?" I offer a historical reconstruction in the form of a fairy tale. None of what follows actually happened, but it makes a good story and has an edifying moral.

So then: Once upon a time, there was a Wicked Behaviorist. He was, alas, a mingy and dogmatic creature of little humor and less poetry; but he did keep a clean attic. Each day, he would climb up to his attic and throw things out, for it was his ambition eventually to have *almost nothing in his attic at all.* (Some people whispered that this was his *only* ambition, that the Wicked Behaviorist was actually just a closet Ontological Purist. For all I know, they were right to whisper this.)

Anyhow, one day when the Wicked Behaviorist was upstairs cleaning out his attic, the following Very Interesting Thought occurred to him. "Look," he said to himself, "*I can do without perceptual processes.*" (Because he had been educated in Vienna, the Wicked Behaviorist usually thought in the formal mode. So what actually occurred to him was that he could do without a *theory* of perceptual processes. It comes to much the same thing.) "For," it continued to occur to him, "perceptual identification reduces without residue to discriminative responding. And discriminative responding reduces without residue to the manifestation of conditioned (as it might be, operant) reflexes. And the theory of conditioned reflexes reduces without residue to Learning Theory. So, though learning is one of the things that there are, perceptual processes are one of the things there aren't. There also aren't: The True, or The Beautiful, or Santa Claus, or Tinkerbell; and unicorns are metaphysically impossible and George Washington wore false teeth. So there. Grrr!" He really was a *very* Wicked Behaviorist.

Fortunately, however, in the very same possible world in which the WB eked out a meager existence as a value of a bound variable (for who would call that living?), there was also a Handsome Cognitivist. And whereas the WB had this preference for clean attics and desert landscapes, the HC's motto was: "The more the merrier, more or less!" It was the HC's view that almost nothing reduces to almost anything else. To say that the world is so full of a number of things was, he thought, putting it mildly; for the HC, every day was like Christmas in Dickens, ontologically speaking. In fact, far from wishing to throw old things out, he was mainly interested in turning new things up. "Only collect," the HC was often heard to say.

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Above all – and this is why I'm telling you this story – the HC wanted mental processes in general, and perceptual processes in particular, to be part of his collection.

Moreover, the HC had an argument. "Perceptual processes," he said, "*can't* be reflexes because, whereas reflexes are paradigmatically dumb, perceptual processes are demonstrably smart. Perception is really a part of cognition; it involves a kind of *thinking*."[1]

"And what demonstrates that perceptual processes are smart?" grumbled the Wicked Behaviorist.

"I will tell you," answered the Handsome Cognitivist. "What demonstrates that perceptual processes are smart is *Poverty of The Stimulus Arguments*." [A Poverty of The Stimulus Argument alleges that there is typically *more information* in a perceptual response than there is in the proximal stimulus that prompts the response; hence perceptual integration must somehow involve the *contribution* of information by the perceiving organism. [See Chomsky: "Rules and Representations" *BBS* 3(1) 1980.] No one knows how to quantify the relevant notion of information, so it is hard to show conclusively that this sort of argument is sound. On the other hand, such phenomena as the perceptual constancies have persuaded almost everybody – except Gibsonians and Wicked Behaviorists [see Ullman: "Against Direct Perception" *BBS* 3(3) 1980, and Rachlin: "Pain and Behavior," this issue) – that Poverty of The Stimulus Arguments have to be taken very seriously. I shall assume, in what follows, that that is so.] "Poverty of The Stimulus Arguments," continued the HC, "show that perceptual identifications can't be reflexive responses to proximal stimulus invariants. In fact, Poverty of The Stimulus Arguments strongly suggest that perceptual identifications depend on some sort of *computations*, perhaps on computations of quite considerable complexity. So, once we have understood the force of Poverty of The Stimulus Arguments, we see that there probably are perceptual processes after all." "And," the HC added in a rush, "I believe that there are Truth and Beauty and Santa Claus and Tinkerbell too (only you have to read the existential quantifier leniently). And I believe that for each drop of rain that falls / A flower is born. So *there*." (Some people whispered that the Handsome Cognitivist, though he was *very* handsome, was perhaps just a little wet. For all I know, they were right to whisper that, too.) End of fairy tale.

My point is this: Modern Cognitivism starts with the use of Poverty of The Stimulus Arguments to show that perception is smart, hence that perceptual identification can't be reduced to reflexive responding. However – and I think this is good history and not a fairy tale at all – in their enthusiasm for this line of argument, early Cognitivists failed to distinguish between two quite different respects in which perceptual processes might be smarter than reflexes. Or, to put it the other way around, they failed to distinguish between two respects in which perception might be similar to cognition. It's at precisely this point that *Modularity* seeks to insert its wedge.

Reflexes, it is traditionally supposed, are dumb in two sorts of ways: They are *noninferential* and they are *encapsulated*.[2] To say that they are noninferential is just to say that they are supposed to depend on "straight-through" connections. On the simplest account, stimuli elicit reflexive responses directly, without mediating mental processing. It is my view that the HC was right about perceptual processes and reflexive ones being different in *this* respect; Poverty of The Stimulus Arguments do make it seem plausible that a lot of inference typically intervenes between a proximal stimulus and a perceptual identification.

By contrast, to describe reflexes as encapsulated is to say that they go off largely without regard to the beliefs and utilities of the behaving organism; to a first approximation, all that you need do to evoke a reflex is to present the appropriate eliciting stimulus. Here's how *Modularity* put this point:

> Suppose that you and I have known each other for many a long year . . . and you have come fully to appreciate the excellence of my character. In particular, you have come to know perfectly well that under no conceivable circumstances would I stick my finger in your eye. Suppose that this belief of yours is both explicit and deeply felt. You would, in fact, go to the wall for it. Still, if I jab my finger near enough to your eyes, and fast enough, you'll blink. . . . [The blink reflex] has no access to what you know about my character or, for that matter, to any other of your beliefs, utilities [or] expectations. For this reason the blink reflex is often produced when sober reflection would show it to be uncalled for. . . . (p. 71)

In this respect reflexes are quite unlike a lot of "higher cognitive" behavior, or so it would certainly seem. Chess moves, for example, aren't elicited willy-nilly by presentations of chess problems. Rather, the player's moves are determined by the state of his utilities (is he trying to win? or to lose? or is he, perhaps, just fooling around?) and by his beliefs, including his beliefs about the current state of the game, his beliefs about the structure of chess and the likely consequences of various patterns of play, his beliefs about the beliefs and utilities of his opponent, his beliefs about the beliefs of his opponent about *his* beliefs and utilities, and so on up through ever so many orders of intentionality.

So, then, cognition is smart in two ways in which reflexes are dumb. Now the question arises: What is *perception* like in these respects? *Modularity* offers several kinds of arguments for what is, really, a main thesis of the book: Although perception is smart like cognition in that it is typically inferential, it is nevertheless dumb like reflexes in that it is typically encapsulated. Perhaps the most persuasive of these arguments – certainly the shortest – is one that adverts to the persistence of perceptual illusions. The apparent difference in length of the Mueller–Lyer figures, for example, doesn't disappear when one learns that the arrows are in fact the same size. It seems to follow that at least *some* perceptual processes are insensitive to at least some of one's beliefs. Very much wanting the Mueller–Lyer illusion to go away doesn't make it disappear either; it seems to follow that at least some perceptual processes are insensitive to at least some of one's utilities. The ecological good sense of this arrangement is surely self-evident. Prejudiced and wishful seeing makes for dead animals.

This sort of point seems pretty obvious; one might wonder how Cognitivist enthusiasm for "top down," "cognitively penetrated" perceptual models managed to survive in face of it. I think we have already seen part of the answer: Cognitivists pervasively confused the question about the encapsulation of perception with the question about its computational complexity. Because they believed – rightly – that Poverty of The Stimulus Arguments settled the second question, they never seriously considered the issues implicit in the first one. You can actually *see* this confusion being perpetrated in some of the early Cognitivist texts. The following passage is from Bruner's "On Perceptual Readiness":

> Let it be plain that no claim is being made for the utter indistinguishability of perceptual and more conceptual inferences. . . . I may know that the Ames distorted room that looks so rectangular is indeed distorted, but unless conflicting cues are put into the situation . . . the room still looks rectangular. So too with such compelling illusions as the Mueller–Lyer: In spite of knowledge to the contrary, the line with the extended arrowheads looks longer than the equal-length line with arrowheads inclined inward. *But these differences, interesting in themselves, must not lead us to overlook the common feature of inference underlying so much of cognitive activity.* (Bruner 1973, p. 8; emphasis added)

The issue raised by the persistence of illusion is not, however, whether some inferences are "more conceptual" than others – whatever, precisely, that might mean. Still less is it whether perception is in some important sense inferential. Rather, what's at issue is: How rigid is the boundary between the information available to cognitive processes and the information available to perceptual ones? How much of what you know/believe/desire actually does affect the way you see? The persistence of illusion suggests that the answer must be: "at most, less than all of it."

So far, my charge has been that early Cognitivism missed the distinction between the inferential complexity of perception and its cognitive penetrability. But, of course, it's no accident that it was just that distinction that Cognitivists confused. Though they are independent properties of computational systems, inferential complexity and cognitive penetrability are intimately related – so intimately that, unless one is *very* careful, it's easy to convince oneself that the former actually entails the latter. [For discussion see Pylyshyn: "Computation and Cognition"*BBS* 3(1) 1980.]

What connects inferential complexity and cognitive penetrability is the truism that inferences need premises. Here's how the argument might seem to go: Poverty of The Stimulus Arguments show that the organism must contribute information to perceptual integrations; "perceptual inferences" just *are* the computations that effect such contributions. Now, this information that the organism contributes – the premises, as it were, of its perceptual inferences – must include not just sensory specifications of current proximal inputs but also "background knowledge" drawn from prior experience or innate endowment; for what Poverty of The Stimulus Arguments show is precisely that sensory information alone underdetermines perceptual integrations. But, surely, the availability of background knowledge to processes of perceptual integration *is* the cognitive penetration of perception. So if perception is inferentially elaborated, it *must* be cognitively penetrated. Q.E.D.

What's wrong with this argument is that it depends on what one means by cognitive penetration. One might mean the availability to perceptual integration of some information not given in the proximal array. Because Poverty of The Stimulus Arguments show that some such information must be available to perceptual integration, it follows that to accept Poverty of The Stimulus Arguments is to accept the cognitive penetrability of perception *in this sense.* But one might also mean by the cognitive penetrability of perception that *anything that the organism knows, any information that is accessible to any of its cognitive processes,* is ipso facto available as a premise in perceptual inference. This is a much more dramatic claim; it implies the *continuity* of perception with cognition. And, if it is true, it has all sorts of interesting epistemic payoff (see Fodor 1984). Notice, however, that this stronger claim does not follow from the inferential complexity of perception.

Why not? Well, for the following boring reason. We can, in principle, imagine three sorts of architectural arrangements in respect of the relations between cognition and perception: *no* background information is available to perceptual integration; *some but not all* background information is available to perceptual integration; *everything one knows* is available to perceptual integration. Because Poverty of The Stimulus Arguments imply the inferential elaboration of perception, and because inferences need premises, the first of these architectures is closed to the Cognitivist. But the second and third are still open, and the persistence of illusions is prima facie evidence that the second is the better bet.

We arrive, at last, at the notion of a psychological module. A module is (inter alia) an informationally encapsulated computational system – an inference-making mechanism whose access to background information is constrained by general features of cognitive architecture, hence relatively rigidly and relatively permanently constrained. One can conceptualize a module as a special-purpose computer with a proprietary database, under the conditions that: (a) the operations that it performs have access *only* to the information in its database (together, of course, with specifications of currently impinging proximal stimulations); and (b) at least some information that is available to at least some cognitive process is *not* available to the module. It is a main thesis of *Modularity* that perceptual integrations are typically performed by computational systems that are informationally encapsulated in this sense.

Modularity has two other main theses, which I might as well tell you about now. The first is that, although informational encapsulation is an essential property of modular systems, they also tend to exhibit other psychologically interesting properties. The notion of a module thus emerges as a sort of "cluster concept," and the claim that perceptual processes are modularized implies that wherever we look at the mechanisms that effect perceptual integration we see that this cluster of properties tends to recur. The third main thesis is that, whereas perceptual processes are typically modularized – hence encapsulated, hence stupid in one of the ways that reflexes are – the really "smart," really "higher" cognitive processes (thinking, for example) are not modular and, in particular, not encapsulated. So *Modularity* advocates a *principled distinction* between perception and cognition in contrast to the usual Cognitivist claims for their continuity.

Since *Modularity* goes into all of this in some detail, I don't propose to do so here; otherwise, why would you buy the book? But I do want to stress the plausibility of the picture that emerges. On the one hand, there are the perceptual processes; these tend to be input driven, very fast, mandatory, superficial, encapsulated from much of the organism's background knowledge, largely organized around bottom-to-top information flow, largely innately specified (hence ontogenetically eccentric), and characteristically associated with specific neuroanatomical mechanisms (sometimes even with specific neuroanatomical loci). They tend also to be domain specific, so that – to cite the classic case – the computational systems that deal with the perception/production of language appear to have not much in common with those that deal with, for example, the analysis of color or of visual form (or, for that matter, the analysis of nonspeech auditory signals). So strikingly are these systems autonomous that they often rejoice in their proprietary, domain-specific pathologies: compare the aphasias and agnosias. *Modularity* takes the view that it is high time to praise Franz Joseph Gall for having predicted the existence of psychological mechanisms that exhibit this bundle of properties. (Gall was approximately a contemporary of Jane Austen's, so you see how far we have come in cognitive psychology – and in the novel, for that matter.) It is precisely in the investigation of these "vertical faculties" that modern Cognitivism has contributed its most important insights, and *Modularity* suggests that this is no accident. Precisely because the perceptual mechanisms are encapsulated, we can make progress in studying them without having to commit ourselves about the general nature of the cognitive mind.

On the other hand, there are the true higher cognitive faculties. So little is known about them that one is hardput even to say *which* true higher cognitive faculties there are. But "thought" and "problem solving" are surely among the names in the game, and here *Modularity's* line is that these are everything that perception is not: slow, deep, global rather than local, largely under voluntary (or, as one says, "executive") control, typically associated with diffuse neurological structures, neither bottom-to-top nor top-to-bottom in their modes of processing, but characterized by computations in which information flows every which way. Above all, they are paradigmatically *un*encapsulated; the higher the cognitive process, the more it turns on the integration of information across superficially dissimilar domains. *Modularity* assumes that in this respect the higher cognitive processes are notably similar to processes of scientific discovery – indeed, that the latter are the former writ large. Both, of course, are deeply mysterious; we don't understand nondemonstrative inference in either its macrocosmic or its microcosmic incarnation.

If much of the foregoing is right, then mainstream Cognitive science has managed to get the architecture of the mind *almost exactly backwards*. By emphasizing the continuity of cognition with perception, it missed the computational encapsulation of the latter. By attempting to understand thinking in terms of a baroque proliferation of scripts, plans, frames, schemata, special-purpose heuristics, expert systems, and other species of domain-specific intellectual automatisms – jumped-up habits, to put it in a nutshell – it missed what is most characteristic, and most puzzling, about the higher cognitive mind: its nonencapsulation, its creativity, its holism, and its passion for the analogical. One laughs or weeps according to one's temperament. It was, perhaps, Eeyore who found precisely the right words: "'Pathetic,' he said, 'That's what it is, pathetic.'"

Well, yes, but *is* much of this right? I want at least to emphasize its plausibility from several different points of view. Perception is above all concerned with keeping track of the state of the organism's local spatiotemporal environment. Not the distant past, not the distant future, and not – except for ecological accidents like stars – what is very far away. Perception is built to detect what is right here, right now – what is available, for example, for eating or being eaten by. If this is indeed its teleology, then it is understandable that perception should be performed by fast, mandatory, encapsulated, . . . etc. systems that – considered, as it were, detection-theoretically – are prepared to trade false positives for high gain. It is, no doubt, important to attend to the enternally beautiful and to believe the eternally true. But it is more important not to be eaten.

Why, then, isn't perception even stupider, even less inferential than it appears to be? Why doesn't it consist of literally reflexive responses to proximal stimulations? Presumably because there is so much more variability in the proximal projections that an organism's environment offers to its sensory mechanisms than there is in the distal environment itself. This kind of variability is by definition irrelevant if it is the distal environment that you care about – which, of course, it almost always is. So the function of perception, from this vantage point, is to propose to thought a representation of the world from which such irrelevant variability has been effectively filtered. What perceptual systems typically "know about" is how to infer current distal layouts from current proximal stimulations: the visual system, for example, knows how to derive distal form from proximal displacement, and the language system knows how to infer the speaker's communicative intentions from his phonetic productions. Neither mechanism, on the present account, knows a great deal else, and that is entirely typical of perceptual organization. Perceptual systems have access to (implicit or explicit) theories of the mapping between distal causes and proximal effects. But that's all they have.

If the perceptual mechanisms are indeed local, stupid, and extremely nervous, it is teleologically sensible to have the picture of the world that they present tempered, reanalyzed, and – as Kant saw – above all *integrated* by slower, better informed, more conservative, and more holistic cognitive systems. The purposes of survival are, after all, *sometimes* subserved by knowing the truth. The world's deep regularities don't show in a snapshot, so being bullheaded, ignoring the facts that aren't visible on the surface – encapsulation in short – is not the cognitive policy that one wants to pursue *in the long run*. The surface plausibility of the *Modularity* picture thus lies in the idea that Nature has contrived to have it both ways, to get the best out of fast dumb systems *and* slow contemplative ones, by simply refusing to choose between them. That is, I suppose, the way that Nature likes to operate: "I'll have some of each" – one damned thing piled on top of another, and nothing in moderation, ever.

It will have occurred to you, no doubt, that Cognitivism could quite possibly have hit on the right doctrine, even if it did so for the wrong reasons. Whatever confusions may have spawned the idea that perception and cognition are continuous, and however plausible the encapsulation story may appear to be a priori, there is a lot of experimental evidence around that argues for the effects of background knowledge in perception. If the mind really is modular, those data are going to have to be explained away. I want to say just a word about this.

There are, pretty clearly, three conditions that an experiment has to meet if it is to provide a bona fide counter-instance to the modularity of a perceptual system.

1. It must, of course, demonstrate the influence of background information in some computation that the system performs. But, more particularly, the background information whose influence it demonstrates must be *exogenous* from the point of view of the module concerned. Remember, each module has its proprietary database; whatever information is in its database is ipso facto available to its computations. So, for example, it would be no use for purposes of embarrassing modularity theory to show that words are superior to nonwords in a speech perception task. Presumably, the language processing system has access to a grammar of the language that it processes, and a grammar must surely contain a lexicon. What words are in the language is thus one of the things that the language module can plausibly be assumed to know consonant with its modularity.

2. The effect of the background must be distinctively perceptual, not postperceptual and not a criterion shift. For example, it is of no use to demonstrate that utterances of "implausible" sentences are harder to process than utterances of "plausible" ones if it turns out that the mechanism of this effect is the hearer's inability to believe that the speaker could have said what it sounded like he said. No one in his right mind doubts that perception interacts with cognition *somewhere*. What's at issue in the disagreement between modularity theory and "New Look" Cognitivism (e.g., Bruner 1973) is the *locus* of this interaction. In practice, it usually turns out that the issue is whether the recruitment of background information in perception is *predictive*. Modularity theory says almost never; New Look Cognitivism says quite a lot of the time.

3. The cognitively penetrated system must be the one that shoulders the burden of perceptual analysis in normal circumstances, and not, for example, some backup, problem-solving type of mechanism that functions only when the stimulus is too degraded for a module to cope with. Therefore, it is of no use to show that highly redundant lexical items are easier to understand than less redundant ones when the speech signal is very noisy – unless, of course, you can also show that the perception of very noisy speech really is bona fide speech perception.

So far as I know, there is very little in the experimental literature that is alleged to demonstrate the cognitive penetration of perception that meets all three of these conditions (to say nothing of replicability). This isn't to claim that such experiments cannot be devised or that, if devised, they might not prove that New Look Cognitivism is right after all. I claim only that, contrary to the textbook story, the empirical evidence for the continuity of perception with cognition is not overwhelming when contemplated with a jaundiced eye. There is, in any event, something for laboratory psychology to do for the next twenty years or so: namely, try to develop some designs subtle enough to determine who's right about all this.

"But look," you might ask, "why do you care about modules so much? You've got tenure; why don't you take off and go sailing?" This is a perfectly reasonable question and one that I often ask myself. Answering it would require exploring territory that I can't get into here and raising issues that *Modularity* doesn't even broach. But roughly, and by way of striking a closing note: The idea that cognition saturates perception belongs with (and is, indeed, historically connected with) the idea in the philosophy of science that one's observations are comprehensively determined by one's theories; with the idea in anthropology that one's values are comprehensively determined by one's culture; with the idea in sociology that one's epistemic commitments, including especially one's science, are comprehensively determined by one's class affiliations; and with the idea in linguistics that one's metaphysics is comprehensively determined by one's syntax. All these ideas imply a sort of relativistic holism: because perception is saturated by cognition, observation by theory, values by culture, science by class, and metaphysics by language, rational criticism of scientific theories, ethical values, metaphysical world-views, or whatever can take place only *within* the framework of assumptions that – as a matter of geographical, historical, or sociological accident – the interlocutors happen to share. What you can't do is rationally criticize the framework.

The thing is: I *hate* relativism. I hate relativism more than I hate anything else, excepting, maybe, fiberglass powerboats. More to the point, I think that relativism is very probably false. What it overlooks, to put it briefly and crudely, is the fixed structure of human nature. (This is not, of course, a novel insight; on the contrary, the *malleability* of human nature is a doctrine that relativists are invariably much inclined to stress. See, for example, John Dewey in *Human Nature and Conduct* [1922].) Well, in cognitive psychology the claim that there is a fixed structure of human nature traditionally takes the form of an insistence on the the heterogeneity of cognitive mechanisms and on the rigidity of the cognitive architecture that effects their encapsulation. If there are faculties and modules, then not everything affects everything else; not everything is plastic. Whatever the All is, at least there is more than One of it.

These are, as you will have gathered, not issues to be decisively argued – or even perspicuously formulated – in the course of a paragraph or two. Suffice it that they seem to be the sorts of issues that our cognitive science ought to bear on. And they are intimately intertwined: surely, *surely*, no one but a relativist would drive a fiberglass powerboat.

Coming in our next installment: "Restoring Basic Values: Phrenology in an Age of License." Try not to miss it!

NOTES

1. See, for example, Gregory (1970, p. 30): "perception involves a kind of problem-solving; a kind of intelligence." For a more recent and comprehensive treatment that runs along the same lines, see Rock (1983).

2. I don't at all care whether these "traditional assumptions" about reflexes are in fact correct, or even whether they were traditionally assumed. What I want is an ideal type with which to compare perception and cognition.

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A neo-Cartesian alternative

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Fodor's thesis raises many points deserving commentary. I shall focus on the question of the encapsulation of input systems and suggest an alternative to Fodor's formulation.

Fodor sets up certain representations as psychologically special: outputs of input analyzers and interlevels. The outputs of input analyzers are taken to be "the most abstract members of their inferential hierarchies" that could be assigned by a "bottom-up" analytic process. Examples are "basic categories" of objects in vision and syntactic structure in language. Interlevels are the representations that constitute the bottom-up stages that are computed before these output representations are arrived at. Fodor is somewhat contradictory about these representations at different points in the monograph, suggesting at times that input analyzers are responsible for fixation of perceptual belief (pp. 68–69) and at other times that the outputs of input systems are representations as primitive as Marr's primal sketches (p. 74). It is quite clear, however, that Fodor does not see the outputs of input systems as either this "high" or this "low." These outputs are "phenomenologically accessible," in the sense that they can serve as the basis for the nondemonstrative inferential processes that Fodor calls "central processes." Fodor claims that these representations are projected from transduced sensory information by "input systems," each input system being devoted to the projection of specific representations ("domain specificity").

What is psychologically special about these output representations and the interlevels of the input systems is that they are not projected from sensory transductions or verified in the light of anything the organism knows outside the input system in question. That is, the projection and verification of interlevels can be influenced only through top-down information from within a module, but the projection of the output of a module cannot be influenced by any top-down information. (Modules are themselves quite large; one module is "language.")

This feature of "informational encapsulation" is a crucial aspect of the modularity thesis that Fodor proposes. The point I wish to stress is that Fodor proposes this as a *psychological* phenomenon. I shall argue, in contrast, that if input systems are encapsulated, they are encapsulated because of the nature of the representations they compute, not because of any special psychological feature of their processing.

My argument rests on the fact that small top-down effects from outside putative modules do affect interlevels – and are cited byFodor. One such effect is that of semantic sentential context upon phoneme restoration (Warren 1970; cited by Fodor, p. 65). Fodor dismisses this effect's being a counterexample to encapsulation, in part because it operates at the level of a response bias rather than at the level of "perception," according to signal detection theory analyses. But one might well ask: Where else could it operate? Semantic context cannot project hypotheses regarding phoneme identity *directly;* it can only interact with phoneme recognition via its utility in predicting the occurrence of particular words. Moreover, context cannot serve to *ensure* that what is presented is actually a word (in experiments) or determine what word was presented and thus what phonemes were (or were not) presented. Semantic context can, at most, make the occurrence of a particular word more easily integrable into discourse and thus prejudice a recognition system to guess that a word has been presented on the basis of less bottom-up information when that information is consistent with a predicted word than when it is not.

If this is a reasonable view of the way top-down effects occur at interlevels of input systems, and if there is also evidence for top-down effects upon the output of input systems (alluded to by Fodor in the visual system, p. 74), then the existence of psychologically encapsulated processors devoted to the establishment of interlevels and outputs of input systems is cast into serious doubt. In essence, the encapsulation of these processes is not secondary to the existence of psychological systems that constrain these processes. Rather, it is due to the nature of the representations involved and the possible loci at which these representations could interact.

Suppose we have a psychological system where every piece of knowledge at an organism's disposal can influence the recognition of items such as the syntactic structure of a sentence or the identity of an object. It is nonetheless the case that most of the information the organism possesses will not be relevant to this task (exceptional situations may make some information more relevant, but, in cases like syntactic structures, the effect would probably be small). The reason for this is that representations such as syntactic structure are extremely "eccentric," to use Fodor's term, and there is very little general information that actually bears upon their positive identification. What context can do is make a guessing strategy more reasonable, depending upon the pay-off for using a guessing strategy, as in the panther example developed by Fodor (pp. 70–71). Context cannot ensure a positive perceptual identification or a perceptual projection, in Fodor's sense.

If this account is plausible, then there is nothing special about the psychological processes involved in the projection and recognition of interlevels and outputs of input systems. These processes, like the central processes Fodor describes, are isotropic and Quinean, but these properties are simply not relevant to the "perceptual projection" of specific representations. They are relevant to the fixation of *all* representations in systems of belief, accomplishment of nondemonstrative inferences, and the like. We may consider "perceptual" representations to be those "highest inferences" projected by input systems that serve in (conscious?) inferences. As perceptual conditions become less adequate, confirmation (and disambiguation) of these levels depends increasingly upon general knowledge. Confirmation of interlevels can typically be accomplished only by ascertaining other interlevels and the output representations. This is why input modules appear encapsulated: confirmation of *interlevels* can go on only via module – internal representations – whereas *projection* of *all* representations is bottom-up.

Does this mean that the mind is not modular? No. It means that the mind is modular because of the nature of the representations it recovers (its "domain specificity") and because of the relationship that representations bear to each other in formal systems. This notion is very much akin to the Cartesian view of modularity described by Fodor in the first section of the book, but it differs in an important aspect. What is present in a "mental organ" is not, or not only, knowledge consisting of a set of propositional structures, in the sense of "contents of belief" (p. 7). In addition to, or, perhaps, rather than, content of belief being the knowledge content of a mental organ, the *form* of the representations in which certain types of propositional knowledge are stated constitutes a mental organ. Psychological processes such as perception are constrained to recover eccentric representations because these representations are necessary way stations along the road to central processes in humans. (In many cases, you can establish the semantics of a sentence only via the recovery of its syntax.) Constraints on the form of representations and on their formal interactions, as noted above, vitiate the attempt to bring all of an organism's knowledge to bear directly on the confirmation of many, if not all, interlevel representations.

In this view, certain input processes are domain specific, in Fodor's terms, and, by virtue of constraints on the interaction of formal representations, they may be effectively informationally encapsulated. But there is nothing special about the psychology of input systems per se. What is special, and what dictates the modularity of mind, is the special nature of certain representations and the formal systems to which they belong. I believe that this is the essential claim of modern neo-Cartesianism. Though it is a great deal weaker than Fodor's version of modularity, it is strong enough to allow a lot of psychology to be done.

On Spearman's "problem of correlation"

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My entry point is the quotation from Spearman placed immediately after Fodor's title page, a quotation that notes that "the sole permanent effect of . . . attacks [on the doctrine of form faculties] was . . . to banish the word 'faculty', leaving the doctrine represented by this word to escape scot free." Fodor has made promising steps toward returning faculty psychology to respectability after the century or so in which it has been, as he says, "hanging around with phrenologists and other dubious types" (p. 1). As someone concerned with the relevance of individual differences to cognitive psychology, I am inclined to welcome a revival of some form of faculty psychology if it can provide a firm theoretical basis for integrating the findings of differential psychology with those of experimental and other approaches. I am gratified that, as his favorable use of a quotation from Spearman indicates, Fodor appears not to put differential psychologists in the class of the "dubious types" associated with faculty psychology.

Spearman was only the first of many difference psychologists and factor analysts to be criticized – in their view unjustly – for appearing to subscribe to faculty psychology. In his classic work, ***Multiple-Factor Analysis***, Thurstone (1947, p. 70) admits, "Factor analysis is reminiscent of faculty psychology." He goes on to say: "It is true that the object of factor analysis is to discover the mental faculties. But the severe restrictions that are imposed by the logic of factor analysis make it an arduous task to isolate each new mental faculty." Later he remarks: "Those who criticize factor analysis as faculty psychology talk in the next breath about verbal and nonverbal intelligence, special aptitudes for music and for art, mechanical aptitudes, and disabilities in reading and arithmetic, without realizing that they are implying factor analysis and the interpretation of factors. . . . To find . . . functional unities is the problem of factor analysis. If that is faculty psychology, then so is most investigation of individual differences and most of the work on special aptitudes and defects" (pp. 145–146). Actually, taking their cue from Spearman and Thurstone, most students of individual differences have gone on their way without fretting about whether the parameters and functional unities they claim to discover represent "faculties." Cattell (1971), for example, never broaches the problem in his major work on cognitive abilities. Perhaps this is simply because discussion of mental faculties had gone out of fashion.

Fodor draws attention to Gall's fascination with individual differences in aptitudes and briefly considers whether aptitude differences ought to be regarded as representing "horizontal" or "vertical" faculties. In this connection, he mentions Spearman's "problem of correlation" – "in effect, the interaction of the level of functioning of a faculty with the cognitive domain in which it is employed" (p. 18). In the end, however, Fodor lays aside the problem of correlation and individual differences (p. 21), never seriously to take it up again. I found this omission disappointing. In saying that "no doubt it is possible to achieve some gross factoring of 'intelligence' into 'verbal' versus 'mathematical/spatial' capacities" (p. 104), Fodor ignores a half century of work on the differentiation of cognitive abilities. As described, for example, by Horn (1978), up to 30 or so abilities have been well established in factor-analytic work, and at least some of these might be aligned with the "modules" of the mind posited by Fodor.

In another disappointing feature of Fodor's work, he claims to offer a functional taxonomy of cognitive mechanisms, but one cannot gain any good impression of what he thinks these mechanisms might be or how many he believes there are. He points out, to be sure, that "every faculty psychologist has to find some motivated way of answering the question 'How many faculties are there?'" (p. 18), but even if he rejects evidence from individual differences – as he appears to do – I am not persuaded that he presents an explicit method for answering this question from other kinds of evidence.

There is a rough analogy – one I would not want to push too far – between Fodor's distinction between "modular" and "nonmodular" processes, on the one hand, and the distinction between "primary" or "narrow" factors and "general" or "broad factors" that one finds in theories of the structure of intellect, on the other. Such "primary" factors as verbal, spatial, numerical, and perceptual abilities are relatively easy to identify and interpret, and perhaps they reflect the operation of Fodor-type "modules." "General" or "higher-order" factors, such as "fluid intelligence," "crystallized intelligence," and "general fluency," are more difficult to pin down to identifiable mental processes, and perhaps they belong in the realm of what Fodor calls central, nonmodular, "Quineian/isotropic" systems. Here I see a possible congruence between Fodor's theoretical position and theories of mental structure based on evidence from individual differences. However, I find it hard to accept Fodor's notion that nonmodular processes like reasoning and problem solving are inherently unanalyzable. Fodor seems to ignore the progress that has been made – partly, at least, with the aid of individual difference methodology and data (e.g., see Sternberg 1982) – in process analysis of analogical and syllogistic reasoning.

The evidence from individual differences to which Gall made much appeal cannot in my judgment be dismissed as easily as Fodor appears to do. The "latent traits" and factors that are assumed and actually recovered in several individual difference methodologies (factor analysis, the theory of mental tests, multidimensional scaling) present the potential of being identified with Fodor's "*causal mechanisms that underlie the mind's capacities*" (p. 24, italics his), if proper care is taken in assembling evidence for their causal effects. Fodor hints at one of the cautions: "There is no obvious reason why the same faculty should not be strong in one employment and weak in another, so long as the employments are not themselves identical" (p. 17). That is to say, correlations among performances can be interpreted as indicating identical faculties or, as the case may be, different faculties only when the degrees of "employments" of faculties are similar or controlled. This leaves open the problem of assessing "employments," but that problem has yet to be proved insoluble.

Module or muddle?

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Linguists seem to feel the need to explain why what they do is interesting. They explain to deans, to funding agencies, to the lawnmower salesman in the next seat on an airplane, and to each other. Here are two themes that the lawnmower salesman is likely to have become familiar with over the last twenty-five years or so. (1) What we'd really like to know about is the nature of the human mind. Language is a complex and peculiarly human ability, and linguistic data (data at least about what people know, if not about what's involved in knowing it) are plentiful, systematic, tolerably clear, and immediately accessible. So, for methodological reasons, studying language is the best way of studying the mind. (2) The results so far indicate that people know some surprising facts about their language, facts that they couldn't have derived by general induction procedures over the sentences heard during language learning. So human infants must be innately programmed with specifically linguistic principles such as structure dependence, the transformational cycle, the A-over-A constraint, and (more recently) subjacency and the binding principles.

Perhaps we always detected a whiff of inconsistency between these two assertions, but now Jerry Fodor's *Modularity of Mind* has sharpened up the issues and forces us to choose. Is language typical or is it special? If it's special, it is subserved by a module, and studying it will tell us about that module but not about others or about the nonmodular mental processes assumed to underlie thought. Conversely, the language faculty can serve as a model of the mind at large only to the extent that it isn't modular, isn't really a faculty at all.

Which is our best bet? The module alternative is fashionable this year, and perhaps safer too, for Fodor predicts that anyone whose research topic belongs to the nonmodular mind is doomed to frustration. Nevertheless, at least in some areas of language study, the module model has competitors. If we want language to be interesting because it's modular rather than because it isn't, we must therefore do something to disarm the opposition. In what follows I will address only the syntactic processing of sentences involved in language comprehension; I will say nothing about sentence production or language acquisition or about the various nonsyntactic aspects of comprehension. Most particularly, I will *not* enter the fray on the borderline between syntactic processing and semantic/pragmatic processing, even though, as Fodor observes, this area is highly significant to the issue of the informational encapsulation of language processing.

There is a class of models of human syntactic processing that I will call "algorithmic," because they assume that the parsing mechanism is programmed to examine input words sequentially as they are received and to respond to each one in some quite specific way, such as adding certain nodes to a phrase marker in temporary memory. A very different model of the parsing device is what has been called a "heuristic" or sometimes a "detective" model. This one portrays the parser as casting through a sentence for potentially useful superficial clues about its structure and using them as a basis for making a structural guess (which may later be checked out by some more systematic analysis-by-synthesis procedure). An algorithmic parser is, as Fodor puts it, "deeply unintelligent" – a property characteristic of modular systems. A detective parser, by contrast, seems to need a smart, flexible central control structure (ghost?) whose job is to construct "inferences to the best explanation" – an activity characteristic of nonmodular systems.

The contrast I'm trying to establish here might well be challenged on the ground that any heuristic procedure is an algorithm at heart. If the clue gathering and inferencing of the detective parser can be simulated by a computer, the computer's program embodies the parsing algorithm. A superficial appearance of wise deliberation is perfectly compatible with deep unintelligence. There's something to this, but I think it's a technical defense only. I don't myself write programs much fancier than a dozen lines of BASIC, but friends of mine who do are of the opinion that simulating a detective parser would take a very much more complex program than simulating a so-called algorithmic parser. And the reason is essentially the point that Fodor emphasizes in his characterization of the nonmodular "central systems." A detective parser's computations are *global*. Typically, no one clue will be decisive for sentence structure; each must be weighted and integrated. If clues conflict, then one must be allowed to override another, and so forth. (For example, a noun-verb-noun sequence is a sign of a clause, but this evidence might be outweighed by the presence of a complementizer within it – unless, of course, the assumption that this word is a complementizer turns out to conflict with some other clue somewhere else in the sentence, in which case. . . .) Furthermore, what counts as a useful superficial clue to structure is likely to be highly language-relative, suggesting that the success of a detective procedure requires considerable experience with parsing this particular language. It is therefore hard to see how a workable mechanism could be in place at birth, ready to be applied to any language as its grammar rules are acquired.

To a would-be modularist, then, the detective-style model of human sentence parsing is the opposition. Ironically, by far the best worked out model of this kind is due to Fodor and his colleagues (J. A. Fodor, Bever & Garrett 1974). Algorithmic models have developed more recently and come in a wider variety (see Ford, Bresnan & Kaplan 1982; Frazier & J. D. Fodor 1978; Kimball 1973; Marcus 1980; Wanner & Maratsos 1978). So, should we side with early Fodor or late Fodor on the modularity of sentence processing? I shall claim (a) that though there was a reason for favoring the detective model in 1974, that reason has since gone away; and (b) that in 1984 there are reasons for favoring the algorithmic approach. The arguments supporting these claims will necessarily be given here in barest outline.

The earliest psycholinguistics based on generative linguistics was preoccupied not with modularity but with the relation between competence and performance. It was hoped that the rules and ordering principles of the competence grammar could be construed as an algorithm for sentence parsing. Instead, Fodor, Bever, and Garrett (1974) delivered the bad news! The experimental data would not condone any such identification. The parser might compute the structures defined by the grammar, but it could not be using the rules of the grammar in any systematic fashion to do so; hence the postulation of heuristics for arriving at these structures by trial and error. But then the linguists decided (as linguists are wont to do) that the grammar was wrong. The several kinds of grammars that have supplanted that early variety of transformational grammar differ radically from each other, but interestingly, they all share the property that their rules *can* be employed fairly directly for purposes of sentence parsing.

The algorithmic approach is thus possible after all. What suggests that it is correct is the nature of the generalizations about parsing. For instance, a variety of superficially unrelated ambiguity resolution strategies (e.g., favor a complement clause analysis over a relative clause analysis; favor an active rather than a passive reading of an ambiguous verb) can be seen as consequences of one very general principle: Attach the next word into the phrase marker using as few nodes as possible (see Frazier 1978; Frazier & J. D. Fodor 1978). This general principle apparently applies not only to English but also to the other languages (unfortunately, too few) that have been studied; it is therefore more explanatory than a proliferation of construction-specific and language-specific heuristics. But it works only in the context of a very systematic left-to-right structure-building algorithm.

The general trend of current theorizing thus favors a quite rigid follow-the-cookbook approach to syntactic parsing that is eminently compatible with the existence of a language module. The precise details of this algorithm are the focus of current debate, and one of the issues that has arisen is whether there are modules within the module (see Crain & J. D. Fodor 1984; Frazier, Clifton & Randall 1983). Fodor suggests that there are modules within the visual system (p. 47); but modularization, like explanation, must surely stop somewhere. Among the research questions stimulated by Fodor's book will be "Where?"

Special purpose computation: All is not one

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My approach is to take *Modularity* as a programmatic sketch of the kinds of things it would be worth having a theory about. In spirit, it reminds me very much of Donald Hebb's *Organization of Behavior* (Hebb 1949), and I hope that this book has the same kind of impact. It is the kind of book that I used to argue that Fodor *should* have written.

It is customary to refer to a new Fodor paper as "vintage Fodor," and that appellation is still appropriate. Roughly, the mode of argument is as follows. Fodor first considers what the nature of the language processor *must* be like on the basis of rational considerations, and then he considers whether there is any absolutely compelling evidence to the contrary. Finding none, he then selects evidence that is broadly compatible with his view and uses it essentially for illustrative purposes. I know this description will infuriate many experimental psychologists, who will sense that Fodor is more interested in the issues than in the facts. That is a pity, but I at least take heart that fury is a better response than total indifference.

Basically, what Fodor has done is to tie together a number of theoretical strands connected loosely to the notion that language perception is mediated by a special purpose computational system. He proposes that the proper object of study in both psycholinguistics and cognitive psychology generally should be the set of analytical routines that deliver descriptions of the external world in a format suitable for central inferential systems. These routines are modular in construction. They are mandatory (in the same sense as a reflex), rapid, do not engage central processing capacity, and are informationally encapsulated. These proposals have rough parallels in existing formulations, for example, the Posner–Snyder (1975) distinction between automatic and strategic processing. But Fodor's account is sketched on a much broader canvas, and it attempts to say why these properties should occur together.

Fodor's central concern is to combat the All is One doctrine, which argues for the indivisibility of cognitive processes and has as its aim the reduction of all mental operations to a common set of inferential processes. The specific target for his attack is "the view that sentence processing grades off insensibly into inference and the appreciation of context; into general cognition in short" (p. 91). Fodor objects to two consequences of this view. First, it denies that there is any interesting internal structure to the perceptual-cognitive apparatus. Fodor's aim is quite the contrary. He wants to find some joints to carve at, and he argues that this is a normal condition for scientific progress. The second consequence is related to the first. Fodor argues that we may never understand how the central cognitive system operates (because of its flexibility, etc.), and that unless we can isolate subsystems that operate in a more limited manner, we may never develop any interesting theory of cognition at all.

In Fodor's taxonomy, central cognitive systems are isotropic and Quineian: There is no way to limit the range of facts or beliefs that may play a role in the fixation of perceptual belief, nor is there any fixed method of choosing the best interpretation that can be assigned to the outputs of the perceptual modules. Fodor concludes that this characteristic means we cannot have a *science* of central cognitive processes.

As an example of the lack of scientific advance in this area, Fodor offers the case of artificial intelligence (AI) attempts to build a truly intelligent machine. He might also have contrasted the contents of current texts in cognitive psychology. The sections dealing with what Fodor would treat as input modules are rich in content, whereas the sections dealing with thought and reasoning have probably not changed very much over the past 20 years. This is not to say that little research activity continues in these fields. Fodor's point, of course, is that this research has not led to any significant increase in understanding or even to a clear appreciation of the problems.

The implications of isotropy are difficult for the dedicated experimentalist to grasp. Consider the problem of ambiguity resolution. Katz and Fodor (1963) argued that it was not possible to have a theory of disambiguation, because this would require a systematization of all human knowledge. Any particular fact might be relevant to the resolution of a particular ambiguity, making it impossible to hope that we could eventually have a complete theory. Thus, to understand the terms "horseshoes" and "alligator shoes," we need to know that horses wear shoes and that alligators don't, and also that shoes can be made out of alligators but not horses. This is a point about isotropy. But does it mean that there is nothing for the experimental psychologist to investigate? Surely it is still possible to make scientific claims about how ambiguity resolution occurs? For example, we could investigate whether real-world knowledge is relevant when the ambiguity can be resolved on syntactic grounds. We could ask questions about when disambiguation occurs: Does it occur online, or after the phrase in which the ambiguous term appears, or at the end of the clause?

These questions about the mechanisms of ambiguity resolution come up once the relevant information has been isolated. It is perhaps a question of taste as to whether such questions are worth asking. From one point of view, the only question of interest is specifying how the hearer decides which facts are relevant. Attempting to finesse this issue is analogous to constructing a theory of parsing without any theory of syntax. I think that Fodor's main aim here is to focus attention on what has been left out of the account, not to argue that it is pointless to investigate any aspect of the total process. Hence, the experimental psychologist does at least have the option of studying one small part of the total problem. Unfortunately, this option is not open to AI researchers, whose task is to design an algorithm that will resolve any arbitrarily chosen term. Any attempt to offer a partial solution (say, by designing an algorithm that works in an artificially restricted domain) totally begs the ultimate question.

Two further issues should be raised in this context. First, doesn't Fodor's pessimism concerning central cognitive processes undermine any attempt to understand the modular systems? The reason for asking this question is simply that all current methods of interrogating the internal states of the processor work through the central cognitive processes. Thus, for example, the time required to classify a letter string as a word or a nonword is taken as an index of the time required for the language module to contact the relevant entry in the mental lexicon. But the act of deciding which response to make is very much a central process, and hence isotropy rears its head again. Of course, experimental psychologists are slowly coming to appreciate this point; it is obvious that decision making is affected by all manner of "extraneous" items of information. For example, repeated items in a lexical decision experiment are classified faster. Does this mean that the lexical access module has been "primed," or does it mean that the subject notices the repetition and uses this information to speed up decision? Or, to take another example, does the slower response to a word presented in an inappropriate sentential context mean that lexical access is inhibited or that the decision maker can't help taking appropriateness into account, even though it is irrelevant to the task?

Because the decision process is obviously isotropic, does it follow that a science of input modules must await the development of more direct methods of interrogation that bypass the central cognitive processes? I assume that the answer is no and that the right way to deal with this problem is to treat it as a case of specifying adequate boundary conditions. Experiments on lexical access must be evaluated in terms of how well they meet the boundary conditions for a good experiment. Lexical decision times will reflect lexical access times only to the extent that irrelevant influences have been eliminated. These irrelevant influences can be identified and eliminated (or so we hope), even if we don't have an adequate theory of decision making that will enable us to predict which experiments are good and which are bad. I take it that the same problem arises in all science. An experiment measuring gravitational forces will doubtless be influenced by nearby earth tremors. But this influence can be identified after the fact, even if we know absolutely nothing about how tremors occur or when they will occur. Our inability to predict tremors should not be taken as grounds for abandoning physics.

The second problem is perhaps more serious. Recall that the language input module is informationally encapsulated, meaning that strict limits govern the information that this module consults during its operation. Now, there are many "interactive" theories of language processing that argue that even the most fundamental operations (e.g., lexical access) have access to real-world knowledge, as shown by the fact that words forming plausible completions of a sentence take less time to process than they would if there were no context. Fodor assaults this evidence head-on, pointing out that whatever context may say about *content*, it cannot say anything about *form*. He also points out that the inhibitory effects obtained for implausible completions suggest that some additional (central) processes are involved; the language processor could scarcely be committed to predicting which words were *not* likely to occur.

I agree totally with these arguments, and I believe that there are good grounds for discounting the evidence (e.g., see Forster 1981). Yet I also think that there are good grounds for assuming that plausibility does play a role at some later stage of processing, given that implausible sentences appear to take longer to process in a variety of tasks, notwithstanding earnest admonitions to the subjects to ignore the meaning of the sentence and even though meaning is totally irrelevant to some of the tasks (e.g., matching two sentences on the basis of form). In short, the plausibility effect appears to be online and mandatory, and it apparently affects processes that yield relatively shallow descriptions; more precisely, it affects tasks such as form matching that logically require only shallow descriptions. Hence the effect should be seen as a reflection of the properties of a modular system. But the plausibility of a sentence such as "John tickled Mary with an axe" could not be relevant to any informationally encapsulated system, because plausibility is an isotropic property.

There are two ways to handle isotropy (assuming that the currently available facts are not totally misleading). First, we could simply argue that not all mandatory processes need to be assigned to some modular system. Hence, being mandatory is a necessary but not a sufficient condition for being modular. This definition means that some part of the central system that deals with the output of the language module is involuntarily susceptible to plausibility. This characteristic weakens Fodor's claim, because one of the properties that distinguish vertical from horizontal faculties is thereby lost.

The other alternative is to suggest that there may be some modular systems that are not informationally encapsulated. For example, one possibility is that the plausibility effect arises during sense selection. Even if a word is not strictly ambiguous, there are still aspects of its meaning that can be determined only contextually, for example, the different senses of "comfortable" in phrases such as "comfortable chair" and "comfortable job." It seems fairly clear that this operation could not possibly be informationally encapsulated; one needs to have access to general information about chairs and jobs in order to understand how they can be comfortable. Obviously, this process of sense construction is likely to be very sensitive to plausibility effects, as in cases such as "comfortable alligator," or "comfortable spoon." It could be suggested that such implausible phrases involve a processing cost when time is spent trying to discover the relevant facts that will make sense of the phrase. For Fodor, this alternative would also be unattractive, because it still means that one loses a property that distinguishes between vertical and horizontal faculties, namely, informational encapsulation. Fodor would want to look elsewhere for an account of the plausibility effect.

One place to look is in some kind of error-detection process. When the output of the language module is discovered to be highly implausible or nonsensensical, an error flag is raised, and the input is resubmitted for analysis. Who raises the flag? Presumably, some central cognitive process that is not informationally encapsulated. The only problem with this account is that it might be impossible to distinguish between a nonencapsulated language processor and an ecapsulated processor that works in tandem with and is closely monitored by a nonencapsulated system. (I should stress that this is not just a problem for Fodor's account but applies equally well to other proposals concerning the "autonomy" of processing levels, e.g., Forster 1979). To make the distinction, we need to discover a processing task that recruits the output of the language module but ignores plausibility. This approach may be impossible if the only accessible level of representation is one that takes plausibility considerations into account.

Fodor looks in a different place, however. He suggests that whenever it *appears* as if the input module is not encapsulated, it is really simulating a more flexible and intelligent device. Consider, for example, that the lexical access module is impressed by the relatedness of terms such as "doctor" and "nurse." Fodor does not assume that this recognition shows that the access module can go to background information about doctors and nurses. Instead he assumes (along with many others) that there are internal links among lexical entries. These links are literally just associations. That is, the lexical module knows that the terms are connected in some way, but not why. As Fodor puts it, "associations are the means whereby stupid processing systems manage to behave as though they were smart ones" (p. 81). One could imagine explaining plausibility effects in these terms. The words in a plausible sentence are likely to be more closely related than those in an implausible sentence, which may assist the language input module in some way. There is a problem, however. Plausibility can be manipulated without varying lexical content: "cows produce milk" is a more plausible assertion than "milk produces cows," and this difference makes a difference in processing time (Ratcliff 1983).

My current view, for what it is worth, is that many of our tasks are sensitive to well-formedness at too high a level. From the point of view of the central cognitive system, something is badly wrong with the assertion that milk produces cows; and in many tasks that require judgments of well-formedness (the speeded grammaticality task, for instance), the decision-making system has difficulty in restricting its attention to the relevant sorts of evidence. Thus, for example, it might be that in making judgments of syntactic well-formedness, we can successfully ignore truth value but not coherence.

In short, it is far too early to determine whether Fodor's thesis can be sustained. New insights await new technology. I am not pessimistic on this score. Swinney's application of the lexical priming technique (Swinney 1979) made it possible to show that all meanings of a word are initially accessed during sentence processing. Prior to this development, the chances of getting this kind of evidence seemed pretty slim. In our own laboratory, my colleagues and I are working on a technique that may allow us to distinguish between what one actually perceives and what one infers that one has perceived. For example, a subject is shown the following sentence under RSVP conditions (i.e., very fast, one word at a time): "Mary put the kettle on the slove when the guests arrived." Most subjects are of the opinion that they saw the word "stove," an opinion not shared by subjects who received the same input except that "kettle" became "kitten." But does this discrepancy indicate that the latter subjects' lexical processors accessed the entry for "stove" or that the subjects merely inferred that the sentence must have contained the word "stove"? For Fodor (and anyone, really), this is an absolutely crucial question, but it has never been addressed experimentally.

A modular sense of place?

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We share Fodor's belief that the mind is composed of modules, with restricted access to the potentially relevant data or, equivalently, modules designed to deal with a restricted class of data. We disagree that this mode of organization does not extend to central systems.

Recent experimental evidence gathered by one of us (Cheng 1984; in preparation) suggests that of all the information available to a rat for determining where it is in an environment, the animal relies primarily on purely geometric information, information about the metric configuration of surfaces in the environment. This finding suggests a place-determining module that records only geometric information and not nongeometric properties of surfaces and locales, such as reflectance characteristics (black vs. white), texture (smooth vs. rough), and smell (anise vs. peppermint). The data suggest that these nongeometric properties, though recorded in some central system(s), are less accessible for determining place, even when reinforcement contingencies strongly favor their use. The place-determining module is akin to a purely geometric map with no labels or special symbols on it, a map recording only the metric configuration of surfaces.

One line of evidence for our claim is that a rat required to remember a location often confuses it with other locations that are geometrically equivalent to it. Geometrically equivalent locations stand in the same geometric relations to all surfaces qua surfaces but differ in their relations to nongeometric properties. Figure 1 illustrates an environment with purely geometric ambiguity. The environment is rectangular, but the surfaces making up the rectangle differ in nongeometric properties: One long wall is white; the other is black. Distinctive panels differing in appearance, texture, and smell make up the corners.

Suppose that a rat must remember a location within such an environment. If the animal uses only the geometric information to make a match between its cognitive map and the perceived environment, two equally good congruences can be found – one "correct" congruence and one "erroneous" congruence, in which the map is rotated 180° with respect to the environment. The latter is a match only on geometric grounds. There is no ambiguity if nongeometric information is taken into account. If nongeometric information is also on the map, then when the map is rotated 180° with respect to the perceived environment, the nongeometric information on the map will not match the nongeometric information in the environment, even though the surfaces on the map line up with the surfaces of the environment. If an animal systematically makes rotational errors that arise when the map is misaligned by 180° with the environment, then we have evidence for a central module that relies primarily on purely geometric information in determining place.

In a "working memory" experiment run in this rectangular environment, the location to be remembered in each trial was chosen at random from among 80 locations. An animal was shown the location of food; it was then removed and put back in

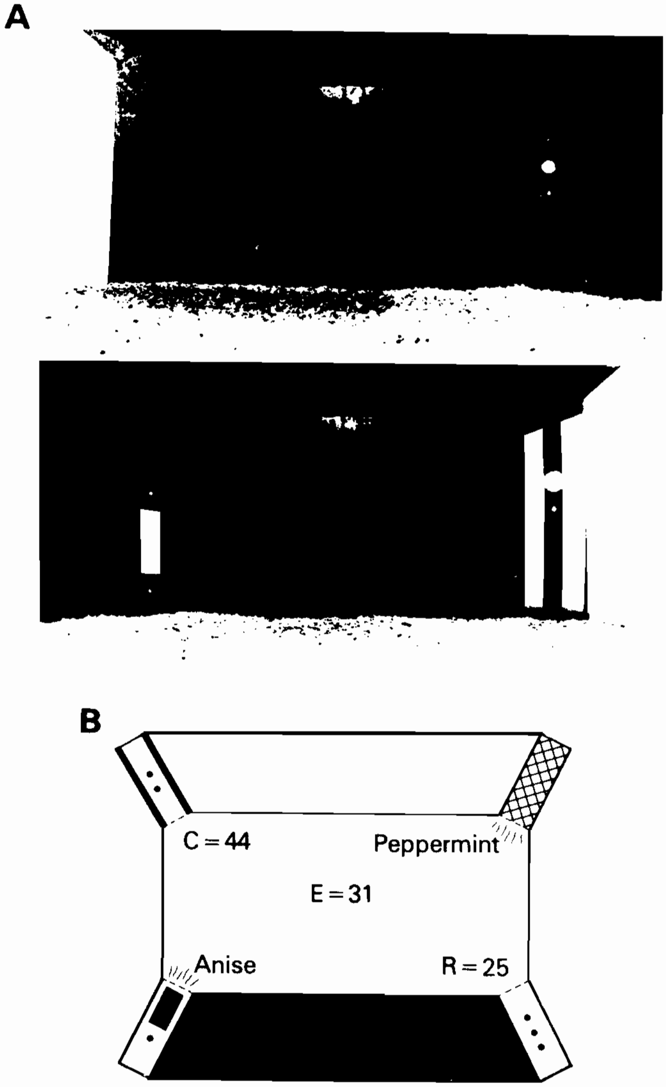

Figure 1 (Gallistel and Cheng). A rectangular environment used in experiments by Cheng (in preparation). A. The two ends of the rectangular box as seen by a rat placed at the center. The panels in the corners differ in smell and texture as well as in visual appearance. B. A "plan" of the environment, along with the results from a working memory experiment. Rats were shown food at a randomly chosen location within the box for each trial; they were then removed, and put back in an exact replica of the box 75 seconds later, with the food buried. The figure shows the average percentage of trials on which they dug at the correct location (C), at the geometrically equivalent location of 180° rotation through the center from the correct location (R), and elsewhere (E). The data, though shown at a single location, are averaged across locations and rats. The data from individual rats do not differ significantly from the grouped pattern, nor was there any effect of locations.

an exact replica of the environment 75 seconds later, with the food buried. In the majority of trials, the animals chose to dig at one of the two geometrically correct locations. Between the correct location and the rotational error, however, each animal chose about equally often (Figure 1B). No animal chose the correct location significantly more often than the erroneous but geometrically equivalent location.

In "reference memory" experiments, where the food stayed in the same corner from trial to trial, rats learned to choose the correct corner in preference to its geometrically equivalent diagonally opposite corner. They did so even when all the walls were black. This experiment shows that rats can use the nongeometric information contained in the panels. Nonetheless, many diagonal errors were made, indicating that it was easier for the rat to learn the geometric characteristics that define the correct place than the nongeometric characteristics.

After the rats had learned to choose the correct corner, removing the distinctive panels from that corner and the geometrically identical diagonally opposite corner led immediately to 50–50 choice between the two corners, even though the distinctive nongeometric data on the panels in the remaining corners could have been used to find a unique congruence between map and environment. Furthermore, when the panel in the correct corner (along with the food) changed places with the diagonally opposite panel, thereby strongly altering the configuration of the nongeometric information on the four panels, the rats' performance was unperturbed by the resulting featural incongruence: They continued to choose the geometrically acceptable corner with the "correct" panel (the corner diagonally opposite the one they had been choosing). On the other hand, a geometrically less drastic affine transformation of panel configuration, produced by moving all four panels one corner over, led to a decline in performance. The affine transformation preserves all but the metric properties in the geometric relations among the distinctive panels, but it carries the "correct" panel into a geometrically different corner. The panel's current location within the overall metric configuration of the environment cannot be made congruent with its previous geometric address. [See also Olton et al.: "Hippocampus, Space and Memory" *BBS* 2(3) 1979.]

These results suggest that, to establish where it is, the rat first consults a representation containing only geometric information. Checking for correct nongeometric information is done only locally. We hypothesize that geometric addresses may be used to access nongeometric information stored in other modules. Thus, when the nongeometric information is in a location with the correct geometry, the rat may use it to verify whether it has established the correct congruence between its map and the perceived environment. But when it is in a geometrically wrong location, as in the affine transformation, the animal does not use nongeometric information to check the geometrically achieved congruence. The rat may seldom or never try to establish global congruence for the configuration of nongeometric data.

In sum, we seem to have a central system that has limited access to the potentially relevant data for the fixation of belief. Here is a system that records something about the distribution of surfaces for the later organization of motivated action in search of food. Both geometric and nongeometric information are potentially relevant, but we believe that the module that establishes global congruence between the map and the perceived world uses only the geometric data. If we are correct, then the rat's place-finding abilities rest on modularized information processing.

Some data (Crawford 1941; Tinklepaugh 1932) suggest that in searching for hidden food, chimpanzees also rely primarily on the global metric configuration of surfaces rather than on distinctive nongeometric features of the food location. This finding emboldens us to suggest that a similar modular organization may also be found in humans, although we know of no compelling evidence for this view. More generally, we think that Fodor's assumption regarding the nonmodularity of central processes may be unduly pessimistic. Although humans can bring diverse data to bear on belief fixation, different kinds of data may be encoded and organized by separate modules. Ready accessibility may hide a deeper modularity in the theoretical units underlying mentation. The interconnected Quinean web may be organized largely by a *number* of central spiders, each weaving its own brand of information.

The centrality of modules

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In 1981, just as I was beginning to write a book on "multiple intelligences," I learned that Jerry Fodor was preparing an essay on modularity. I mentioned our common interest to him and also sent him an early paper on my theme. When I next saw him, Jerry remarked, "Well, what you are doing has nothing to do with what I'm doing" (I quote from memory). Even allowing for Fodorean hyperbole, this reaction seemed excessive, and so I looked forward to making my own comparison. As it happened, our books were published within a few months of each other, and, at least in my own mind, they contribute to a more pervasive discussion on these themes in the cognitive sciences (see also Flanagan 1984). I have elected in these comments to compare our positions. I hope that such an exercise will prove of more than parochial interest.

Similarity depends upon perspective. When contrasted with the views of John Anderson (1983), Roger Schank (1972), or the followers of J. J. Gibson (Turvey, Shaw, Reed & Mace 1981), Fodor and I are embarked on neighboring courses. On the other hand, I readily recognize that Fodor's views are closer to Chomsky's than they are to mine and that he approaches his task from the perspective of a philosopher. My views, on the other hand, are irremediably psychological (Quinean is not part of my vocabulary). Still, I think that Fodor's initial distancing was overwrought.

Agreement need not be symmetrical, of course. Fodor might disagree with the theory of multiple intelligences, even though I am much in sympathy with the views expressed in *Modularity*. For the record, I broadly endorse his move to modules, his distinction between horizontal and vertical faculties, and his reading of current experimental literature. He has properly noted the enormous difficulty of accounting (within any current framework) for the fixation of belief, the solving of complex problems, the discovery of analogies, and the propounding of scientific theories. Endlessly provocative, Fodor's essay advances thinking in the cognitive sciences.

Nearly all of my troubles with Fodor's treatise stem from our different versions of *vertical faculties* (I shall use this neutral term whenever I am trying to characterize both Fodor's "modules" and my "intelligences"). Fodor's modules are very tightly designed input mechanisms, modeled after reflexes, which apparently go off without respect to the environment and which are part and parcel of standard human operating equipment. Fodor sharply distinguishes these modules from a qualitatively different mental mechanism, which he calls a central system. In contrast, my "intelligences" contain a core processing mechanism – something like Fodor's module – but are influenced from early on by the surrounding cultural environment ("interpreted," Fodor might say) and undergo a lengthy and often complex developmental history. Perhaps for these reasons, I find no need for a central system or executive process. Instead, I find it more useful to think of all mental processes as ranging on a continuum, with relatively modular mechanisms (like pitch discrimination or shape constancy) at one end, and relatively isotropic mechanisms (like musical composition or architectural design) at the other. I feel that Fodor's approach achieves a certain decisiveness, but at the cost of ignoring the most important human cognitive achievements. My approach, though admittedly more tentative, has the virtue of suggesting some ways of explaining human behaviors that transcend reflexes.

Our differing notions of vertical faculties reflect methodological and theoretical predilections. To begin with the selection of modules themselves, Fodor compiles his current list (the perceptual systems and language) from a mixed bag of logical (e.g., domain specifying) and empirical (e.g., fast operation) criteria, which strikes me as idiosyncratic: if language, why not music, for example? I am aware that Fodor does not place much stock in the particulars of his list, but the arguments that he marshals for each of the criteria of modules strike me as equally unconstrained.

In my own effort to state criteria for an intelligence (Gardner 1983, ch. 4), I have restricted myself to empirical considerations and have sought to survey the evidence with respect to each candidate intelligence in as systematic a fashion as possible. Indeed, in *Frames of Mind*, I arrive at a list of seven intelligences after surveying eight different sources of information, ranging from the possibility of prodigious behavior in an area to the existence of organic syndromes in which a single "faculty" has been destroyed or spared in isolation. Researchers can (and already have) disagreed with my resulting septet, but at least others have the option of evaluating the evidence that I have culled in favor of each intelligence.

A key point of contention has to do with the extent to which vertical faculties can be considered in isolation from the surrounding culture. Fodor writes as if each of his modules simply unfolds, independently of the symbol (or interpretive) systems being used by the culture. In my view, a fully encapsulated module is an ideal observable only in freaks (for example, *idiot savants* or autistic children). Even phoneme perception and sensitivity to visual illusions are affected by the kinds of sounds or sights present or absent in a particular culture. Instead, I adopt the view that there is a core computational capacity at the center of each intelligence, which can be observed in uninterpreted form early in life; however, from early on, this core is brought to bear on and is strongly fashioned by patterns encountered in the culture – natural languages, musical systems, mathematical systems, and the like. The highest human activities depend on the ability to access these core capacities (cf. Rozin 1976). Encapsulation gradually dissolves, though it is never completely eradicated; the "core" may become visible again under certain conditions of brain damage.

To point to another area of dispute, I believe that any vertical account should entail a developmental perspective. (Fodor is, of course, well-known for his antipathy to such accounts.) In my view, we will never attain even a first-level understanding of the principal forms of thought unless we trace their evolution from the relatively modular and encapsulated forms of processing, which can be observed in infancy, to the far more open or "isotropic" forms characteristic of mature individuals. (Indeed, I think that the difference between encapsulated and unencapsulated or isotropic forms may prove to be less of a systemic and more of a developmental phenomenon.) My own view is that every "intelligence" has a developmental history, which after the first year or so of life involves engagement with the symbol systems of the culture, and which culminates in the mastery of entire cultural domains by adolescence or thereafter. Thus we may begin with the proclivity to analyze sounds, or even to parse phrases, in certain ways, but each of these processes undergoes perpetual reorganization in the light of the particular experiences encountered by an individual over the course of life. A nondevelopmental account of modularity veers away from what is distinctive about human (as opposed to digital computer) cognition.

It should be clear, then, that this developmentally oriented researcher highlights a different set of concerns in his brand of vertical faculty psychology. In a sense, perhaps, Fodor's initial remarks to me were on the mark. But in another sense, I am more Fodorean than Fodor (and perhaps closer to Chomsky): I am not ready to posit a wholly different set of processes of the "central system" variety. I think that, just as Fodor was driven to archnativism because of the difficulties of understanding how knowledge can be acquired (or concepts formed), he has been driven to a "central system" position because he cannot figure out how the kinds of thinking and argument in which he (and the rest of us) engage could possibly be modular.

It should be conceded at the outset that no one has privileged or extensive information on this topic: We are all weaving "just-so stories," especially when it comes to vertical faculties beyond the visual-perceptual system. Moreover, there remains the herculean task of carving out the distinction among various vertical approaches (modules, intelligences, society of mind, production systems, mental organs, on the one hand) and a contrasting set of concepts (homunculi, executive processes, central systems, general problem solvers, on the other). Still, I would like to place my own cards on the table.

To begin with a nonempirical point, an analyst should favor *one* cognitive system over *two* on the grounds of parsimony. If one posits a second type of system, it becomes necessary to trace two evolutions, two forms of hardware, systems of connections and communications, and so on – nontrivial scientific assignments.

As already suggested, I would prefer to blur any distinction between modules and central systems. In my Ockhamite view, there are relatively modular processes at the center of every cognitive activity, but there is also some degree of penetration or cross talk everywhere. As elements become more susceptible to automatization (squiggles come to be seen as letters), their processing seems more encapsulated; but as elements become subjects of special scrutiny (the typographer critically compares fonts) they seem less modular. I would replace a dichotomy with a continuum, from more encapsulated (or modular) to less, allowing processes to move in either direction.

Fodor's argument for central systems invokes neuropsychological evidence. He finds no evidence for any isolated breakdown (let alone neural localization) of his isotropic processes, such as those involved in fixation of belief or the solving of problems. I read this literature very differently. Certainly one encounters a variety of acalculias, each exhibiting a different deficit in mathematical reasoning. Head (1926) spoke of a semantic aphasia, Luria (1966) of logical-grammatical disorders, many writers of special problems in conceptualization arising from left posterior lesions. All of these relatively "open" or isotropic processes exhibit neurological localizations and can break down in relative isolation from other capacities. My own work with organic patients has documented a variety of special difficulties in interpreting and integrating narratives in patients with unilateral light hemisphere lesions, even as aphasic patients show remarkably preserved capacities to deal with these linguistic entities. So even if these capacities prove less potently modular than syntactic parsing or face recognition, they are hardly as equipotential or "central" as Fodor suggests.

Fodor's own claims about central systems are not sufficiently spelled out. At one point (p. 55), he acknowledges that there may be a need for some kind of executive control over central representational capacities, but he lets this idea drop. He offers no account of whether the modules simply communicate with one another (a form of "centrality" with which I am in full sympathy) or whether there is a wholly separate system that supervises the communication. Nor in the latter event does he indicate whether this coordinator is "as dumb" or "non-isotropic" as the modules or whether it is a kind of omniscient or homuncular entity that "knows" and "directs" subsidiary modules.

In general, Fodor raises the issue of how we can have beliefs or do science – and then throws up his hands and says these processes cannot be illuminated by cognitive science. I hold that the notion of modules harbors the potential to account for the range of human cognitive activities and that it is crucial to attempt to account for all performance using a vertical approach. If one broadens the notion of module, it may be possible to illuminate, rather than to eliminate, the study of higher cognitive processes. (This, in a sentence, is the rather grandiose program of *Frames of Mind*).

One can span the distance from modularity *strictu sensu* to complex cognitive accomplishments by exploiting two assumptions. First, each module sooner or later becomes involved in a range of highly complex symbol-using activities, which makes it less strictly encapsulated, in Fodor's sense. With development, the "encapsulated module" becomes increasingly fictional. The poet exploits his language faculty, the composer a musical faculty, the scientist a logical-mathematical faculty; but this very deployment, taking place within a highly meaning-laden context and reflecting many years of development, necessarily involves those reflective and integrating capacities that Fodor perforce places in a central system.

The second assumption is that modules can and do work together in any complex human activity but that such cooperation requires no higher-level supervision. The dancer uses musical and bodily faculties, the novelist employes linguistic and personal faculties, the lawyer draws on a combination of linguistic, logical-mathematical, and other personal faculties as well. Just as ordinary human beings can come to work together cooperatively, each using personal skills without the need for a master conductor, so, too, various modules, faculties, or intelligences can come to work together in carrying out complex cultural tasks. (On my own analysis, one of the principal means by which we [or our "intelligences"] learn to communicate is by observing other individuals whose intelligences are engaged in cooperation and cross talk.)

Certainly, before we invoke a master executive or a fully isotropic central processor with the additional theoretical baggage that such assumptions entail, we ought to see how much of human cognitive accomplishment can be subsumed under a "vertical-faculty" account that recognizes developmental histories, meaningful symbolic systems, and the possibility for communication among once encapsulated and isolated computational systems. I suspect that when such an exercise has been carried out, the perceived need for a central processor or central system will evaporate.

ACKNOWLEDGMENT

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Modularity: Contextual interactions and the tractability of nonmodular systems

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This is both an infuriating and a stimulating book. The psychological claims are ill-founded (as I shall try to document), but at the same time the issues that Fodor raises are important and tractable. Fodor makes two broad claims about the mind. The first is that information from the world passes through three distinct stages or systems on the way to the mind: a sensory transducer system, a perceptual input system (or systems), and finally a central cognitive system. The second general claim is that the language processing system (and the musical and mathematical systems as well?) consists of input systems that are designed exactly like the perceptual input systems. These perceptual and language input systems are vertically organized and modular: they are innate, neurologically distinct, and hardwired; unlearned, they operate automatically and reflexively, they are domain specific, and, most important for computational models of information processing, they are informationally encapsulated. Because of this last property, they have access only to information (a) from within the domain to which they are dedicated, and (b) within that domain, only to information from below, that is, from stages of processing at lower levels. This characteristic, of course, permits only one kind of information-processing system, namely, the strictly bottom-up, data-driven kind.

To what extent are these two broad claims true? The first claim must be at least partially true. But, from what we know of, say, color vision or object perception, this first claim – that there are three types of systems – is hopelessly simplistic. The visual system may indeed be very roughly characterized as consisting of transducers (i.e., rods and cones) and intermediate input systems (possibly including rods and cones in addition to higher centers), all leading to a modality-independent central cognitive system (perhaps the mind's eye?). But such a characterization tells us nothing more than that (a) raw sensory data is transformed so that it is interpretable at higher levels in the system, and that (b) there is a level that is modality and domain independent, and at this level information from various input systems may be compared, integrated, and perhaps stored. This characterization, then, is not particularly novel or useful. What are useful and stimulating are the claims about (a) the analogy between the language and perceptual input systems; (b) the modular characteristics of these input systems; and (c) the impossibility of making scientific progress with nonmodular, general cognitive systems. I will comment briefly on each of these claims, starting with the last.

Fodor argues that scientific progress can be made only at the level of modular systems: "the limits of modularity are also likely to be the limits of what we are going to be able to understand about the mind" (p. 126). Only relatively simple systems that behave in isolation as they behave in nature can be successfully studied, so "If . . . the central cognitive processes are nonmodular, that is very bad news for cognitive science" (p. 128). This argument seems plausible, but there are two compelling counterarguments. First, we have made reasonable progress in understanding cognitive systems that are not modular in Fodor's sense of the term. Human memory, for example, is now far better understood than it was in Ebbinghaus's day, even though human memory is clearly not a collection of informationally encapsulated, vertically organized modules. We also understand more about how people play chess than we did 20 years ago, a fact that might have prompted Fodor to speculate that perhaps chess, too, might be modular and hence innate, hardwired, and so forth.

The second counterargument to the claim that nonmodular systems are intractable involves just those systems that Fodor claims are modular, but are not – the perceptual input systems. The central reason for our ability to study modular systems profitably is their insularity: They are informationally encapsulated, and so they do not interact with information from other input systems or from higher-level systems, such as the central cognitive system. Therefore, when artificially isolated for study, they behave just as they behave normally. But it is precisely at the level of perception (here understood to mean phenomenological experience, not belief) that such insularity is the exception rather than the rule. Perceptual phenomena are notoriously context sensitive. They are of course sensitive to perceptual context, but they are also sensitive to past experience (learning) and to belief. For example, the colors that we experience are demonstrably affected by past experience (e.g., orientation and motion-specific color aftereffects that can persist for months) and by belief (e.g., the effects of perceived form and contour upon color contrast). Yet such interactions have hardly impeded our understanding of color perception. Indeed, the discovery of interesting interactions has led to deeper understanding. So, to the extent that scientific progress can be made with systems that are not strictly modular – such as the human memory system or the color perception system – Fodor's argument fails.

This view is of course related to the second important claim, that perceptual input systems are modular and so are hardwired, innate, unlearned, and informationally encapsulated. I have just noted that even so innate a system as color perception is subject to learning (e.g., orientation-specific color aftereffects) and is not informationally encapsulated. The same can be shown for virtually all other perceptual systems. There are clearly demonstrable interactions between perceptual systems, such as between vision and hearing, and there are also interactions between cognitive levels, such as between beliefs and perceptual experience. For example, our perception of the fragmentary Street figures (Leeper 1935) or "Droodles" (ambiguous line drawings by humorist Roger Price) is highly influenced by our beliefs about what these visual patterns represent. So if Fodor wishes to argue for the modularity and autonomy of the language system on the basis of their equivalence to perceptual input systems, this argument is misplaced. Perceptual systems seem not to be strictly bottom-up, noninteractive devices.

This means, of course, that the last claim about the analogy between language and perceptual processing systems is no longer interesting. The claim that the language processing system is strictly bottom-up, autonomous, and informationally encapsulated must stand on its own and be evaluated in terms of the available data. Here Fodor makes a strong case for the noninteractive position, but he does so by selective sampling of the experimental literature and by providing alternative explanations for those cases of contextual interactions that he does report. In all fairness, the empirical case is far from settled, and we can be grateful to Fodor for spelling out the issues so clearly and forcefully. So, while there is a great deal to disagree with in the book, there is also a great deal to stimulate as well.

This is the strength of the book. It stimulates philosophical and theoretical thought, and at the same time raises issues that are empirically testable. This is a rare accomplishment indeed.

Fodor's holism

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I like Fodor's *Modularity*, and I think the first three chapters of the book are plausible and probably true. The penultimate chapter, on central processes, seems to contain half of what Fodor has to say about the functioning of mind. I don't believe it, partly because I don't see what it is exactly that Fodor claims about central processes, but in any case it seems to me that Fodor does not make the important issues about holism and the fixation of belief fully clear. Fodor's thesis seems to be that the central processes in us by which we fix belief operate holistically. I claim that the striking thing about us is that both individually and culturally we learn not to be holistic in the fixation of belief.

Together, the following statements capture Fodor's chief claims about central processes and the fixation of belief:

1. "Looked at this way, the claim that input systems are informationally encapsulated is equivalent to the claim that the data that can bear on the confirmation of perceptual hypotheses includes, in the general case, considerably less than the organism may know. That is, the confirmation function for input systems does not have access to all of the information that the organism internally represents; there are restrictions upon the allocation of internally represented information to input processes" (p. 69).

2. "By saying that confirmation is isotropic, I mean that the facts relevant to the confirmation of a scientific hypothesis may be drawn from anywhere in the field of previously established empirical (or, of course, demonstrative) truths. Crudely: everything that the scientist knows is, in principle, relevant to determining what else he ought to believe. In principle, our botany constrains our astronomy, if only we could think of ways to make them connect" (p. 105).

3. "By saying scientific confirmation is Quineian, I mean that the degree of confirmation assigned to any given hypothesis is sensitive to properties of the entire belief system; as it were, the shape of our whole science bears on the epistemic status of each scientific hypothesis. Notice that being Quineian and being isotropic are not the same properties, though they are intimately related. For example, if scientific confirmation is isotropic, it is quite possible that some fact about photosynthesis in algae should be relevant to the confirmation of some hypothesis in astrophics [sic] ('the universe in a grain of sand' and all that). But the point about being Quineian is that we might have two astrophysical theories, both of which make the same predictions about algae and about everything else that we can think of to test, but such that one of the theories is better confirmed than the other—e.g., on grounds of such considerations as simplicity, plausibility, or conservatism. The point is that simplicity, plausibility, and conservatism are properties that theories have in virtue of their relation to the whole structure of scientific beliefs have *taken collectively*. A measure of conservatism or simplicity would be a metric over *global* properties of belief systems" (pp. 107–108).

4. "I am suggesting that, as soon as we begin to look at cognitive processes other than input analysis – in particular, at central processes on nondemonstrative fixation of belief – we run into problems that have a quite characteristic property. They seem to involve isotropic and Quineian computations; computations that are, in one or other respect, sensitive to the whole belief system. This is exactly what one would expect on the assumption that nondemonstrative fixation of belief really is quite like scientific confirmation, and that scientific confirmation is itself characteristically Quineian and isotropic. In this respect, it seems to me, the frame problem is paradigmatic, and in this respect the seriousness of the frame problem has not been adequately appreciated" (pp. 114–115).

5. "To put these claims in a nutshell; there are no content-specific central processes for the performance of which correspondingly specific neural structures have been identified. Everything we now know is compatible with the claim that central problem-solving is subserved by equipotential neural mechanisms. This is precisely what you would expect if you assume that the central cognitive processes are largely Quineian and isotropic" (p. 119).

I am unsure how to understand the modals, the "cans" and the "possibles," in these passages. Is Fodor claiming that when we determine whether or not to accept a conclusion we somehow apply a measure or criterion, or determine a relation, which requires us to consider individually every other belief we hold? Is Fodor claiming that when we set about to get evidence pertinent to a hypothesis we are entertaining, we somehow consider every possible domain we could observe? It sounds very much as though he is saying that, but of course it is not true. The trouble with philosophers and holism, whether Quine or Putnam or Kuhn, is that the claims about holism they seem to make, and that sound interesting, are so palpably false, and they do not trouble to distinguish them from perhaps less interesting claims that might be true.

The following statements all seem true of the acquisition of scientific knowledge:

1. In arguing for a theory or hypothesis, or in getting evidence pertinent to it, scientists appeal to a very restricted range of facts. Astrophysicists never cite botanical facts, except out of whimsy, and botanists never cite facts that are extragalactic. I defy Fodor to find a twentieth-century paper in which someone not crazed is concerned with the consistency of astrophysics and botany.

2. We are capable of detecting inconsistencies between theo-

ries of remote subjects, such as astrophysics and terrestrial evolution, and revising them in consequence.

3. Many of the methods used in the special sciences are applications of the same general principles, whether about confirmation, analogy, or explanation.

I think these points are banal. Their implications are not. The third point argues that we are possessed of domain-independent strategies for forming beliefs. The first point argues that we somehow learn to apply these strategies in special domains in such a way that the domains become encapsulated, with most of them isolated from most of the others. The second point argues that the learned encapsulation is defeasible.

The essential questions are these: What are the domain-independent strategies that may be used to fix belief? How do they become, in their application to particular domains, encapsulated? How do we manage to encapsulate our theories and the evidence pertinent to them while maintaining such global virtues as consistency? What happens when contradictions are found between otherwise isolated domains – do we decapsulate them both, reverting to a more primitive strategy, or do we encapsulate them still, but within a new and larger compartment? These are essential questions about the fixation of belief. They may be understood differently according to whether or not one is concerned with a computational model of mind. Each has partial, noncomputational answers scattered throughout the literature of philosophy of science and, happily, increasingly in artificial intelligence work as well.

There are several ways in which we ensure, or help to ensure, the global virtue of consistency without resorting to global calculations. One is by isolation of predicates. No psychoanalytic or psychometric claims will contradict claims of physics unless they are already inconsistent within psychoanalysis or psychometrics. The languages are simply different. Another means by which we help to ensure consistency is by establishing routes from one theory to another and verifying their consistency at appropriate checkpoints. Thus we do not need to determine that theories of fundamental particles are consistent with organic chemistry, pharmacology, molecular biology, or whatever. If the theories are consistent with (or approximately consistent with) quantum theory, then they will be consistent with theories of larger structures as well, provided quantum theory is. They have no other access. In these ways, and no doubt in others, we ensure that global virtue by local means. It is part of the structure and point of intertheoretical reductions and explanations, and that is one of the reasons why it is so very mistaken to claim that explanation is not transitive. The mechanisms that ensure that consistency can be determined locally also ensure that simplicity can be determined locally. Astrophysics can only make botany less simple, while still being consistent with established botanical theory, if astrophysics forces some new botanical conclusions that must somehow be incorporated within botanical theory. Similarly with particle physics and organic chemistry. Since the one subject has no implications for the other, or has implications only through a fixed set of intermediating hypotheses (e.g., giving nonrelativistic quantum theory as an approximation), developments in one subject generally have no influence on the simplicity of theories in remote subjects.

How do we learn to encapsulate domains when we have initially applied to them strategies that are domain independent? In truth, I have no idea just how we do so, or how we could make an android do so. The philosophy of science literature does suggest some strategies. In the first place, if we start with domain-independent strategies and end up with strategies that are particular to a domain, it appears that we have learned something and that our set of beliefs, or "background knowledge," somehow generates more specific methods. Consider causal explanation, and suppose that Salmon (1971) is roughly right about how it proceeds: We explain a kind of effect by locating all of the factors that are statistically relevant to it. If we start with primitive conceptions of kinds, we can construct lots of other kinds therefrom by standard definitional procedures and collect conclusions about what factors are relevant to what sorts of events. Given such conclusions, events can be explained causally by determining which other events of the relevant kind also occurred, and we need no longer sort through all kinds of events to locate relevant factors. Again, consider the process of testing that I call "bootstrapping," which consists essentially of using some parts of a theory in a noncircular way to deduce from evidence instances of other hypotheses of the theory. The method is not domain specific, but given a domain of phenomena and a theory that is well tested with respect to itself by such phenomena, in any further theoretical elaborations we will naturally use those established hypotheses in obtaining instances or counterinstances to new conjectures. The method becomes domain specific.

The notions of philosophy of science are not psychology, and it may be that we learn not to be holist in ways that are quite different from those suggested by philosophical idealizations. Neither are the notions of philosophy of science computational, and they generally require a great deal of elaboration and restriction before they are fit to be part of android design. Fodor regards the frame problem in artificial intelligence as serious and as principally a problem about confirmational relevance. Tempted as he is by holism in epistemology, he is also tempted, I think, to believe that the frame problem is unsolvable. One should not be tempted by holism in epistemology, or, if tempted, one should not succumb. There is no problem in characterizing various strategies that will generate local methods from global, domain-independent methods. The hard part is to characterize them in a way that is computationally feasible. Hard, but for all we know as yet, not hopeless.

On Gall's reputation and some recent "new phrenology"

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Jerry Fodor's choice of Franz Joseph Gall as the hero of *Modularity* is both appropriate and (characteristically) clever. Fodor seems rather sad that his Gall isn't honored in textbooks, has had an unfairly rotten press, and was finished by his mistakes. I have two points to make: First, overall, Gall has not been all that neglected or maligned; second, those scientists continuing to labor in Gall's tradition have found considerable support for his original ideas.

Fodor cheers Gall's rejection of such faculties as imagination, reason, memory, and attention – "horizontal faculties" in Fodor's terms. Gall called them "indeterminate" and "general attributes" of the fundamental faculties. Gall thought the horizontal faculties were worthless in accounting for the differences among species, between humans and animals, and among individuals, and in determining the brain structures responsible for these differences. Instead, Gall proposed to find a set of fundamental faculties (Fodor calls them "vertical faculties") that were the products of specific cerebral organs and that could account for inter- and intraspecific differences in behavior. Each fundamental (vertical) faculty would partake of the attributes (horizontal). For example, the fundamental faculty, sense of music, would have the attributes of imagination, reason, memory, attention, and so on. The actual fundamental faculties were to emerge from the study of brain–behavior correlations.

So far, so good. Gall was always good at programs. Unfortunately, many of the fundamental faculties Gall came up with were not an obvious improvement over the attributes. True, among his faculties, or organs, he had three kinds of memory and such things as sense of numbers, sense of music, sense of mechanics, and sense of place and space. But he also had such items as wisdom, religion, and vanity. (Note that the only examples Fodor gives of Gall's faculties are those of music and numbers, among Galls's best.) The business soon got out of hand. Spurzheim (1934), Gall's erstwhile collaborator and publicist (and the source of the covers of Fodor's book), added eight to Gall's original twenty-seven, including eventuality and causality. By the end of the century there were over 100 phrenological faculties proposed; sometimes space on the cranium ran out and the face had to be used in addition (Clarke & Dewhurst 1972).

The idea of vertical faculties was indeed promising, but the problem then, as now, is that it's not so clear what the vertical faculties are. Moreover, in the rapid popularization of Gall's ideas the reasons for his rejection of the horizontal faculties and search for vertical ones was soon lost, and horizontal faculties crept back in. Finally (and Fodor doesn't stress this enough), Gall viewed himself as a student of comparative anatomy and physiology, not just of the human brain. He constantly cited evidence for his faculties from animal neuroanatomy and behavior and, indeed, he stressed that of his 27 faculties, 19 were common to humans and animals. Of course, Gall's scale of nature was a preevolutionary static one; but perhaps comparative arguments about brain and behavior had to wait for Darwin.

Indeed, after Darwin the credits started coming in. Herbert Spencer (1851) and George Henry Lewis (1867) praised Gall's new concepts of mental faculties and his rejection of the old ones. Ribot (1906), the first systematic student of amnesia, credited Gall with being the first to realize the multiple nature of memory, and Allport (1937), spent seven pages of his classic, *Personality: A psychological interpretation*, praising Gall's psychological achievements. Like Fodor, Allport found Gall's faculties the most important but least understood of his meritorious contributions. Finally, Young (1970) spoke of Gall's recognition as "the first modern empirical psychologist of character and personality" (p. 18). (These kudos come from looking up Gall in the indexes of several books lying around my office. Presumably a more systematic search would turn up more.)

Of course, Gall's place in history comes primarily from his influence on the study of brain function, not from faculty psychology. Since Gall's importance to the development of neuroscience doesn't seem to have filtered down to cognitive scientists, it may be worth spelling it out again.

Before his phrenology (not Gall's term – he preferred "physiology of the brain"), Gall had already achieved eminence as a cerebral neuroanatomist (Ackerknecht 1958). For example, he had distinguished cortical gray and white matter, differentiated projection, association, and commissural fibers, and established the pyramidal decussation. He viewed the brain as an elaborately wired machine for producing behavior, thought, and emotion and the cerebral cortex as a set of organs with different functions. These ideas departed substantially from prevailing notions about the brain. The Aristotelian stress on the unity of the mind, the attribution of emotions to the viscera, and the dismissal of the cortex as an unimportant rind were all still widely accepted beliefs. Of course, the belief in the hegemony of the brain had a long tradition beginning with Alcameon of Croton in the 6th century B.C., but as Flourens, Gall's arch rival put it, "It existed in science before Gall appeared – it may be said to reign there ever since his appearance" (quoted in Young 1970, p. 21).

Gall's program for the study of the brain was quite reasonable. It was based on several assumptions: (1) Intellectual abilities and personality traits develop differentially in each individual. (2) These abilities and traits reflect innate faculties that are localized in specific organs of the cerebral cortex. (3) The development or prominence of these faculties is a function of the activity, and therefore the size, of the cortical organs. (4) The size of each cortical organ is reflected in the prominence of the overlying skull, that is, in cranial bumps.

The primary method of data collection of Gall and his colleague Spurzheim was to examine the skulls of a wide variety of people, from lunatics and criminals to the eminent and accomplished. Correlations between brain structure and behavior in animals and between brain damage and mental dysfunction in humans were used to supplement their cranial examinations. They summarized their results in phrenological busts and charts.

Two errors transformed Gall and Spurzheim's reasonable goals into patent nonsense. The first was the assumption that the morphology of the skull was similar to that of the underlying brain. The second was their uncritical methodology, which relied almost entirely on seeking confirmatory anecdotes. For example, the organ of destructiveness was placed above the ear because a proturberance was found there in a medical student who was so fond of testing animals that he became a surgeon; the organ of amativeness was placed in the cerebellum because Gall had noticed that a passionate widow's neck was hot to his touch.

At least once, he got it right. Gall's correct localization of language in the lower part of the frontal lobe derived from an observation of a fellow medical student who had both a prodigious verbal memory and bulging eyes. The bulging eyes were supposed to reflect a well-developed frontal lobe. Gall supported this view with several case descriptions of aphasia after specific damage to the left frontal lobe. (These descriptions represent among the earliest detailed accounts of motor aphasia.)

In the scientific community at least, the supposed correlation between skull and brain morphology was soon recognized as erroneous. Gall's program continued, but now correlations were sought between brain (rather than skull) and behavior, not only through the study of brain damage but also with direct anatomical and physiological methods. Gall's ideas on the localization of mental functions became a guiding influence for much of 19th century neuroscience. Thus, Broca's demonstration in 1861 of an association between damage to the third frontal convolution and aphasia was viewed, at the time, as a direct confirmation of both Gall's specific localization of language and his more general belief in the localization of psychological function in the cerebral cortex. As Broca himself put it, Gall's "principle of cerebral localization . . . has been, one may say, the point of departure for all the discoveries of our century on the physiology of the brain" (quoted in Head 1926, 1:18).

In spite of its absurdities and excesses, phrenology facilitated the development of the study of the brain and behavior in several ways: by its belief in specific brain mechanisms underlying specific mental abilities and traits, by its emphasis on the importance of the cerebral cortex in mental activity, and by stimulating a surge of research on the psychological effects of brain damage in humans and of experimental lesions in animals. After Gall, less radical parcellations of brain function, such as those of Flourens, were much more readily acceptable. Cytoarchitectonic and functional maps of the cerebral cortex now ubiquitous in neuroanatomy, neurophysiology, and neuropsychology textbooks bear more than a coincidental resemblance to phrenological charts. They are the direct descendants of the ambitious, albeit heavily flawed, program of phrenology.

(If you think I'm overglorifying Gall see Young's (1970) monograph or Boring's (1950) text. Incidentally, Boring completely missed the novelty of Gall's vertical faculties and erroneously claimed that Gall obtained his list of faculties from Reid and Stewart of the Scottish school.)

Finally, a few notes on Fodor's argument. The best evidence against attributes and for vertical faculties with their cerebral organs or modules comes directly from the program inspired by Gall, namely, the study of the effect of brain lesions. Neither Sherrington nor Lashley were afraid to call this "the new phrenology." Since Fodor cites so few of these data, I'll give some examples. Whereas there is no adequate evidence for any brain lesion (or drug or electrical stimulation) affecting selectively and solely a single attribute or horizontal faculty such as memory, attention, perception, or will, there are many examples of brain lesions having a specific effect on, and only on, a Gallian or vertical faculty.

Take memory. No one has ever selectively destroyed memory and only memory. Even lesions in the hippocampal region, which have a devastating effect on many types of memories, leave memories of motor skills intact (Corkin 1968). Indeed, within this region hippocampal lesions primarily impair spatial memory, and amygdala lesions impair object memory (Murray & Mishkin 1982; Parkinson & Mishkin 1982). Both these memory deficits are supramodal. By contrast, inferior temporal lesions impair visual memories while leaving intact both memory in other modalities and visuosensory functions (Gross 1973). And so on. The evidence for specific perceptual deficits after localized lesions provides even more direct support for Gall's organs, for example, isolated deficits in the perception of color (Meadows 1974) and of movement (Zihl, Von Cramon & Mai 1983). Why does Fodor fail to mention any of this? After all he knows that we know that he knows about these and similar reports. Is he afraid of the odium theologicum of phrenology?

Cognitive self-organization and neural modularity

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Throughout his interesting and provocative essay, Fodor raises important issues and then draws conclusions about cognitive psychology and neuropsychology that are not supported by the literature. How and why this can happen in a work by a well-known thinker is important to understand, because it reflects upon the nature of the issues he is addressing and the methods he uses to analyse these issues.

The combination of important issues linked to questionable conclusions is illustrated by a statement from the last page of the essay: "the reason why there is no serious psychology of central cognitive processes is the same as the reason why there is no serious philosophy of scientific confirmation. Both exemplify the significance of global factors in the fixation of belief, and nobody begins to understand how such factors have their effects. In this respect, cognitive science hasn't even *started;* we are literally no farther advanced than we were in the darkest days of behaviorism" (p. 129). These strong claims were not made to be ignored. Their interesting points concern the linkage of cognitive processes to scientific confirmation and acknowledgment that "global" mechanisms need to be analyzed.

What Fodor has in mind with these claims is clarified by his discussion of a freely moving robot (pp. 113–119). He writes: "The robot must be able to identify, with reasonable accuracy, those of its previous beliefs whose truth values may be expected to alter as a result of its currect activities. . . . How, then, does the machine's program determine which beliefs the robot ought to reevaluate given that it has embarked upon some or other course of action? . . . fixation of belief really is quite like scientific confirmation." Fodor argues that no one has touched this problem because of its "isotropic" nature; that is, its computations are sensitive to the whole belief system.

Actually, there is a large and quantitative theoretical literature about this problem, including some articles in *Psychological Review* (Grossberg 1980, 1982a). The scientific results of this literature lead to a perspective rather different from the one Fodor has described. Within this theoretical literature, Fodor's question about "which beliefs the robot ought to reevaluate" is called the *stability-plasticity dilemma:* "The stability-plasticity dilemma concerns how internal representations can maintain themselves in a stable fashion against the erosive effects of behaviorally irrelevant environmental fluctuations yet can nonetheless adapt rapidly in response to environmental fluctuations that are crucial to survival. How does a network *as a whole* [italics mine] know the difference between behaviorally irrelevant and relevant events even though its individual cells, or nodes, do not possess this knowledge? How does a network transmute this knowledge into slow and fast rates of adaptation, respectively?" (Grossberg 1982a, p. 536). This quote illustrates the relevance of the stability-plasticity issue to Fodor's central thesis, and its italicized phrase indicates that "global" factors are an explicit part of the solution. The acknowledged relevance of the stability-plasticity problem to the question of scientific confirmation is illustrated by the following quotation: "The general problem of stabilizing adaptive codes in a fluctuating input environment requires that certain feedback relationships exist between the codes of individual events and the codes of various event combinations. Are such universal problems . . . one reason for the success of probability models? . . . Especially in cases in which a system continually reevaluates hypotheses based on disconfirming feedback does the present framework seem to be intrinsically richer than probability theory" (Grossberg 1982b, p. 633).

Why is Fodor totally unaware of this scientific literature? It is here that a profound difference in theoretical methods becomes relevant. Fodor's entire argument is based upon concepts that are easily framed using daily language. One of the most important conclusions of a stability-plasticity analysis is that daily language is fundamentally inadequate to derive and understand the design principles and mechanistic instantiations that regulate the stability-plasticity balance. Fodor's distinctions, which seem so plausible when stated in daily language, simply do not hold on the level of mechanistic deep structure.

For example, Fodor distinguishes "modules (which are, relatively, domain specific and encapsulated) and central processes (which are, relatively, domain neutral and isotropic/Quineian). We have suggested that the characteristic function of modular cognitive systems is input analysis and that the characteristic function of central processes is the fixation of belief" (p. 112). By contrast, in my theory, the *same* design principles and mechanisms are often used to analyze and predict data about both of these types of processes. Similar theoretical laws have been used to analyze and predict data about such ostensibly disparate domains as cognitive and perceptual self-organization (Grossberg 1984a), reinforcement, motivation, and attention (Grossberg 1984b), speech, language, and motor control (Grossberg 1984c), evoked potentials (Grossberg 1984a), circadian rhythms (Carpenter and Grossberg 1984), and brightness, color, and form perception (Grossberg 1983). This does not mean that specialized anatomies, or wiring diagrams, do not appear in these disparate applications, but that is not the type of distinction Fodor is making.

Fodor also emphasizes that "the distinction between *vertical* and *horizontal* [italics mine] modes of computational organization is taken to be coextensive with the functional distinction between systems of input analysis and systems that subserve the fixation of belief" (pp. 119–120). By contrast, vertical modes (bottom-up feature filtering, top-down templates or expectancies) and horizontal modes (cooperative-competitive interactions) occur within both the sensory and the cognitive processes of our theory, and both work together to define the network modules that are capable of solving the stability-plasticity dilemma. The spatial scale of these vertical and horizontal interactions can vary with the particular application, but that is not the type of distinction Fodor is making.

These general differences between the two approaches lead to many specific differences in the explanation of particular data. For example, after discussing how top-down templates may be used to match the phonetic content of an utterance, Fodor writes: "Apparently rather similar phenomena occur in the case of visual scotoma. . . . What happens is presumably that information about higher-level redundancies is fed back to 'fill in' the missing sensory information" (p. 65). Fodor goes on to criticize this conclusion because "the involvement of certain sorts of feedback in the operation of input systems would be incompatible with their modularity. . . . One or other of these doctrines will have to go" (p. 66). I agree with Fodor's criticism of the use of top-down templates to explain this sort of filling-in. However, I do so for reasons quite different from those of Fodor, since in my work, top-down feedback can operate without disturbing a system's "modularity." Moreover, although I do not use top-down feedback templates to explain filling-in, another type of boundary-completing cooperative feedback seems to be critically involved.

I explain this type of filling-in in terms of the interactions between a boundary completion process and a featural (e.g., color) filling-in process (Grossberg 1983, 1984c). These processes have enabled my colleagues and me to explain and simulate many paradoxical perceptual data that had not previously been explained in a unified way. Two predictions of our theory concerning the dynamics of boundary completion have, moreover, recently received experimental support from neurophysiological recordings that von der Heydt, Peterhans, and Baumgartner (1984) have made of cells in area 18 of the monkey visual cortex. Such predictive contributions, despite their incompleteness, do not warrant the conclusion that "cognitive science hasn't even *started*" (p. 129).

The design principles that have led us to such predictions in several areas of cognitive science and neuropsychology are nowhere in evidence in Fodor's stimulating essay. That is because Fodor's methods are simply not powerful enough to disclose these principles. I hope that people like Fodor who are searching for new approaches will avail themselves soon of these more powerful methods.

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Evidence for and against modularity

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Fodor's *Modularity* presents an argument for "faculty psychology." The historical antecedents of Fodor's approach are in neuropsychology and linguistics rather than psychometrics or experimental psychology. The latter fields have something to say to Fodor, and he has something to say to them. Faculty theories of individual differences have been stated explicitly, both in modern times (Gardner 1983) and in the past. Theories of attention have also been explicitly concerned with modularity. How well are Fodor's ideas supported in these data-rich fields?

Faculty theories of individual differences are based upon the commonplace observation that different people have different talents. Hence there must be different faculties of the mind. This proposal suffers from two faults. Labels are not explanations. Postulating a faculty for mathematics does not explain how a mathematician thinks. There is also a disturbing psychometric fact. Tests that differ in their overt demands on cognition (e.g., tests of verbal and spatial analogies) are positively correlated in most populations (McNemar 1964). This is why Spearman (1927) postulated a general intelligence (g) factor. A similar concept is featured in most of today's theories of intelligence.

Fodor's faculty psychology is logically superior to a psychology of definitions to the extent that faculties are derived from assumptions about the compulsory processing of various types of stimuli. To claim this advantage, it is necessary to provide some empirical way of defining "compulsory" (modular) processing, a point that will not be discussed here. A more empirical question will be asked. Do the derived faculties map onto facts about individual differences? A study by Marshalek, Lohman, and Snow (1983) will be used to show that a reasonably good mapping is possible.

High school and university students take a large number of tests. Some of these tests require the execution of what Fodor would apparently regard as compulsory, nonconscious actions, such as the detection of a letter in an array. Other tests require complex reasoning, as in the solving of verbal or visual analogies. Marshalek et al. constructed a space of mental tests by using the intertest correlations as measures of distance between tests and then applying multidimensional scaling techniques. Figure 1, Panel (a) is a schematization of their results. Tests that depended largely on elementary information processes fell on the periphery of the area, but the tests involving complex reasoning were centrally located.

Panel (b) of Figure 1 applies Fodor's concept of modularity to this data. Various input modules are assumed to conduct compulsory, data-driven analyses of auditory and visual linguistic data and of visual nonlinguistic stimuli. As Fodor points out, the nature of this processing may be quite complex. Furthermore, some of it is certainly learned. Because the input modules are autonomous, performance of simple tasks may vary independently across individuals. Given that the more complex ("conscious") reasoning processes require the same central resources, performance of complex tasks should be correlated across individuals.

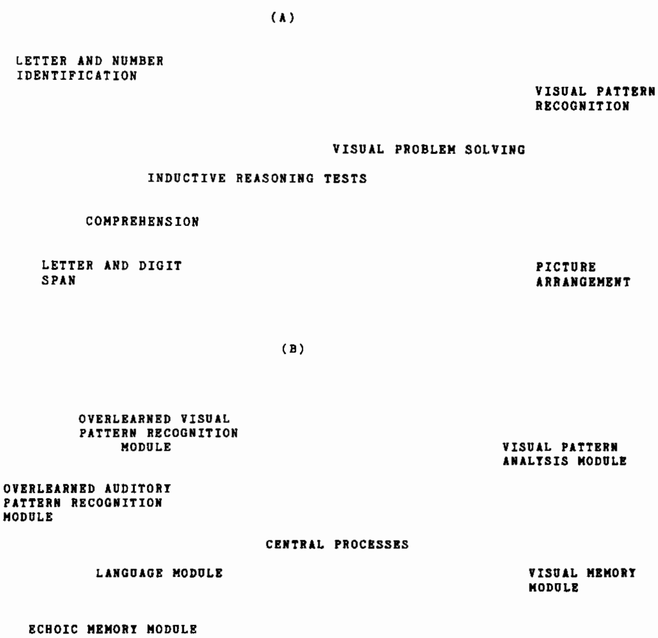

Figure 1 (Hunt). Panel (a) is a highly schematized summary of the results of Marschalek et al.'s (1983) study of problem solving in high school and college students. Panel (b) is a possible modular arrangement that would give rise to these results.

Individual differences in verbal comprehension provide a specific arena in which to examine the application of Fodor's ideas to data on intelligence. Verbal comprehension appears to be based on two quite different classes of analysis, which I have called "mechanical" and "controlled" processing (Hunt 1978, 1983). Lexical identification is a good example of an automatic process. The letter sequence DOG immediately arouses the associated concept of the word. Fodor would assign lexical identification to mechanisms inside the language input module. Understanding the deeper meaning of speech acts is a central process. Consider our understanding of Mark Anthony's lines: "I have come to bury Caesar, not to praise him."

Controlled and mechanistic processes provide somewhat different sources of individual differences in language understanding. Individual differences in the ability to recognize isolated words and word meanings are moderately correlated (*r*'s about .3) with global tests of verbal comprehension. Higher-order processes, such as the ability to extract meaning from sentences or the ability to relate the meanings of words to each other provide additional, partially independent predictors of overall verbal comprehension ability (Hunt, Davidson & Lansman 1981; Palmer, MacLeod, Hunt & Davidson 1984).

Priming effects provide an especially interesting example. It is well known that the recognition of a word can be speeded by presenting it in an appropriate context. An example is the rapid recognition of the word *nurse* in the sequence Doctor – Nurse. Similar effects can be shown for sentence contexts. These mechanistic context effects are almost entirely nomothetic. Individual differences in priming are small and not clearly related to individual differences in more general verbal comprehension tasks (Palmer et al. 1984; Stanovich 1980). Applying Fodor's analysis, contextual priming effects appear to arise from properties of the language input module. Although priming may be an important nomothetic phenomenon in word recognition, it seems to depend on mechanisms that do not vary greatly from person to person. This characteristic contrasts markedly to the ability to define an unfamiliar word by observing the context in which it occurs. The latter ability shows sharp individual differences and is a good marker of verbal comprehension ability (Sternberg & Powell 1983).

Fodor's approach deals reasonably well with some of the findings concerning individual differences in cognition. However, the match between theory and data is less clear when we consider studies of attention. Clearly, one can monitor auditory or visual signals, or both at once. The demands that monitoring tasks place on reasoning are usually trivial. According to Fodor's concept of autonomous processing, simple attentional studies in one modality should show effects that are relatively independent of effects in other modalities. This is not the case. There is a correlation of about .6 across individuals in the ability to execute auditory and visual attention demanding tasks. (Lansman, Poltrock & Hunt 1983). Furthermore, there are individual differences in the ability to coordinate the activity of simultaneously executing tasks that, in theory, should be conducted by different modules (Ackerman, Schneider & Wickens 1984). Why should autonomous modules need coordinating?

The data from studies of interference patterns due to the simultaneous execution of two tasks are even more damaging to Fodor's ideas than are the individual differences data. Extensive interference is almost always observed, even when the tasks seem to involve different modules. In particular, the speed of information processing in one task is almost always reduced when another task is present. Of course, there are exceptions. Not everything interferes with everything. For instance, balancing (a motor module?) interferes with visual memory but not verbal memory (Kerr, Condon & McDonald 1983). After a great deal of practice, some people can learn to perform processing on different, simultaneously presented input streams in an almost autonomous manner (Hirst et al. 1980). Nevertheless, there appear to be many cases in which tasks that would be assigned to different modules by any simple theory of modularity do interfere with each other.

Central processing activity can also be shown to influence the primitive elements of an input module. Consider a recent result in the study of perception (Wong & Weisstein 1983). Observers scanned the face-goblet reversible figure illusion to report the presence of a line. The line was always presented in the center of the visual field, so that it was in the figure or the ground region of the percept, depending upon the stage of the illusion. Sharp lines (high spatial frequency stimuli) were best detected when presented in the figure, but blurred lines (low spatial frequency stimuli) were best detected in the ground. Spatial frequency analysis, presumably a process buried deep in the visual input module, was influenced by a central perceptual process. How autonomous was the module?

Just before Fodor published through MIT, John Anderson (1983) lauded the indivisibility of the mind in a Harvard publication! War need not break out in the streets of Cambridge; both are right. There is undoubtedly some modularization in mental action. On the other hand, there may be many cases in which module *a* and module *b* are interconnected at one level and then are jointly connected to module *c* at a higher level. (Consider the example of how balancing combines with visual and verbal memory.) The problem is to find models both for intramodular functioning and intermodular coordination. Complicated models are less exciting than broad principles, but they may be more accurate pictures of how the mind works.

What constitutes a module?

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Fodor's rough approximation is that modular cognitive systems are domain specific, innately specified, hardwired, autonomous, and not assembled. Are these necessary properties of any cognitive system that one might want to call modular? In part, it is very difficult to say, because Fodor's discussion of modularity focuses almost entirely on a module devoted to language processing and only in passing on possible counterparts in visual perception. In limiting the discussion in this way, Fodor has raised certain problems for those who might seek to identify other modules. For, should one discover that a particular candidate module lacks one or more of these properties, it would seem necessary to reject the candidate as a possible module. However, there is another possibility: Because Fodor has focused on such a restricted domain of examples, he has misidentified some of the necessary properties of modular systems. The argument here is that, had he started with a different set of potential modules, say, including reading and music perception among others, he may have advanced a different set of criteria for modularity. To what extent, then, are the properties that Fodor has identified truly necessary ones for modularity?

Let us consider one plausible candidate for a modular cognitive system: fluent reading. In many respects, fluent reading would seem to fit the notion of a module. It is fast, automatic, highly organized, domain specific (at least to the same extent that spoken language perception is), and it seems to qualify as an input system to the central processor. Moreover, it appears to be informationally encapsulated to the same degree as spoken language processing. However, there seems to be a problem in applying the notions of innately specified and not assembled to the candidate fluent reading module. Unlike the case of spoken language perception, fluent reading is not something that all humans learn to do without explicit tutoring. Furthermore, it seems to be an activity that draws on skills from a variety of different areas, such as visual form perception and language comprehension. Thus, fluent reading would seem to qualify as a kind of module that is assembled out of parts from other modules. There is a weak sense in which one could hold that such a module is innately specified, namely, insofar as the parts used in assembling the module are components of other innately specified modules, one might want to claim that fluent reading is derived from innately specified parts. But this claim is different from saying that the module as a whole is innately specified. Notice, also, that even the weak claim comes at the cost of relaxing the criterion that modules are not assembled. Nor is fluent reading the sole candidate for such an assembled module. Other domains that take on functional significance for the organism and that require rapid decisions for inputs are likely to be similarly structured (e.g., see Shiffrin & Schneider 1977).

The claim that modules are hardwired is closely linked to their being innately specified and not assembled. Again, the choice of this property appears to be the result of the modules on which Fodor focuses. In particular, much of the evidence that he cites in support of this contention is related to localization of language functions in the brain. However, as one of us has argued elsewhere (Mehler, Morton & Jusczyk 1984) and as Fodor (1975) previously argued, there need not be any one-to-one mapping between psychological processes and physiological structures. In fact, if there are assembled modules (such as reading), it is hard to see how they could be hardwired from the start.

A different sort of problem is posed with respect to the claim for autonomous computation in the modules. In particular, it is difficult to see how one could ever provide an empirical test for this claim, given the sorts of qualifications that Fodor adds to his arguments. Initially, the claim for autonomous computation seems fully consistent with a rigidly vertical organization for cognitive modules similar to the one that Gall proposed. However, Fodor retreats from this position (p. 72) when he adopts a mixed model of horizontal and vertical components. He still argues that computation by any component that could appear in more than one module is autonomous in the sense that it is affected only by the operations within the module. But with the same operations allowed to appear in more than one module, there is no way of empirically distinguishing a system that is truly autonomous from one that employs certain general operations. Moreover, once the door is opened to the possibility of some horizontal organization, it is no longer autonomous in the sense which Gall intended. In fact, it is not clear what it means to say that computation is autonomous in this context.

Another consideration is the extent to which the modules are free from influence by the central processes – that is, the degree to which they are cognitively impenetrable. There are two senses in which the central processes could affect the modules. One of these is that knowledge available in the central processes could serve to modify or reorganize the functioning of a module in the long run. Fodor seems amenable to such a possibility (e.g., he allows that information in the lexicon, such as word associations, could be made available to the language module). However, the second type of influence from central processes, one occurring directly during on-line processing, is not acceptable to Fodor. In fact, he goes to considerable lengths to refute claims that such top-down effects from central processes occur during language comprehension. The problem here is again one of adopting a solution that renders the model safe from empirical disconfirmation: Note that according to Fodor "you need also to show that the locus of the top-down effect is *internal* to the input system" (pp. 73–74). However, later he also adds that "it is possible to imagine ways in which mechanisms *internal* to a module might contrive to . . . mimic effects of cognitive penetration" (p. 78). It is hard to see how one can empirically refute such a position. For example, by incorporating the lexicon in the language module, Fodor is free to cover any possible boosts in processing speed due to semantic relatedness.

Given the objections we have raised here, Fodor may be surprised to hear that we are still sympathetic to the notion that the most progress is apt to be made in cognitive psychology by pursuing the notion of a modular organization of input systems. We think that he is fundamentally right in the arguments he advances about domain specific, highly organized, and constrained input systems that are fast and manadatory. We do differ with him especially with respect to the assumption that the input systems are not assembled. Consequently, because we see this assumption tied closely to claims about hardwired and innately specified systems, we believe that these views need to be reexamined. Last, we do worry about the lack of restriction on the modules, which permits them to be so easily extended to include seemingly disconfirming data. Nevertheless, Fodor has aimed high in this book and given us all a lot to think about.

The mind as a Necker Cube

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Although Fodor's contrast between the specificity of systems that process information and the generality of those that transform and reflect upon it contains many new words, this opposition resembles closely the division between perception and thought that most Western scholars have imposed on the fluidity of cognitive processes. Fodor's originality lies in his strong claim that the modular input systems are tied to specific neural sites, whereas thought is equipotential and unencapsulated. This suggestion is not as comprehensive as Fodor implies, however, for processing systems occasionally generalize, and central processes can be remarkably specific.

Let us consider first the evidence for specificity in central processes. Most two-year-olds, faced with two familiar objects and one unfamiliar one, will infer that a spoken unfamiliar name must apply to the strange object. When an examiner asks the child to "Give me the *zoob*," the child will scan the three objects and give the examiner the unfamiliar toy (Kagan 1981). But this same child will not assume that an unfamiliar taste, sound, or haptic sensation has a name.

In a more complex example, eight-year-old Costa Rican children were taught, over a period of several months, a set of cognitive strategies, such as rehearsal, association, and counting, to help them recall information. When their recall memory performance was evaluated with a broad set of tests and materials, they failed to apply the useful rules to all of the new problems (Sellers 1979). Once again, the central strategies were activated in specific ways. Teachers of high school mathematics know that if a problem is changed from the length of the shadow a telephone pole casts on the ground to the distance a climber must ascend on a mountain, many sixteen-year-olds fail to apply to the second problem the rule that was successful with the first. Failure of generalization is the popular explanation, but I take this robust fact to mean that there is extreme specificity in central processes. My belief about the shape of the earth is limited to the domain of its application. When I drive my car to Cambridge, I believe the world is flat; when I think of the increasing daylight hours in May, I believe it is round.

Further, I suspect that the central systems that underlie beliefs are as dependent upon specific neural functions as are input processes. Inference, appreciation of right and wrong, and signs of self-consciousness emerge in all children after the middle of the second year, and it cannot be a coincidence that neuroanatomists note an asymptote in the velocity of growth of the brain and in the decrease in the density of neurons at this same age (Rabinowicz 1979). This correlation implies a specific neural base for these abstract, central processes.

The specificity in central processes is matched by generality and wholism in modular systems. Infants under one year can detect the similarity between a discontinuous sound and an interrupted line (Wagner et al. 1981) – two quite different perceptual inputs. Further, three-year-olds automatically treat a black, irregular, angular design as symbolic of the word *mad* and a pink, symmetric, curved design as symbolic of the word *happy*, without being able to say why this belief is so compelling (Demos 1974). These facts imply that the perception of a broken line or an angular design is not as encapsulated as Fodor suggests. We should further remember that all the sense modalities exaggerate slight changes at energy boundaries, and lateral inhibition appears to be a general characteristic of sensory systems.

Fodor's strict partition is vulnerable because he names the sources of the information when discussing modular systems and so builds in specificity, but he fails to name the targets of beliefs and inferences. Yet all beliefs are about something. The major strain in Fodor's argument is that he wants the few critical features he assigns to the terms *modular* and *central* to be their only qualities. However, both systems are more versatile than Fodor would like them to be. The fact that I call a bright red autumn maple leaf a beautiful object of nature does not prevent my neighbor from regarding the same leaf as potential mulch.

The basic hypothetical units in our theories of nature have turned out to possess unique, distinctive qualities, as well as to exhibit principles relevant to a set of diverse units. Sometimes it is theoretically useful to focus on the unique – the sequence of bases that defines a particular gene. At other times it is more fruitful to remember that the nucleus of the cell, like a tree, is simply a network of chemical molecules in which the healthy function of any unit depends on the viability of the whole – a biological form of Quineianism. It is best to remember Woodger's advice that "an understanding of the pitfalls to which a too naive use of language exposes us is as necessary as some understanding of the artifacts which accompany the use of microscopical techniques" (Woodger 1952, p. 6).

The modularity of behavior

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Dissecting a phenomenon into its parts adds structure to theory; we require that that be balanced by an increase in the complexity of the data that it covers. Do Fodor's modules, each with its own operating characteristics and parameters, resonate with separate parts of the data field, or has he jerrymandered a map whose boundaries are more beholden to the politics of extant theory than to the will of the data? Logical consistency and correspondence with the gross features of the data are all that we should call for at this early stage, and on these points he acquits himself. Although there will be disagreements about the importance of various exceptions to his thesis, such reevaluations are among the major mechanisms for the evolution of paradigms.

Aside from the issue of utility, that ultimate test of any theory, we may speculate on the social impact of Fodor's thesis. I suspect that it will give license to specialists to generate idiosyncratic theories of their phenomena, and no longer attempt to treat all faculties as exemplifications of the law of effect, or of neural networks, or of computer registers, or of holograms. However, since specialists have been generating their own schema all along, this activity should not increase the number of tongues at Babel. Like all licensing procedures (rituals designed to legitimate the inevitable, and sell that ticket to respectability for the price of residual control, weak though it may be), this one might entail some public benefit. With differences in domain recognized, we may treat the psychological endeavor as more of a team effort, with the expectation that the output of each team should fit into the matrix provided by those who study other modules – in computer lingo, that there should be a "handshake" in communication between the parts. This mutuality might involve the issue of how the inputs and outputs of each module must be formatted to be intelligible to each other, or it might concern the role of the central processing unit in allocating attention to the modules and sequencing them, or it might concern the search for homologies in structure or function among the modules. Not that such things haven't been done before, but they're usually done imperially, with one language and agenda imposed on all systems. Fodor's modularity notion should have the beneficial side effects of both increasing the respect for specialized accounts of different mental functions and enforcing the expectation that those accounts be related to treatments of other modules in a sensible way. Then again, it may perhaps merely further encapsulate specialists in their own fields, providing a philosophical apology for their cocoons.

Skinnerians often express the hope of understanding mind as covert behavior. I wonder whether Fodor's distinctions might be useful in the conception of behavior as overt mentation. Are Fodor's properties of mental modules exportable? His first property of input systems considered as modules is that they are "domain specific" (how could they be otherwise and be called "modules"?). Do we know of any domain-specific behaviors? The study of "constraints on learning" during the last decade makes some students of animal behavior doubt the existence of any *un*constrained behavior (see, e.g., Moore 1973)! As Weiss noted long ago (1941), "the strict constitutional limitations of learning ability . . . have certainly not received due emphasis. . . . since the total performance of an animal is an integrated act, involving shifting combinations of partial performances of elementary character, it remains to be demonstrated whether the modification concerns those elementary acts – the building blocks of behavior as it were – as such, or merely their combination into more complex acts on a higher level" (p. 244 in Gallistel 1980). [See also *BBS* multiple book review, *BBS* 4(4) 1981.] Gallistel summarizes the current view: At and below the level of amphibians, behavior is modified only by selective facilitation or inhibition of existing motor patterns. Rats are somewhat more flexible, and there is a "tremendous efflorescence of the ability to 'invent' coordination patterns on the way from rat to man" (1980, p. 271). But even in such mammals, the Brelands have noted a tendency for motor patterns "invented" by a trainer to drift toward more innate forms, a tendency that they labeled "instinctive drift" (Breland & Breland 1966). Shettleworth (e.g., 1980) has extensively investigated the interaction between reinforcement and the organization of action patterns in hamsters, and found those modules to be differently affected by different rewards.

The second principle, "mandatory operation," also maps well onto behavior. Hearst and Jenkins (1974) reported a version of the autoshaping paradigm in which pigeons were given a signal for food that would be briefly available at the other end of a long chamber. Time and again the pigeons pecked at the signal and dashed to the food aperture just in time to see the hopper receding from their reach. This performance is evocative of some human nightmares, but has the added irony that pecks at the signal were never logically necessary. Apparently they were physiologically mandatory, however, for in such situations animals may respond on over 80% of the trials, and they never learn to respond to the signal by moving toward the food hopper. Other experiments have shown that animals will repeatedly respond for conditioned reinforcers in the signaled absence of primary reinforcers. Wickens reports an experiment in which humans were conditioned to withdraw their finger to avoid an electrode capable of delivering a mild electric shock. The subjects could not inhibit the conditioned response when instructed to do so. In fact, "one subject lost a small wager by failing to inhibit his response after betting the experimenter that he could" (in Gallistel 1980, p. 112).

The third principle is that the "details are not accessible to the top." We see this in output systems when we find it impossible to begin a piece of music in the middle. The fourth principle is that such modules are "fast." Such speed for skilled motor performance was noted by Lashley in his famous discussion of "the problem of serial order in behavior" (1951). The fifth principle is "informational encapsulation": "True" knowledge (input from other parts of the system) doesn't effect the module's operation. One sees this in the derailment of action patterns when all evidence suggests the inappropriateness of the response, in the cases of displacement activities and adjunctive behavior, and most insidiously in the case of brood parasitism. Human fetishes and phobias stand adamant to reason.

Other principles are "fixed neural architecture" (see Sperry 1945 for elegant experiments on the neural architecture of muscular action and Valenstein 1973 for a review of the elicitation of fixed action patterns by electrical stimulation of the brain); "breakdown patterns" (see Staddon, 1983, for a discussion of failure modes in behavior, and Norman, 1981, for a categorization of action slips); "the ontogeny of pace and sequencing" (see the burgeoning field of developmental neurobiology).

In the end, is Fodor's taxonomy worthwhile? A number of principles seem to overlap (for instance, mandatory operation seems but an extreme implication of encapsulation). Conversely, there is little discussion of the interaction among modules, the rules for negotiating resolution of outputs to provide a coherent picture of the external world. But this he admits: "central cognitive processes . . . exemplify the significance of global factors in the fixation of belief, and nobody begins to understand how such factors have their effects" (p. 129). Another modularity theorist, B. F. Skinner, promised a specification of the reflex that would "cut nature at her joints." But he must have buried the dulled knives and butchered limbs, for we are left little evidence for a search for an optimal specification and no data exemplifying "an increase in orderliness as the preparation is progressively restricted" (Skinner 1935). In contrast, here we see Fodor seriously concerned about how to make the cuts; he'll get a lot of help in the kitchen, for sure. Lets hope that we are left with more than hamburger.

Parallel processing explains modular informational encapsulation

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Fodor makes a defensible case for modularity of input systems, including speech perception. The rest of language, which is most of it, he barely addresses. He presumably consigns it to that cognitive wasteland that he despairs of in the closing section of his book. I shall raise some issues with respect to the input modules.

In advocating modularity as a property of domain-specific input systems, Fodor states that "the key to modularity is informational encapsulation" (p. 98) and that "only such representations as constitute the *final* consequences of input processing are fully and freely available" (p. 56), "earlier ones being discarded as soon as subsystems of the input analyzer get the goodness out of them" (p. 58). But, given this serial "stage" model of information flow, what barrier in the brain ensures "the relative inaccessibility of intermediate levels of input analysis" (p. 57)? One can of course resort to that workhorse of neuropsychological conjecture, the private connection. But a more economical way of explaining why the intermediate representations are inaccessible is that they do not exist. A parallel model of input analysis dispenses with levels of representation, as well as the "hardwired" constraints on information flow (p. 127) that allegedly connect them.

Stage theory is so familiar as to seem self-evident. Thus, discussing letter categorization, Fodor states: "At a minimum, *some* shape information must be registered prior to alphabetic value, since alphabetic value depends on shape" (p. 58). But must it? Only if there resides an interpreter in the brain, for whose convenience a representation has to be displayed. Posner (1978) has offered evidence for the parallel processing of letters for shape and for name. Input arriving at cortex is both registered as pattern and categorized, in parallel, by different subsystems of the input analyzer. Indeed, pattern perception and interpretation are known not only to be capable of independent variation but also to be doubly dissociable. Not only can patterns be perceived without understanding (as envisaged by stage theory), but the reverse can also obtain: correct categorization of a pattern incorrectly read (in "deep dyslexia"; Coltheart, Patterson & Marshall 1980), or even of a pattern not represented in awareness at all (Allport 1977; Marcel 1974). Emotional evaluation can also precede pattern perception (Zajonc 1980). Such findings suggest parallel processing.

In addition, the parallel model parsimoniously accounts for informational encapsulation, because there is no intermediate stopping point, and thus the opportunity for extramodular influence does not even arise. The final product is the only product. What in a serial model is an interlevel is in the parallel model a subset of component outcomes experienced in isolation.

Parallel models not only dispense with the conceptual clutter and paraphernalia of stage theory but also dispense with the traditionally hypothesized pathways connecting level to level. Fodor's concept of neural architecture does not reach beyond hardwired connections, and in their absence he despairs of further neuropsychological insights: "in the case of central processes, you get an approximation to universal connectivity, hence no stable neural architecture . . ." (p. 119). This, it appears, is "bad news for cognitive science" (p. 128). Actually, one can easily contemplate a stable and researchable central communication system without cables – radio rather than telephone, the constraint being the range of messages a given receiver can decode. Each central receiver is selectively tuned for messages crossing cerebral space (just as input mechanics are tuned for selective attention to external events). Be that as it may, evidence no longer sustains the notion of one-way traffic converging from point-to-point representation at a "first cortical relay" onto some polymodal integrative locus. Instead, each modality is now known to possess many point-to-point representations, distributed over what used to be regarded as "association cortex" (Merzenich & Kaas 1980; Zeki 1978). These are connected, not unidirectionally, but reciprocally. This organization fits the view that diverse stimulus attributes are analyzed in parallel (Cowey 1979). The final product would be a pattern of neuronal firing distributed across the multiple loci that house the module's repertoire of component processes (rather as envisaged by Luria 1966).

In summary, Fodor's concept of the module's informational encapsulation gains credibility in the context of a parallel processing model of input analysis.

Combe's crucible and the music of the modules

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In his *Second Dialogue between a Philosopher of the Old School and a Phrenologist*, George Combe (1824) has the traditional philosopher declare that:

The mind, so far as consciousness is concerned, is single, and the phrenological faculties are distinguished from one another only by the kinds of external objects with which they are conversant; your faculty of locality, for example, is only the mind attending to relative position; and your faculty of colouring is the mind attending to the rays of light. (p. 205)

Now, as Fodor (1983) points out, from this kind of domain specificity "nothing useful follows": "the psychological mechanisms that mediate the perception of cows are ipso facto domain specific *qua mechanisms of cow perception*" (p. 48).

Combe's phrenologist then elaborates an ingenious metaphor for the philosopher's common-sense theory of mind: "The mind, considered as a general power existing in different states, may be likened to a wind-instrument with only one form of apparatus for emitting sound, – a trumpet [he means a natural horn: JCM] for example" (p. 206). Combe resolutely pursues the metaphor:

If [the trumpet] is excited with one degree of force [i.e., one type of external stimulation: JCM] it emits one kind of note, which is the result of the metal being in a certain state. If excited with another degree of force, it emits another kind of note, and this is the consequence of the metal being in another state. The number of notes that may be produced will be as great as the variety of states into which the metal may be excited by every possible impulse of wind. (p. 206)

The phrenologist defers criticism of this analogy in order to introduce a musical variation upon it:

I would rather liken the mind to another musical instrument – a piano-forte, having various strings. The first string is excited, and a certain note is produced; the second is excited, and another note swells upon the ear. Each note, it is true, results from the instrument being in a particular state, but it cannot exist in the state which produced the first note without the first string; nor in the state which produced the second note without the second string; and so forth." (pp. 206–207)

The distinction is now clear: "The trumpet represents the mind as conceived by the metaphysicians; the piano-forte shadows it forth as apprehended by the phrenologists" (p. 207).

The philosopher concedes that he can "conceive the distinction" but calls the phrenological conception of mind "a mere gratuitous hypothesis," whereas the traditional model is, he claims, "supported by the evidence of consciousness" (p. 207). This appeal to the unity of conscious experience is characteristic of later attacks upon phrenological fractionation. When Flourens (1846) wrote his critique of Gall's neuropsychology, he quoted Descartes in support of the unity of the self-conscious mind:

I remark here . . . that there is a great difference between the mind and the body, in that the body is, by its nature, always divisible, and the mind wholly indivisible. For, in fact, when I contemplate it – that is, when I contemplate my own self – and consider myself as a thing that thinks, I cannot discover in myself any parts, but I clearly know that I am a thing absolutely one and complete. (Young 1970, p. 72)

Popper and Eccles (1977) echo this refrain in their interpretation of the mental status of the West Coast commissurotomy patients of Vogel and Bogen: "The unity of self-consciousness or the mental singleness that the patient experienced before the operation is retained, but at the expense of unconsciousness of all the happenings in the minor, right, hemisphere" (p. 311).

The phrenologist, however, remains singularly unimpressed by the evidence of consciousness. The issue is not whether conscious experience reports a unified percept of, say, a red chair at a particular spatial locus, but rather whether color, form, and locality are computed by distinct organs prior to being assembled into a "whole" by the central process of "focused attention" (Fodor 1983, p. 133). In brief, the phrenological hypothesis is that "*input systems are modules*" (Fodor 1983, p. 46).

Combe's phrenologist observes that "the mind is conscious of existing in various states but it has no consciousness of the instruments by means of which it enters into them" (p. 208). The argument from conscious experience is thus irrelevant to phrenological theory, a point that Combe again provides with a musical accompaniment: "Suppose that you had never seen either a trumpet or piano-forte, nor heard them described, – that they were played in your presence behind a screen, and you were required, from the mere notes emitted by each, to form a theory of its mechanism, could you be sanguine in your hopes of success in the attempt?" (p. 207). This early version of the Turing Test (which I propose to rename the Combe Crucible) provided much of the methodological impetus for the dramatic case reports of domain-specific impairment of cognitive function consequent upon local brain damage that fill the pages of the phrenological journals. The philosopher, having conceded that he would indeed achieve little success in the Combe Crucible, accepts that if one "were permitted to approach the piano-forte, and to try experimentally what notes could be produced from it by striking its various strings" (p. 208), one might "understand the theory of the production of its notes better." The rediscovery by Alan Turing of Combe's crucible serves, of course, to confirm the validity of Jorge Luis Borges's (1966) acute reflection that "universal history is the history of the diverse intonation of a few metaphors."

I shall not expand here upon my conviction that practically all the neurophysiological and neuropsychological data I am familiar with are more appropriately interpreted on the piano-forte than on the trumpet (Marshall 1984). But I cannot repress the urge to close with a mixed metaphor.

When Nicolò Paganini (1782–1840) played in London, the phrenological community flocked to his concerts in the hope of confirming that his excessively well-developed organ of music was indeed reflected in a large bump over the Sylvian fissure (Marshall 1980, p. 121). Paganini looked like the devil but played like an angel, a fact that, irrespective of whatever cortical correlates may be implicated, we are now inclined to attribute to Marfan's syndrome – an inherited anomaly of connective tissue.

Be that as it may, it is well-known that Paganini was wont to conceal a razor blade at the tip of his bow. During one of his more exhuberent caprices, Paganini would calmly proceed to saw progressively through each of the violin's strings until only one remained, the whilst continuing the piece without a beat or a note missed. The phrenologists were delighted: Before their very eyes a piano-forte had been transformed into a trumpet, but to the conscious ear, the unity of the musical experience seemed seamless.

Verticality unparalleled

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Having long found reason to believe that speech is special, we have, naturally enough, been surprised at the firmness with which others have asserted the contrary – that speech is just like everything else, or, what comes to the same thing, that everything else is special, too. Apparently, our claim has run counter to some deeply held conviction about the nature of mind. One of Fodor's achievements is that he makes this conviction explicit. On the orthodox view, as Fodor sees it, mental activities are "horizontally" organized; arguments for the specialness of speech and language fit better with the assumption that they are vertical. Of the many observations provoked by Fodor's lucid analysis of these opposing views, we can here offer only two. The first has to do with the relations among vertically organized input systems; the second, with the relations between input systems and output systems.

Fodor's input systems, being "domain specific" (p. 47), are in parallel, and their outputs complement each other. Thus, when two modules are sensitive to the same aspects of a signal, representations from both modules should be cognitively registered. This assumption is surely plausible for those modules, such as for shape and color, that compute complementary representations of the same distal object. But the situation is different for speech. There, the linguistic module appears to take precedence over the module (or modules) that look after distal objects that are not linguistic. Given the same aspect of the signal, the linguistic and the nonlinguistic module are able to compute representations of different distal objects, but if a linguistic representation is computed, the nonlinguistic representation is not cognitively registered. Consider an example to which Fodor himself alludes (p. 49): the transition of the third formant during the release of a consonantal constriction in a consonant-vowel syllable. When artificially isolated from the rest of the signal, this transition is perceived nonlinguistically as a chirp or glissando (Mann & Liberman 1983; Repp, Milburn & Ashkenas 1983). But in its normal acoustic context, the same transition is not so heard. It simply contributes to the perception of a distal object that is distinctly linguistic: the place of articulation of the consonant.

Fodor's account of these facts would be that the isolated transition is ignored by the linguistic module but not by the nonlinguistic module, which registers it cognitively as a chirp. His account would also exclude the possibility that, for the transition in context, the linguistic module would register a chirp as well as a consonant. For the linguistic module, such a representation would be at most "intermediate" (pp. 55ff.) and hence inaccessible to central cognitive processes. (We ourselves doubt that the linguistic module computes any such representation *at all;* we prefer to believe that the earliest representation is an articulatory one.) But the simple parallel arrangement of the modules that Fodor assumes does cause trouble, for it means that "the computational systems that come into play in the perceptual analysis of speech . . . operate *only* upon acoustic signals that are taken to be utterances" (p. 49), but it does not preclude the possibility that other systems will operate on these same signals. It suggests that the transition in context will be registered not only phonetically, by the linguistic module, but also nonphonetically, by the nonlinguistic module. The listener would therefore hear both consonant and chirp. More generally, and more distressingly, the listener would hear all speech signals both as speech and as nonspeech.

What seems called for is a mechanism that would guarantee the precedence of speech but would not constitute a serious weakening of the modularity hypothesis. This precedence mechanism would ensure that, though both the linguistic and the nonlinguistic modules may be active (since speech and nonspeech may occur simultaneously in the world), a signal will be heard as speech if possible and otherwise as nonspeech, but not as both. It is rather compelling evidence for the existence of such a mechanism that it can be defeated under experimental conditions that evade ecological constraints. This is what occurs in the phenomenon known as "duplex perception" (Liberman, Isenberg & Rakerd 1981; Mann & Liberman 1983; Rand 1974). As we have noted, if a third-formant transition that unambiguously fixes the perception of a consonant-vowel syllable (for example, either as /da/ or as /ga/) is extracted and presented in isolation, it sounds like a nonspeech chirp. The remainder of the acoustic pattern, presented in isolation, is perceived as a consonant-vowel syllable, but in the absence of the transition, the place of the consonant is ambiguous. When the transition and the remainder are presented dichotically, a duplex percept results: The chirp is heard at the ear to which the transition is presented, and an unambiguous consonant (/da/ or /ga/, depending on the transition) is heard at the other ear; the ambiguous remainder is not heard (Repp et al. 1983). Thus, the transition is perceived, simultaneously, as a nonspeech chirp and as critical support for the consonant. Apparently, the precedence mechanism recognizes that the transition and the remainder belong together, but it is also aware that there are two signal sources, one at each ear, and that only one of them is speech. It therefore allows both the linguistic module and the nonlinguistic module to register central representations that depend on the formant transition.

How might this precedence mechanism work? An obvious possibility is that it scans the acoustic input and sorts speech signals from nonspeech signals, routing each to its appropriate module. But such a sorting mechanism would seriously compromise the modularity view, because, having to cut across linguistic and nonlinguistic domains, it would be blatantly horizontal. Fortunately for the vertical view, the horizontal compromise appears to be wrong on empirical grounds.

The point is that a sorting mechanism would require that there be surface properties of speech that it could exploit. These properties would be characteristic of speech signals in general, but not of nonspeech signals. Moreover, they would be distinct from those deeper properties that the linguistic module uses to determine phonetic structure. It is of considerable interest, then, that while natural speech signals do have certain surface properties (waveform periodicity, characteristic spectral structure, syllabic rhythm) that such a mechanism might be supposed to exploit (and that manmade devices for speech detection *do* exploit), none of these properties is essential for a signal to be perceived as speech. Natural speech remains speechlike, and even more or less intelligible, under many forms of distortion that destroy these properties (high- and low-pass filtering, infinite peak clipping, rate adjustment). And, more tellingly, quite bizarre methods of synthesis – for example, replacing the formants of a natural utterance by sine waves with the same trajectories (Remez, Rubin, Pisoni & Carrell 1981) – suffice to produce speechlike signals. Thus, speech appears to be speech, not because of any surface properties that mark it as such, but entirely by virtue of properties that are deeply linguistic. A signal is speech if, and only if, the language module can in some degree interpret the signal as the result of phonetically significant vocal-tract gestures. (In the same way, there are no surface properties that distinguish grammatical sentences from ungrammatical ones: a sentence is grammatical if, and only if, a grammatical derivation can be given for it.) We therefore reject this horizontal compromise, and consider two other possible precedence mechanisms, both thoroughly vertical.

The first is an inhibitory precedence mechanism that works across the outputs of the modules in this way: If the linguistic module fails to find phonetic structure, then the output of the nonlinguistic module is fully registered; if, on the other hand, the linguistic module does find phonetic structure, the link to the nonlinguistic module causes the "corresponding" parts of its output to be inhibited but leaves the phonetically irrelevant parts unaffected. Such a mechanism is certainly conceivable and, being a central mechanism, would not compromise modularity. It would, however, be most unparsimonious. For if the inhibitor mechanism were to know which aspect of the output of the nonlinguistic module corresponded to aspects of the signal that were treated as speech by the linguistic module, it would have to know everything that the two modules know: the relationships between phonetic structure and speech signals, as well as the relationship between nonlinguistic objects and nonspeech signals. Thus a central mechanism would, in effect, duplicate mechanisms of two of the modules.

Turning, therefore, to the second possible precedence mechanism, we propose that, while the outputs that the modules provide to central processes are in parallel, their inputs may be in series. That is, one module may filter or otherwise transform the input signal to another module. We suppose that the linguistic module not only tracks the changing configuration of the vocal tract, recovering phonetic structure, but also filters out whatever in the signal is due to this configuration, including, of course, formant transitions. What remains – nonlinguistic aspects of speech such as voice quality, loudness, and pitch, as well as unrelated acoustic signals – is passed on to the nonlinguistic module. This supposition is parsimonious in that it in no way complicates the computations we must attribute to the linguistic module; the information needed to perform the filtering is the same information that is needed to specify the phonetic structure of utterances (and ultimately the rest of their linguistic structure) to central processes.

A further point in favor of this serial precedence mechanism is that something similar appears to be required to explain the operation of other obvious candidates for modularity, such as auditory localization, echo suppression, and binocular vision. Consider just the first of these. The auditory localization module cannot simply be in parallel with other modules that operate on acoustic signals. Not only do we perceive sound sources (whether speech or nonspeech) as localized (with the help of the auditory localization module), but we also *fail* to perceive unsynchronized left- and right-ear images (with other modules). Obviously, the auditory localization module does not merely provide information about sound-source locations to central cognitive processes; it also provides subsequent modules in the series, including the linguistic module, with a set of signals arrayed according to the location of their sources in the auditory field. The information needed to create this array (the difference in time-of-arrival of the various signals at the two ears) is identical to the information needed for localization.

Unfortunately, hypothesizing a serial precedence mechanism does not lead us directly to a full understanding of duplex perception. Until we have carried out some more experiments, we can only suggest that this phenomenon may have something to do with the fact that the linguistic module must not only separate speech from nonspeech, but it must also separate the speech of one speaker from that of another. For the latter purpose, it cannot rely merely on the differences in location of sound sources in the auditory field, since two speakers may occupy the same location; it must necessarily exploit the phonetic coherence within the signal from each speaker and the lack of such coherence between signals from different speakers. It might, in fact, analyze the phonetic information in its input array into one or more coherent patterns without relying on location at all, for under normal ecological conditions, there is no likelihood of coherence across locations. Thus, when a signal that is not in itself speech (the transition) nevertheless coheres phonetically with speech signals from a different location (the remainder of the consonant-vowel syllable), the module is somehow beguiled into using the same information twice, and duplex perception results.

Our second general observation about Fodor's essay is prompted by the fact that language is both an input system and an output system. Fodor devotes most of his attention to input systems and makes only passing mention (p. 42) of such output systems as those that may be supposed to regulate locomotion and manual gestures. He thus has no occasion to reflect on the fact that language is both perceptual and motor. Of course, other modular systems are also in some sense both perceptual and motor, and superficially comparable, therefore, to language: simple reflexes, for example, or the system that automatically adjusts the posture of a diving gannet in accordance with optical information specifying the distance from the surface of the water (Lee & Reddish 1981). But such systems must obviously have separate components for detecting stimuli and initiating responses. It would make no great difference, indeed, if we chose to regard a reflex as an input system hardwired to an output system rather than as a single "input-output" system. What makes language (and perhaps some other animal communication systems also) of special interest is that, while the system has both input and output functions, we would not wish to suppose that there were two language modules, or even that there were separate input and output components within a single module. Assuming nature to have been a good communications engineer, we must rather suppose that there is but one module, within which corresponding input and output operations (parsing and sentence-planning; speech perception and speech production) rely on the same grammar, are computationally similar, and are executed by the same components. Computing logical form, given articulatory movements, and computing articulatory movements, given logical form, must somehow be the same process.

If this is the case, it places a strong constraint on our hypotheses about the nature of these internal operations. All plausible accounts of language input are by no means equally plausible, or even coherent, as an account of language output. The right kind of model would resemble an electrical circuit, for which the same system equation holds no matter where in the circuit we choose to measure "input" and "output" currents.

If the same module can serve both as part of an input system and part of an output system, the difference being merely a matter of transducers, then the distinction between perceptual faculties and motor faculties (the one fence Fodor hasn't knocked down) is perhaps no more fundamental than other "horizontal" distinctions. The fact that a particular module is perceptual, or motor, or both, is purely "syncategorematic" (p. 15). If so, then the mind is more vertical than even Fodor thinks it is.

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NOTE

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Too little and latent

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Modularity of various sorts is in the Boston air. Chomsky (1980), Gardner (1983), and Fodor are all pushing for computational isolationism. This move is in line with current thinking in cognitive psychology, though Gardner includes in his "faculties" a lot of what other people would attribute to central processes. While approving in general of Fodor's treatment of input modules, I feel a sense of sadness that he did not put into perspective, within the information-processing framework, the work of the last 15 years or so by researchers like Newcombe and Marshall (1981), Morton and Patterson (1980), Seymour (1979), and Shallice (1981), to take just the U.K. side of this movement. This body of work has gone some way in establishing modular principles of operation of the input and output processes concerned with language on the basis of a variety of data from experimental psychology and neuropsychology. Although the resulting units do not have the formal precision of definition of Fodor's modules, it might have been useful to have an appraisal of their properties within Fodor's analytic framework. There is, in addition, a big debate involving a number of approaches in which the distinctions between processes are blurred. This would be true of schema-based theories (such as Rumelhart 1980) and of the views of psychologists like Jacoby who recently concluded that "perception relies on the retrieval of memory of whole episodes rather than on an abstract of representation such as a logogen" (Jacoby 1983, p. 37). A discussion of the philosophical limitations of such work would make interesting reading.

With respect to the central processes, however, I find myself in profound disagreement with Fodor, concerning both their nature and our ability to study and describe them. Fodor maintains a belief in the integrity of his own belief system that I cannot attribute to my own: "every process has more or less uninhibited access to all the available data" (p. 127). If Fodor doesn't have to live with the selective memory, the contradictory beliefs, or the irrationality that beset the rest of us, he could at least observe it in those around him or see it amply documented in the psychological literature. I would agree that our belief system is isotropic (though it is far from clear to me that all of science is; see Mehler, Morton & Jusczyk 1984). Thus, having read *Modularity* (as opposed to how one felt after reading *Modularity*) could conceivably affect what one chose to eat for lunch afterward. Also, it seems reasonably clear that science is Quineian (and, on that principle, it is possible to see why Fodor claims it must be isotropic), but it is equally clear that our beliefs are, in practice, not. There seems to be no reason to suppose that any particular fixation of belief involves consulting (actively or passively) all those preexisting parts of the belief system that are directly relevant (let alone the indirectly relevant ones), any more than Fodor's considerations of input modules involved consulting the relevant information processing literature. It may rarely be possible to tell in advance which particular parts of our belief system will or will not be consulted, but even this process is not completely mysterious. Thus, Bekerian and Bowers (1983) have shown how the conditions of retrieval influence which of two contradictory beliefs is accessed. What Fodor does is to shift from the heady world of conceivability to statements about inevitability, and we end up with a central, equipotential neural net with no room for psychology ("bad candidates for scientific study"; p. 127). The neural net is, of course, the only device by which one could have even "more or less" uninhibited passive access to the available data.

Fodor cites one review of the problem-solving literature, but only to dismiss it: "In such cases, it is possible to show how potentially relevant considerations are often systematically ignored, or distorted, or misconstrued in favour of relatively local (and, of course, highly fallible) problem-solving strategies" (pp. 115–116). "A bundle of such heuristics" (p. 116), "embarrassingly like a Sears catalogue" (p. 127), could do the job, but because there are "no serious proposals about what heuristics might belong to such a bundle, it seems hardly worth arguing the point" (p. 116). One might pause to wonder why Johnson-Laird (1980, 1983) or the movement represented in Kahneman, Slovic, and Tversky (1982) should not be considered serious, but the question is academic. It seems hardly worth arguing the point because one would be up against an entire belief system, including the virtues of "neurological plausibility" (117). Fodor seems to have concluded that only a subset of psychological theories of the central processes are relevant (which cuts down the required reading somewhat). This subset confuses "computationally global" with nonmodular (as Fodor seems to do), has individual beliefs and knowledge fragments interconnected in a massive transcortical network, and is very wise as well as being real and true. This characterization goes a little beyond the text but needs to be pointed out because, for once, Fodor doesn't put his mouth where his money is.

Quinity, isotropy, and Wagnerian rapture

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Fodor hopes to be remembered for his "'First Law of the Nonexistence of Cognitive Science' . . . the more global (e.g., the more isotropic) a cognitive process is, the less anybody understands it" (p. 107). I'd rather remember him as providing (in Fodor 1975, 1981) the only proposal that begins to make any cognitive process intelligible – be it global, modular, or otherwise. It's certainly ironic that some of the very nondemonstrative inferences for which Fodor argued that the language of thought was needed (Fodor 1975, ch. 2) are ones that he now thinks cannot be computed in it. This reversal seems needlessly perverse, a consequence, I fear, of too much rapture for modularity.

Fodor's argument for his law runs thus: central processes are Quineian and isotropic on the model of confirmation in science. Confirmation tends to be defined over beliefs as a whole and to be open to the relevance of any one of them. Now, "the condition for successful science (in physics, by the way, as well as psychology) is that nature should have joints to carve it at: relatively simple subsystems which can be artificially isolated and which behave, in isolation, in something like the way that they behave *in situ*. Modules satisfy this condition: Quineian/isotropic-wholistic-systems by definition do not" (p. 128). Therefore, no successful science of central processes can exist. "The limits of modularity are also likely to be the limits of what we are going to be able to understand about the mind . . ." (p. 126).

What's surprising about this argument is that it flies in the face of the many Quineian and isotropic systems around us that we do seem to understand. For a timely example, consider the American election system. There are elections at regular intervals in which, in principle, anyone can run (so the system is isotropic); and the results of the election are based upon properties of the entire electorate, for example, majorities (so the system is Quineian). Or consider a telephone system, where any phone can call any other (isotrophy), and where calls are completed depending upon the load and distribution of calls in the system as a whole (Quineity). One might even wonder whether physics, with its several universal force fields, doesn't itself postulate a world as Quineian and isotropic as one might find. One can certainly imagine a cognitive system organized so that any of its beliefs may, as a result of input, be called up randomly for revision; which ones are revised will depend, for example, on how much memory space the entire result consumes.

Surely we can *understand* all these systems perfectly well. The "joints" may not be as physically localizable as in modular systems. But that, to a functionalist (Fodor 1965), should come as no surprise: one expects joints in computational systems (e.g., search procedures, computations of load, however global) to be abstract. The extent to which such joints can be artifically isolated depends by and large on the igenuity – and funding – of the scientist. Quineity and isotropy, by themselves, provide no reason whatever for despair about a science of central processes.

What does raise a problem is not that central processes are Quineian and isotropic but rather that they are not *merely* that. The trouble with the simple cognitive system just mentioned, for example, is not its wholism but simply its stupidity. We know that that system, or a telephone switchboard, would be even stupider than we are, just as we know that what's wrong with associationist models, from Hume ("the ultimate in nonmodular theories of mind," p. 123) through Skinner, is not that we can't understand them – they are all only too intelligible! – but rather that they simply can't do what we can. What seems to be the case (as Fodor himself sketches, p. 121) is that our system – not unlike the American election system – is highly structured and biased toward a relatively small (ruling? innate?) set of hypotheses, among which it selects on the basis of some very ingenious properties of the whole. The problem for cognitive science is, inter alia, to discover the constraints on that set and what those ingenious properties might be.

There is this to be said for Fodor's worry. It's not that central processes, in being global, are nonmodular but rather that they can seem thereby to be nonlocal. Now, whatever global properties a system is sensitive to had better have some systematic local effects. This seems to be as true of telephone systems and election processes (which is why there are *switchboards* and *tallies*) as of Turing machines (which act about as locally as something can). We simply don't seem to have a notion of nonlocal computation. So the problem of discovering the ingenious properties of the whole system brings with it the further problem of discovering how those ingenious properties are locally represented (or how the system manages to behave as though they are). One possibility, suggested by Kant's (1787/1966: B176–B187) "Schematism," is that some further, economical representations of the world described by a subject's beliefs are formed, and computations are defined over *them*. Much of the work on the use of imagery in problem solving suggests that these further representations may often be sensory, that is, expressed in the terms that are the output of sensory (particularly visual and tactile) modules. If, moreover, the resulting representation were, for example, partly imagistic, then there would be a lot of local properties that the system might be able (as it were) to look at. Specifying precisely what the system might pick out of such a secondary representation would, of course, still be a problem (although in this regard – and only as an example – see the suggestive work of Krueger & Osherson, 1980, which seemed to show that subjects' judgments of visual similarity coincide with Goodmanian precepts); but it is a problem whose hopelessness is not immediately apparent.

Notice that not long ago we were in pretty much the same position with regard to deductive inference. *Validity*, after all, is *not* (*pace* p. 128) "a local property of sentences." Validity is a semantical notion, involving a claim about *all possible* models – globality on a modal scale! Fortunately, however, someone figured out how to create a local proxy for it, specifying sentences in a canonical notation with a set of syntactic rules, which a completeness proof shows is adequate to the task. (Some genuinely hopeless globality does remain, however, given that validity for predicate logic is undecidable.) Now the history of the Carnapian program does make it appear that the canonical notation for deductive inference is not adequate for confirmation. But, as the example of a secondary image shows, there can be other local properties of a representation besides those involved in its deductive canonical notation. In any case, what Fodor needs for his gloomy conclusion is an argument that no such local proxies for those global properties are likely to be found. It's hard to see what an argument for a conclusion that strong would look like.

This is not to say that it's easy to think of proxies and canonical systems that will do the trick. We've only begun to do so in the case of deduction in the last hundred years, and even there we haven't yet found unproblematic representations for the full range of scientific hypotheses (e.g., involving events, causes, propositional attitudes), much less understood the sorts of computations that could be defined over them. Traditional philosophy of science has not always been a great help in this regard, concerned as it is with a great many issues other than how a good nondemonstrative inference could be computed. Appeals to the infancy of the project may be getting a little dated, but certainly appeals to its wayward adolescence – in the arms of this, now of that philosophical ideologue, from only some of whom it's recently beginning to break away – do provide a reason for not giving up on its life entirely.

Why does Fodor keep returning (pp. 117, 128) to the example of neuropsychology? That, to be sure, is an area in which we do have no reason whatever to expect a science of central processes. And that's for the by now banal reason that with regard to higher processes a computer or a brain needs to be highly plastic; for example, addresses should be open to whatever needs them. But this is not an area that displays any particular *appearance* of regularity. Confirmation, by contrast, does. As a number of writers (Chomsky 1968; Pierce 1901/1955) have emphasized, people aren't willing to accept just *any* story (in the way, say, that an address might accept most any content): There appear to be remarkable convergences in the kinds of stories people will accept about the world, given the same data. The situation looks not unlike that of grammar: convergences and stabilities whose precise character is both highly abstract and extremely difficult to pin down. An area like that, unlike the case of central neuropsychology, is precisely the sort of area that invites further research. The only limit to it might be, as Fodor rightly emphasizes, our inevitable epistemic boundedness: we simply may not be sufficiently ingenuious to understand our own ingenuity, no matter how regular it is.

For all the interest and plausibility of modules, the lack of them need not bring on, as Fodor fears, the twilight of cognitive science. At least in this field, such extreme "Wagnerian" (p. 126) views, either for or against the enterprise, are probably ill-advised. We need to content ourselves with more moderate rapture.

Faculties, modules, and computers

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Admirers of other works by Jerry Fodor will not be disappointed by *Modularity*, in which he revives the promise of an older "faculty psychology" and defends it with current facts and theories abundant in the neurocognitive sciences. As a thin volume on a thick subject, *Modularity* contains the expected assets and liabilities conferred by brevity. Chief among these are the numerous interesting and highly suggestive passages that remain only in the margins of close analysis.

Fodor's central thesis is that the older "faculty" theories minted in the late eighteenth and early nineteenth centuries were very much on the right track, and that Gall specifically had reached nearly prophetic conclusions, even if his phrenology was fatally defective. Having advanced this thesis often myself (Robinson 1976a, 1979; Robinson & Beauchamp 1978) I can only applaud Fodor's arguments, though there are several places at which I find myself in disagreement with his account.

1. Even in a brief historical review. to discuss faculty psychology without mentioning Thomas Reid (1764) is akin to staging *Hamlet* without a Prince of Denmark. The later phrenologists were fully indebted to Reid's reasoning and to his specific delineation of the mind's "active powers." I raise this issue less to preserve historical accuracy than to note the developed philosophical system that the phrenologists could invoke on behalf of their own neuropsychological speculations.

2. Though, like Fodor, I am entirely sympathetic with the faculty thesis – and as impatient as he with those who have found the thesis easier to ridicule than to comprehend – I am more wary than Fodor of that "psychologist's fallacy" so sternly discussed by William James. Our theories of mind bear the impress of our preferred methods of inquiry. Experimental designs that constrain subjects to process information or to organize their responses in a "modular" fashion yield one picture of mental life. Associationistic, introspectionistic, and psychoanalytic approaches yield other and very different pictures. The psychologist's fallacy is the assumption that the mind operates one way – for example, associationistically – when, in fact, the chosen experimental methods left room for no other outcome. Let us recall that the associationists have mountains of supporting data; the Gestaltists, crowds of *Gestalten*; the latter-day Titchnerians, any number of fundamental "structures." Thus, I am neither surprised nor convinced by the growing number of "modules" unearthed by the modularists!

3. In many passages Fodor respects the convention of referring to unacquired modes of representation and organization as "hardwired." But here the ever misleading engineering idiom is especially misleading, for what we can expect to find in the nervous system is, at most, *prewired*. If a developed faculty

psychology is to be immunized against the seductions of what J. S. Mill chose to call "Asiatic Fatalism," its patrons will have to deal with neuropsychological ontogenesis and not just note it.

4. There are too many places in the text where Fodor crosses the line dividing supposition and legislation. I cite one instance illustratively. In connection with the thesis that "input systems" are "impenetrable," Fodor says, "The point is, roughly, that wishful seeing is avoided by requiring interactions with utilities to occur *after* – not *during* – perceptual integration" (p. 103). This statement can be interpreted in more than one way, but the passage is simply false if it asserts that "utilities" cannot be inserted into even the earliest (receptor) stages of processing. And, on a lesser but equally illustrative point, note the rather queer conflation of "utilities" with wishfulness. Note also the failure to consider *centrifugal* processes by which input systems can be biased and, theoretically at least, remain so. We don't know enough about the possible plasticity of peripheral sensory mechanisms to permit firm generalizations, but it is not reckless to assume that peripheral adaptations may occur and may render processing more efficient by reducing the requirements of integration.

Apart from reservations of the foregoing sort, I continue to be concerned that our metaphorical constructs arising from computer technology may prove to be as off the mark as the older "switchboard" and still older "mental chemistries" ones were. Even in the experienced and skilled hands of a Jerry Fodor, the computational and modular model of mind gives off a somewhat clanging sound, which gets louder the closer the model gets to the brain itself. If heresy is pardonable, I admit that I cannot imagine a brain *computing*, just as I cannot imagine a computer *computing*, though I have occasionally used the latter and have, I suspect, always used the former when *I* compute. The tangled knot that is mind-brain is of such a nature that our theories turn out to be not ways of unraveling the knot but, alas, models of the knot itself. This irony is aptly if unwittingly documented in *Modularity*, a most summoning little book nonetheless.

A rapprochement of biology, psychology, and philosophy

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Right or wrong, Fodor proposes a convincing return to an integrated, biopsychological view of mind, based on more thorough (yet incomplete) knowledge of brain-behavioral relations than existed in Gall's time. The idea that human knowledge may be biologically organized as input systems and analyzers and as central processors may be helpful in moving cognitive psychology into serious contact with neuroscience, evolutionary theory, and developmental psychology.

Fodor's analyses of the errors in faculty psychology (horizontally organized) and associationism (disorganized) are compelling. His vision of vertically arranged faculties with input functions for the more general central processor raises many interesting questions, not all of them resolvable by cognitive research per se. The only cognitive strategy available for investigation of the functions of the central processor is elimination of the functions of the specific, encapsulated input modules. The central processor thus becomes a wastebasket of unaccounted-for variance or processes.

Other research strategies are available, however: comparative, evolutionary, and developmental. Ours is a science of differences, from molecular genetics to social interaction, and one cannot conduct telling experiments unless there is a contrast to be made between two or more events. Although suitable contrasts cannot be made within cognitive psychology for the hypothesized general processor, differences between the normal human adult and other species and younger members of the human species can be studied.

I applaud Fodor's concern with the possible correspondences between brain and cognitive processing. I share his optimism about advances in mapping input modules onto brain functions and his pessimism about mapping more general cognitive processes, such as memory, that are probably widely dispersed in the brain. On the other hand, knowledge that memory and thought are widely distributed in the human brain is not trivial, given the history of dispute between the localizers and the distributors.

The one drawback of this book is its inaccessibility. Given the difficult writing style, the "inside" allusions, and the meandering organization, this book will not be widely known to lay readers or, indeed, to professionals outside of philosophy and cognitive psychology, although the implications of the theory are important for many other fields. Even the vocabulary is formidable – for example, "synecdochically" (p. 25), "hypostatization" (p. 25), "apodictic" (p. 46), and numerous Latin phrases that may be familiar to other readers but which properly intimidated me. If I may be forgiven a Piagetian analogy, I can assimilate most of Fodor's proposals in a new wave of biological thinking in psychology, and I can accommodate myself to some of his proposals by two and three readings of his difficult prose.

Encapsulation and expectation

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Jerry Fodor's *Modularity* is an attempt at a framework for a theory of mind. Fodor, unlike so many other theorists, recognizes that such a theory must address the mind as an entity, not merely a collection of parts. Fodor also realizes that, despite the unitary nature of mind, some mental activities must be isolated from each other to some degree. Unfortunately, from this thesis he infers that cognitive science is impossible, or, at the very least, too hard for us to consider in the present tense. He is wrong about that, and mistaken in some of the other arguments he presents along the way.

The book begins with an amusingly told, if somewhat incomplete, history of philosophies of psychology. He does a nice job of resurrecting Gall and describing the idea of mental faculties. He neatly disposes of the idea that Turing machines have anything to do with a theory of mind, and then sets to his main task: identifying and describing what he believes to be the functional (and neural) modules of mind.

The crucial feature of a mental module for Fodor is that of "information encapsulation," a process (or set of processes) that acts independently of all other processes of mind. Such a process has access to its own private store of information and does not share that information with any other processor. When taken with a grain of salt, this is a good idea. When applied as an absolute, as Fodor suggests, it is a mistake.

Encapsulation is a prejudice about what is relevant (and what is not). As Fodor points out, it is especially important in situations of extreme urgency, where it is more important to be quick than to avoid a false alarm. One certainly does not want to consider everything he knows in identifying a pouncing panther. If this were the point Fodor wanted to make, we would be willing adherents: we, too, believe that the problem of indexing knowledge so that (only) relevant information becomes available at the right moment is central to cognition at all levels. We wish he had suggested a method for finding precisely that information in the vast world of the mind.

Alas, Fodor has another agenda. He wants to use this point to argue that modular cognitive systems cannot make reference to expectations or beliefs, which he (somewhat mysteriously) calls "feedback." He explicitly claims, "One or the other of these doctrines must go" (p. 60). But encapsulation and expectations are not mutually exclusive.

First, he claims that any system that makes reference to expectations cannot be modular. We don't understand why beliefs relevant to some particular domain cannot be isolated from unrelated beliefs (hence modularized?), but that is not the main problem.

More to the point, expectations are clearly operating in domains that Fodor would reserve as modular. He notes that "although this is possible in principle, the burden of my argument is going to be that the operations of input systems are in certain respects unaffected by such feedback." He is forced to backpedal immediately to account for the phoneme restoration effect, an example of expectations acting at the (putatively modular) input system level. We have all sorts of expectations operating at every level of processing, even the lowest: We expect to hear English phonemes in an utterence that begins with them; we expect to hear the end of a word we have heard the beginning of; we expect to see an edge between a book and the table on which it sits.

It is also important to correct the impression that expectation-driven processing prevents perceiving anything unexpected. In his discussion of scripts, Fodor makes this claim by saying that "perceptual analysis of unanticipated stimulus layouts is possible only to the extent that the transducer is insensitive to the beliefs/expectations of the organism" (p. 68). We can't imagine how he got this idea (certainly not from us), but it is quite wrong. First, expectations can be more general than an expectation of a particular stimulus layout (e.g., expectation of location name, rigid object, or vowel). Second, it is only by the virtue of expectations that an organism can be *surprised*. Surprise, or expectation violation, is crucial to learning and generalization (Schank 1982). It is also important in determining where to focus attention – it is how we manage to recognize the degree to which a perception is unanticipated in the first place. Surprise doesn't fit into Fodor's scheme: it is mandatory and fast, like a modular process, but not insensitive to beliefs/expectations, domain dependent (in Fodor's sense), or very encapsulated. It is also a fundamental aspect of cognition.

Another dangerous claim is that language is encapsulated (in Fodor's strong sense). It is not possible to understand language without reference to its content. No utterance can make sense to an understander unless the understander knows something about the topic of the utterance, a thoroughly unencapsulated process. There may well be specialized faculties for recognizing phonetic sounds, or even recognizing syllables (although we suspect that even these will make use of expectations). Since it is not requisite for making sense of an utterence, we think it highly unlikely there are similar faculties for recognizing syntactic well-formedness, as Fodor and others suggest. As for the claim that there is no evidence that "syntactic parsing is ever guided by the subject's appreciation of semantic context" (p. 78), convincing examples of the lack of perceived ambiguity (subjectively and experimentally) in syntactically ambiguous but otherwise simple sentences abound (e.g., "The can of beans was edible").

Our biggest problem with the book is its fundamental pessimism regarding the possibility of understanding the mechanisms of cognition. Fodor claims, "though the putatively nonmodular processes include some of the ones we would like to know most about (thought, for example, and fixation of belief), our Cognitive Science has in fact made approximately no progress in studying these processes" (p. 38). To this we can only say that one man's meat is another man's poison. We believe that, despite the difficulty of the problem, a great deal of progress has been made in the 25 years that Fodor finds so barren. Though we clearly have a long way to go Fodor's pessimism overlooks the shift in emphasis to exploring the contents and processes of cognition and the fascinating, albeit preliminary, results of that change. As Fodor implies, one of the hardest problems of cognition is automatically providing just the right information to a process at just the right time. Fodor would be more helpful if he were thinking about methods for doing this, instead of claiming that for sufficiently small domains it can be done and that for all others it is impossible. Perhaps initial sensory processing *is* more accessible to the modern scientist than cognition, but the problem of finding and using relevant information remains even there.

Coordinating a vast set of knowledge so that the right piece is available to the right process at the right time in a manner that allows change and thus enables learning is the major task confronting cognitive science. If Fodor's book can contribute to getting people thinking about such issues, then his effort is to be applauded. Unfortunately, we suspect that Fodor's sense of the impossibility of the problem may be what comes through. The problem may well be impossible, but it is worth trying.

Organic insight into mental organs

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Fodor's *Modularity* is an original and important taxonomy of mental processes that offers us two major lessons – one positive, one negative, and both controversial. The positive lesson is that some components of mind are best thought of as modular. These modules take the products of sensory transduction and perform various computations on them, putting them into a form that the rest of the mind can deal with. The hallmark of modules is that they are *domain specific* in their operation, and that they are *informationally encapsulated* in what they can bring to bear on the performance of their tasks. Modules, in short, are special purpose, special structure, dedicated computers, finely tuned by evolution to the functions that they serve. The task for the next generation of cognitive scientists is to embark on something of a "which hunt," that is, to find out *which* components of perceptual analysis belong with *which* other ones inside *which* modules. That this is botanizing of modules should be the task of cognitive science will come as a surprise to most cognitive psychologists, for cognitive psychology (mistakenly, according to Fodor) has been searching for principles of cognitive architecture and function that are neither domain specific nor informationally encapsulated.

That this is a mistake is the second, negative lesson of Fodor's book. It is not that there are no nonmodular parts of mind. There are. Fodor calls them "central systems," and their role is to take the outputs of the various modules and integrate them in such a way as to help us fix belief (i.e., decide what is true or false about the world), solve problems, make decisions, and act. The problem is that just because central systems are nonmodular (that is, neither domain specific nor informationally encapsulated), we can't have a *science* of them. Fodor makes this argument by analogy; he likens the characteristics of central systems to the cognitive character of science. Science is a system of belief fixation par excellence. Moreover, its constituent processes of data gathering, inference making, hypothesis construction and test are all public, and thus available for inspection. It is thus the externalization of the deepest and most significant components of our mental life.

Unfortunately, what centuries of struggle to understand science have to tell us is that there is no science of science. Whether one is being normative (i.e., telling science how it ought to do its business) or descriptive (examining how it actually has done its business), there are no laws of science to be found. And this is precisely because the cognitive character of science is nonmodular. Anything we know or discover may be relevant to a given scientific claim (no informational encapsulation), and a test of any particular hypothesis is actually a test of our entire conceptual scheme (no domain specificity). It is an interesting point of historical irony that Thomas Kuhn appealed to psychological principles (of perception) to help explain and justify his account of the history of science (1970), and Fodor now appeals to an essentially Kuhnian account of science to help explain and justify his psychological theory.

What is one to make of these two lessons? As to the first one, that there exist cognitive modules that share an interesting and important set of characteristics, *Modularity* succeeds in convincing me that modules are worth looking for, but not that Fodor has found them. As Schwartz and Schwartz (1984) suggested in a lengthier review of this book elsewhere, Fodor plays a little too fast and loose in identifying modules. They sometimes seem so smart that they are hard to distinguish from central systems, and they sometimes seem so dumb that they are hard to distinguish from "mere" transduction. On Fodor's story, they had better be neither; if they are smart like central systems we can't have a science of them, and if they are dumb like transducers, the science we have of them probably won't be psychology. Whether modules will one day be better delineated than they are right now I don't know. That the task of delineating them is worth the effort I have no doubt.

And as to the account of central systems – what Fodor calls his "first law of the nonexistence of cognitive science" – here I think that Fodor has delivered both a major insight and a major problem for future research. The insight is that the more global a process is, the less anybody does, can, or will understand it. This insight is reflected in what is called the "frame problem" in artificial intelligence research (e.g., McCarthy & Hayes 1969; Newell 1982). The frame problem is the problem of imposing boundaries, or frames, on what data from our vast store of knowledge actually bear on a given topic. Thus far, the frame problem has been solved *for* the computer, by carving the computer's data set into discrete domains or modules. It is also reflected in research on human decision making under uncertainty, in which the regularities one obtains in research may be at least partly the result of the way in which decision frames are constrained by the experimental situation (e.g., Tversky & Kahneman 1981; see Schwartz & Schwartz, 1984, for discussion). Thus, we may be suffering, all of us, from an *illusion* of progress in understanding the way in which central systems fix belief and determine action, because our methods of analysis and experiment solve the hardest problem for the subject (or computer) by framing the relevant domain and thus turning central systems into modular ones.

The problem for future research is that, while central systems, like science, are utterly global *in principle* (that is, anything might be relevant to the fixation of a particular belief), they are surely not so global in practice. The concept of "automaticity" in cognitive psychology captures well the notion that practice can create modules out of all sorts of pieces of central systems (e.g., Atkinson & Shiffren 1966; LaBerge & Samuels 1974; Posner & Snyder 1975). People do solve the frame problem, after all, even if computers don't. How do they do it? How do automatisms of belief fixation get established? This has always been a research question of substantial interest. But now Fodor has raised the stakes. If he is right, answering this question may be prerequisite to answering any other important questions about the nature of central systems.

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Lexicon as module

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Fodor's monograph raises an enormous range of important questions. For reasons of space, this commentary will be "informationally-encapsulated," dealing only with the status of the lexicon as a module in the language comprehension system. One way of responding to Fodor's ideas is to try to work them out in some detail in an interesting domain, and that is what some of us have been attempting to do with regard to word recognition. What follows is a much-abbreviated version of a larger story (see Seidenberg, in press; Seidenberg & Tanenhaus, in press, for summaries).

Psychologists are fond of saying that complex cognitive processes such as language comprehension result from "interactions" among different sources of knowledge. Many people are developing models that represent these interactive processes using formalisms such as production systems (Anderson 1983) and the parallel distributed processing schemes of Feldman, Hinton, Fahlman and others (e.g., Fahlman, Hinton & Sejnowski 1983). Psychologists seem to be taken with the realization that, as Turing machine equivalents, these formal systems are sufficient for the purpose of representing any computationally explicit model of cognition.[1] They (we?) seem to be less sensitive to the problems that arise in attempting to develop explanatory theories within such frameworks; having a sufficient formalism is one thing, developing principled explanations of anything is quite another.

These observations are obvious to anyone with a passing familiarity with the development of linguistic theory over the past 25 years. Transformational grammars (and related variants) make use of an explicit formalism (base rules, transformations, and the like) known to be sufficient for the purpose of representing facts about linguistic structure. As with production systems, or distributed parallel processing systems, the formalism is too powerful; although formalism is known to be able to account for the facts about the structure of human languages, it is also necessary to explain why certain structures do *not* occur (equivalently, why certain languages cannot be learned or processed). In theoretical linguistics, this question has led to a search for principled constraints on various components of the grammar (e.g., on the form of phrase-structure rules or the operation of transformations). In order to develop explanatory theories using the computational formalisms mentioned above, then, it will be necessary to develop principled constraints on their form and operation.

In this context, modularity represents a hypothesis about one possible constraint. In the terminology of the parallel distributed processing systems, it represents a hypothesis about the scope of interactive processes. It suggests that these processes are bounded within specifiable domains. The primary value of Fodor's monograph is that it attempts to provide a theoretical basis for the existence of this particular constraint. I would simply add that there is no hope of developing a computational theory that is explanatory unless some such constraints are discovered.

The suggestion from recent work is that the lexicon constitutes a module in the comprehension system. To the extent that Fodor develops many of his ideas with regard to the lexicon, it is worth considering this claim in more detail. A model such as McClelland and Rumelhart's (1981) can be seen as a first pass at using the parallel distributed processing scheme to represent interactive processes within the lexical module. This framework has some nice properties; it is explicit (unlike the box-and-arrow information processing models psychologists have been using to model word recognition), and it is especially well suited to representing changes in the availability of information over time, which I believe to be the central facts a theory of word recognition must explain. A good many of the basic facts about word recognition can be accommodated within an extended version of the McClelland–Rumelhart model (Seidenberg, in press).

The question then arises: What types of information are relevant to word recognition? The traditional view in psychology has been that the recognition process draws upon information provided by the linguistic and extralinguistic contexts of occurrence. That is, there are interactions between lexical and nonlexical sources of information. Our claim, following Forster (1979) and others, is that word recognition results from the operation of an autonomous lexical module. Word recognition depends not on the information provided by the *literal* context in which a word occurs, but rather upon the *virtual* context created by one's knowledge of the lexicon. In Fodor's felicitous terminology, word recognition is informationally encapsulated.

This view will come as a surprise to reading researchers, whose theories have stressed that skilled reading requires the efficient use of contextual information to facilitate word recognition (e.g., Goodman 1970). This claim also seems to fly in the face of an enormous empirical literature demonstrating that the manner in which a word is processed depends on the context of occurrence. I have reviewed these issues elsewhere (Seidenberg, in press; Seidenberg & Tanenhaus, in press); here I can only summarize the conclusions. Essentially, the context in which a word occurs influences the integration of a word with the context, not the recognition of the word itself. That is, nonlexical information influences postlexical processes, but it does not penetrate the operations of the lexical module. The view that has emerged is one in which the lexicon, operating as an autonomous processing module, yields up certain kinds of information an an invariant manner. This information is then available for further processing (e.g., certain kinds of information are retained, elaborated, integrated with the context; others are suppressed).

In developing these ideas, it has been necessary to evaluate the enormous literature on the use of contextual information in terms of the loci of the effects. Context could either influence the decoding of the word itself (which would violate the modularity hypothesis), or it could influence a postrecognition judgment of the relatedness of word and context (which would not). It has been possible, in a limited way, to develop tasks that are differentially sensitive to these two kinds of effects (Seidenberg, Waters, Sanders & Langer, in press). These have allowed us to determine whether different kinds of contextual information influence sensitivity to a word target, or whether they bias postlexical judgments of context-target relations. In general, the effects are of the latter sort. The view that reading is a "psycholinguistic guessing game" isn't a bad theory of the process – if you're a poor reader whose lexical processes are impaired (Stanovich 1980).

Lexical priming, which Fodor discusses extensively, has a rather special status. Priming effects (i.e., the fact that a word is recognized more rapidly when preceded by a semantically or associatively related word) violate a strong version of the modularity hypothesis in which context has no effect whatsoever on lexical processing. However, as Forster (1979) noted, priming is a consequence of the organization of the lexicon itself; in this sense it is internal to the module. Modularity would be violated only if nonlexical information – provided, for example, by the syntax of the context or a propositional representation of the meaning of the context – influenced recognition. Fodor promotes a clever idea concerning the functions of priming (i.e., it is "the means whereby stupid processing systems manage to behave as though they were smart ones," p. 81), to which I would add three observations. (1) We are talking about tiny effects; the best estimates of the size of the effects come from studies using the naming task, and they are on the order of a hundredth of a second (Seidenberg et al. 1984). For this and other reasons, I doubt whether they contribute in any important way to the comprehension process.[2] (2) These considerations suggest that Fodor is too hesitant about viewing priming effects as artifacts of connections in the lexical network (p. 82). My own feeling is that they are artifacts of the manner in which the lexical network is organized for the purpose of producing speech (Forster 1979), not comprehending it. (3) In the absence of any independent evidence concerning the scope of lexical priming, there is a danger that it makes the modularity hypothesis unfalsifiable. Any annoying effect of context on word recognition can always be attributed to priming. I believe that the scope of these priming effects is quite limited (Seidenberg et al. 1984), but this issue needs to be tied down.

In sum, Fodor's picture of lexicon-as-module has much more going for it than his text suggests. I think a lot of work remains to be done, however, in establishing the properties in virtue of which a system is modular. For example, what are the properties of the lexicon that confer modularity upon it, and are these preserved in other parts of the comprehension system? Many of the characteristics Fodor holds to be characteristic of input systems could well be true of nonmodular systems (e.g., being fast, mandatory, opaque to introspection). In the case of the lexical module, what seems to be relevant, roughly, is that lexical knowledge is stored rather than computed. The McClelland and Rumelhart (1981) model describes interactive processes among elements within the lexicon. These elements (word and letter detectors, for example) bear (essentially) fixed relations to one another. So it is the character of the representations that is relevant, not merely the manner in which they are processed.

I think that some version of the modularity hypothesis has to be true; the only question is whether it is interesting. Do modular systems operate over nontrivial domains? Research on word recognition suggests that, in at least one case, the answer is yes. It would be surprising if the lexicon were unique in this respect.

NOTES

1. In particular, Anderson (1983) thinks that production systems represent a unified, nonmodular theory of mind. Nothing whatever follows about the "architecture of cognition" from the fact that productions are sufficient for the purpose of doing computer modeling. For many psychologists, the hidden attraction of production systems may be that they reintroduce S-R chains as the basis of intelligent behavior.

2. Other reasons include the fact that the conditions under which lexical priming is obtained in laboratory studies rarely obtain in actual texts or discourse (see Foss 1982 for a different view, however).

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Controlled versus automatic processing

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Fodor is fond of analogies; indeed, he believes them to be a basis of much higher order thinking. Perhaps he would then not mind my drawing an analogy between reading *Modularity* and eating a large piece of gourmet cheesecake.

Before eating a piece of gourmet cheesecake, one fully expects to enjoy the experience, even to find it a memorable one. Immediately after the eating, one expects one's stomach to be upset, as the piece of cake sits in one's stomach, not easily lending itself to digestion. One knows that the cake will be heavy going and that, despite its good taste, the caloric content is perhaps higher than the content in nutrients should merit. Finally, one knows that the piece of cake will probably taste less and less good as one becomes satiated, yet one attempts nevertheless to finish off the piece.

Such was my experience with this book. I knew I would enjoy the book; I did. I knew that this book, like Fodor's others, would be unforgettable; it was. I also knew that no matter how much I enjoyed the book when I was reading it, if Fodor followed his past pattern, I might need mental Maalox when I was done; I did. I expected the book to be higher in mental calories consumed than it deserved; it was.

The book trailed off sadly at the end. I think satiation hit Fodor before it hit the reader. Here's why. In the final chapter, Fodor dismisses in about 10 pages the value of the contribution of the work of researchers such as Simon, Minsky, Newell, Anderson, Schank, and Winograd, to name a few. This work "has produced surprisingly little insight" (p. 126), has "led to a dead end" (p. 126), and leaves us with "no serious psychology of central cognitive processes" (p. 129). Moreover, we learn in the preceding chapter that our knowledge of intelligence is pretty much limited to "some gross factoring of 'intelligence' into 'verbal' versus 'mathematical/spatial' capacities" (p. 104). So much for the cognitive theory and research of Carroll (1981), Hunt (1980), Pellegrino and Glaser (1980), and myself (Sternberg 1984), among others. Coincidentally, I suppose, we have made progress only in those areas of cognitive science with which Fodor's theory of modularity deals. What is the support for these claims? Because, we are told, there is practically no relevant evidence from this work (p. 104), Fodor relies upon two sources of evidence: his view of the philosophy of science (which is supposed to be relevant somehow to the organization of higher-order thought processes) and the table of contents of an issue of *Scientific American!* To some, Fodor's weighting of various sources of evidence might seem odd, if not downright bizarre. The lapse of scholarship in the last two chapters is so annoying that it detracts from the rest of the work, which, unlike these chapters, presumably deserves to be taken seriously.

Fodor argues that bottom-up, perceptual processes are modularized, but that top-down, conceptual processes are not. Fodor is a persuasive advocate of his position, but he does not seriously consider alternative hypotheses, or he dismisses them cavalierly. There is an alternative interpretation of the available evidence that actually has stronger evidentiary support than Fodor's view: that automatized information processes are modularized but that controlled ones are not (Sternberg 1981, 1984). Because many perceptual processes are relatively automatized, they are modular; because many conceptual processes are not automatized, they are not modular. But the correlation is not perfect, as the expert-novice literature shows. Fodor's theory, for example, is unable to account for any of the findings in the literature on expert-novice differences, suggesting that experts in various areas do in an automatized fashion things that novices do in a controlled fashion, whether it be driving, reading, or solving physics problems. The view proposed here can account for all of the evidence supporting Fodor's theory, plus other evidence that Fodor's theory cannot accommodate.

Most of the simple perceptual functions with which Fodor deals in his section on the applicability of the modularity thesis are fully or largely automatized by adulthood. Thus, according to the present view, they access modular, or "local subsystems" of declarative and procedural knowledge (Sternberg 1981). But some more complex functions can also be automatized after large amounts of practice or experience and under the right conditions. By automatizing such functions, experts can free processing resources for novel kinds of stimuli, whereas novices do not have such resources freed for dealing with novel stimuli. On this view, then, experts become experts because they modularize processing that novices have not yet (and possibly never will) modularize, whether these processes are the lower-level perceptual ones that Fodor considers to be modular, or the higher-level conceptual ones that Fodor believes are not modular.

I don't forget gourmet cheesecake. I don't forget Fodor's books. I don't forget the stomachaches either. Fodor is always provocative, even when he is wrong. Perhaps that is why this is one of the few books I have read lately in just one sitting.

Author's Response

Reply module

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Before I set out my remarks about the individual commentaries, I think I'd better say a bit about what sort of book *Modularity* was supposed to be. Though I was pretty explicit in my disclaimers (see, for example, paragraph 2), many of the commentators appear to have mistaken my intentions. Thus **Morton** feels "a sadness that [I] did not put into perspective . . . the work of the last 15 years or so . . ."; **Grossberg** lists lots of articles (of Grossberg's) that he thinks I ought to have read and discussed; and **Sternberg** disapproves of "the weighing of evidence" in the section on central processes.

Modularity was not, however, an attempt to make the case for modularity; or to weigh the evidence; or to summarize the literature; or to dissect the alternatives. **Forster** got it right; *Modularity* is "a programmatic sketch of the kinds of things it would be worth having a theory about." Or, to put it less politely than Forster does, *Modularity* is a potboiler. It seems to me crucially important to get cognitive scientists thinking about alternatives to the "New Look/Interactionist" view of cognitive architecture that has dominated the first several decades of the field. *Modularity* provides a sketch of such an alternative, together with a smattering of supporting evidence and an occasional indication of how to get around some of the data that were alleged to support the earlier story. But, as Forster says, the evidence is appealed to "essentially for illustrative purposes"; so, for that matter, are most of the arguments.

Perhaps there will come a time to set out the evidential case for modularity in detail. That won't happen for a while, though. For one thing, if the modularity story is right, a lot of the experimental evidence that argues prima facie for the cognitive penetration of perception will have to be undermined. This will be tricky because to quote **Forster** yet again, "all current [experimental] methods of interrogating the internal states of the processor work through the central cognitive processes" (e.g., through the subject's appreciation of the demand characteristics of the experimental task). It's going to take some cleverness to factor these – from the modularity theorist's point of view –noisy variables out of the reaction times. The burden is upon the modularity theorist to find ways to do so.

My impression is that when experimentalists have thought to try, the results have been not unencouraging.

Often enough, what seems to be the effect of the cognitive penetration of perception proves to be the effect of postperceptual (e.g., decision) processes. (See, for example, the commentary by **Seidenberg** and the references therein.) When this experimental reevaluation is considerably more advanced, and when our picture of a modular architecture is considerably more detailed, *then* it will be time for a book that makes the evidential case for the theory. That book will be a lot longer than *Modularity*, its style will be a lot more sober, and, I expect, it won't be nearly so much fun to write. In short, I plead guilty to the charge that **Forster** anticipates: I *am* more interested in the issues than in the facts. Facts, in my experience, are ephemeral and change with the changing fashions. But issues are forever. (See the commentaries by **Gross** and by **Marshall**.)

Since, however, whereof-one-cannot-speak-thereof-one-must-be-silent, why even try to write about modularity at this early stage? Why not just shut up? I want to attempt, in a paragraph or so, to communicate my present sense of a crisis of theory.

The following, it seems to me, is a fair reconstruction of what a lot of us were thinking 10 or 15 years ago. "Look," we said (if only to ourselves), "because we now have the computer metaphor, we now have a fair idea of what sort of thing a mind is. A mind is an inference machine. In the cases that interest psychologists most, the inferences that minds make are nondemonstrative arguments-to-the-best-explanation. Since perception and cognition are homogeneous (as we learned from our New Look forebears), this picture of the mind holds for both: Perception is a species of thought, and thought is a species of inference-to-the-best-explanation.

"Therefore, let us look carefully at a piece of perception (at sentence parsing, for example). What we will find is thought in microcosm. We will find, for example, that what goes on in my head when I recover the phonetic analysis of a wave form is a lot like what goes on in Sherlock Holmes's head when he figures out the identity of the criminal (see Fodor, Bever & Garrett 1974). This is a happy situation. We can use our hunches about what goes on in Holmes's head (more pedantically, our developing theories of nondemonstrative belief fixation) to predict the character of perceptual processes, and we can use what we find out about perception to enrich our understanding of inference to the best explanation. Thus shall we tug at one another's bootstraps, and thus shall we ascend."

In my current view, however, this program is dead. I have three brief points to offer (for further elucidation, see the commentary by Janet **Fodor,** which seems to me to be right on. Who says marriage makes strange bedfellows?)

(a) It was odd that *we*, of all people, should have held this view. The doctrine of the homogeneity of cognitive processes sits oddly with the doctrine that speech is special (see the commentary by **Mattingly & Liberman**). The latter view requires precisely what the homogeneity of mentation precludes: a *faculty* psychology. This tension runs through a lot of early MIT stuff on cognition. As Mattingly & Liberman point out, it had us arguing that language is special but that everything else is too. In retrospect, our implicit attempts to paper over the cracks were not convincing.

(b) The best work on computational models of perception (in language and in vision, say) has not provided much evidence about the structure of thought. It is, for example, very far from obvious how to generalize what we've learned about parsing to a theory of nondemonstrative inference in problem solving. (Indeed, it is very far from obvious how to generalize what we've learned about parsing to other areas of the theory of perception, for example, to a theory of the perception of visual form. Off hand, I can think of *nothing* that these fields have learned from one another.)

In consequence, though it has provided some really important insights into perception, cognitive science has discovered very little about thought – except that it is hard to understand. **Forster** again: "[in] current texts in cognitive psychology . . . the sections dealing with what Fodor would treat as input modules are rich in content, whereas the sections dealing with thought and reasoning have probably not changed very much over the past 20 years." I don't see how anyone familiar with the field can deny the broad accuracy of this evaluation (though, apparently, some of my commentators are prepared to do so).

(c) Conversely, the best insights we have had about cognition (e.g., the work on the role of stereotypes in inductive inference) have shed surprisingly little light on the computations involved in perception. Perception doesn't seem to be thought in microcosm after all. Contrary to initial expectations, for example, it has not turned out that the facts about parsing are, in any interesting sense, specializations of general truths about cognition. (No doubt *uninteresting* versions of the claim are defensible: For example, all cognitive processes, including perceptual ones, can be formalized as production systems if they can be formalized at all. See Anderson, 1983, where this truism is vigorously maintained.) I wish particularly to emphasize a point that Janet **Fodor** makes: It may be possible to save the idea that thought is in some sense heuristic. Who knows? But nothing in the current computational literature suggests that perception is.

Of course, this is all – as I'm told they say in California – just a value judgment. You may not share this synoptic view of the state of the field; if you don't, *Modularity* won't convince you to (it was never intended for that), and you will feel no urgency for theoretical reform. But if you do share it, then you may be in want of a hypothesis about cognitive architecture that accounts for the (putative) distribution of our (putative) successes and failures. *That's* what *Modularity* was for.

An aside about style before I turn to individual commentaries. The knockabout prose was intended to make it clear that *Modularity* presumed to be rather less than the last word about cognitive architecture. Some of the commentators (**Scarr**, for example) disapprove. Ah me, you can't please everyone. My grandmother laughed at the jokes! (Well, she laughed at some of them.)

Some of what I have to say about the individual commentaries is explicit in the preceding; and about some of the commentaries I have nothing to say at all, either because I agree with them, or because they ask questions that I don't know how to answer. I am grateful to several of the commentators (e.g., to Professor **Gross**) for having provided further ammunition in what I take to be a good cause.

Here's the rest:

I don't understand **Caplan**'s sense of modularity or how it is connected with the questions about Neo-Cartesianism I raised at the beginning of *Modularity*. Though it may be true, as Caplan says, that contextual information is rarely helpful in making *syntactic* decisions (so that one might say that such decisions are encapsulated "because of the nature of the representations they compute"), nothing like this holds for perception in the general case. Lexical content, for example, is often highly redundant in discourse context. So if decisions about lexical content are encapsulated, it is not because of the nature of the representations.

Caplan's main point is that processes of input analysis are not, after all, encapsulated as I claim. He cites the effects of context on phoneme restoration as typical. "Fodor dismisses this effect's being a counterexample to encapsulation . . . because it operates at the level of a response bias rather than at the level of 'perception.' . . . But one might well ask: Where else could it operate?" I would have thought the answer was pretty clear, given the story we all grew up with about how context effects bias perception: that is, they might operate *pre*perceptually, i.e. predictively. Caplan says that semantic context "can only interact with phoneme recognition via its utility in predicting the occurrence of particular words." Of course it is the words, not the phones, that are redundant in semantic context; but the question is: How is this redundancy exploited? The relevant possibilities are that the word is predicted preperceptually on the basis of the context (and the perceptual encounter with the word token serves to validate or disconfirm the contextually based prediction), or that the context is exploited postperceptually to validate those candidate identifications of the token that an encapsulated perceptual system tenders. This distinction could, I quite agree, do with lots of clarification. But I see no reason to doubt that it's real or that the issue between encapsulated and "top down" systems that it engenders is substantive. See the commentaries by **Forster** and by **Seidenberg** for further elucidation.

Carroll, like Gall, is moved by the idea of appealing to individual differences as a means for the identification of psychological faculties. I have nothing more to say about this here than what I said in the book: I find the logic of such appeals obscure. The mere existence of differences between (say) people's abilities to play chess would not, in and of itself, be any sort of reason for postulating a *vertical* chess-playing faculty. It might be that chess playing recruits a complex of *horizontal* mental capacities and that the best chess player is the one whose mix is nearest optimum.

So, even if one is prepared to accept **Carroll**'s optimistic assessment of the progress of individual difference psychology (the factor analytic work has established "30 or so abilities," not just 30 or so factors), that would not be a reason for supposing that central processes are organized around faculties of the sort Gall had in mind, that is, around vertical psychological mechanisms. Carroll says that the problem of making clear how findings about individual differences bear on hypotheses about mental architecture "has yet to be proved insoluble." Of course it has; but that remark seems to put the burden of argument on the wrong party.

I think that what **Gallistel & Cheng** report is terrific, but I don't see why it implies the modularity of central processes. What it looks like on first blush, is a "vertical faculty" in charge of the perception and recall of spatial layout and location. (Perhaps Gallistel & Cheng suppose that *all* of a rat's cognitive processes will turn out to be computationally encapsulated in the same way; but their findings seem neutral on that question. And, anyhow, rats aren't people.) In fact, "spatial orientation" was one of the cognitive systems that *Modularity* suggested might be a good candidate for modularity; so I'm pleased as punch.

By the way, work like that of **Gallistel & Cheng** suggests how modularity theory might put some muscle behind that permanently uncashed check, "perceptual salience" (see also *Modularity*, p. 94). That alone would be worth the price of admission.

I'm not clear just which of my claims it is that **Gardner** doesn't like: that input processes are modular or that central processes aren't. (I think what he really has in mind is a general unsharpening of edges, a process I tend to resist on temperamental and aesthetic grounds.) Anyhow, he says that "a key point of contention has to do with the extent to which vertical faculties can be considered in isolation from the surrounding culture. . . . Even phoneme perception and sensitivity to visual illusions are affected by the kinds of sounds or sights present or absent in a particular culture." Now, I don't want to take on the whole issue of nature versus nurture just here, please. And, no doubt, if we were both to set out our views, mine would turn out to be appreciably more nativistic than his. But I do think Gardner is making a mistake that nativists have warned against since (and including) Descartes: namely, assuming that the innateness of a cognitive capacity implies that it should be available independent of environmental input. Nobody – I mean *nobody* – holds a form of nativism about, say, language, that denies that what the child hears is pretty important in determining what language he comes to speak. (How, precisely, to describe what the nativist does deny is a long story; see Fodor, 1981, chap. 10, for discussion. At a minimum, he denies that environmental effects on innately specified cognitive capacities are typically instances of *learning*, as opposed, say, to imprinting.) In fact, it is central to the nativist program – traditionally in ethology and currently in linguistics – to specify *precisely* what interactions between endogenous and exogenous information the normal maturation of mental capacities require. See, for example, the recent discussion of environmental "parameter setting" in syntax acquisition.

With regard to **Gardner**'s remarks about development, I doubt that cognitive ontogeny recapitulates cognitive phylogeny. I can imagine viewing the former as a gradual emergence of isotropic systems from encapsulated ones; but nothing about individual cognitive development seems to me to suggest the corresponding ontogenetic process. (Deep down, I'm inclined to doubt that there *is* such a thing as cognitive development in the sense developmental cognitive psychologists have had in mind. But perhaps that's a coattail best trailed some other time.)

Gardner's proposed alternative to a mechanism of central integration is that "various modules, faculties, or intelligences can come to work together in carrying out complex cultural tasks." This is, no doubt, a thought worth pursuing, but I don't really see how it would work in any detail. First, I don't think that there are specialized, modular, cognitive systems corresponding to most of the things that we know how to do. (Could there be a special mechanism for, say, mowing lawns in the way that there might be a special mechanism for talking?) Second, coordination problems are *hard;* their resolution typically demands foresight, weighing of gains and losses, appreciation of feedback – all the stuff, in fact, that our cognitive science doesn't know how to model. I simply don't believe that this all falls out of an unmonitored, preestablished harmony of the modules.

I think **Gardner** and I agree pretty well about what the options are. The residual dispute is only over what's true.

Glucksberg writes: "we have made reasonable progress in understanding cognitive systems that are not modular. . . . Human memory, for example, is now far better understood than it was in Ebbinghaus's day." I doubt that this is so. What is true – and what Ebbinghaus apparently didn't know – is that memory, for anything but the most nonsensical of nonsense stimuli, is constructive, strategy ridden, isotropic, and Quineian. But what we aren't within hailing distance of is an account of how memory – or, as remarked in *Modularity*, anything else – goes about having these properties. (I also doubt that we "understand more about how people play chess than we did 20 years ago"; that's why our best chess-playing machines have to make do with big, fast, boring memory searches.)

Glucksberg continues: "Perceptual phenomena are notoriously context sensitive. . . . e.g., the effects of perceived form and contour upon color perception." But this is so beside the point that I wonder whether Glucksberg hasn't just *missed* the point. The parsing of, as it might be, a token of "plant" as a verb or noun is sensitive to (lexical and syntactic) context. Now, parsing may be nonmodular, but that sort of observation about context sensitivity doesn't make it so. To show nonmodularity, you have to show that a capacity is affected by information that is *external to the module by independent criteria.* Glucksberg makes no attempt at all to do this, suggesting that he has failed to appreciate the logic of arguments about modularity. Perhaps all his example shows is that there is an (encapsulated) system in the business of inferring color from form (and/or vice versa).

Similarly, with **Glucksberg**'s other arguments. To show effects of experience on perceptual systems is not, in and of itself, to show effects of *learning.* And, anyhow, no nativist has ever supposed that innate capacities are unaffected or unformed by environmental interactions. (See my reply to **Gardner.** How many times has this point been made in the last 30 years? How many times in the last three centuries? How many times is one going to have to make it again?) I do think that the effects of instruction upon the recognition of fragmentary figures is a problem for a strictly data-driven view of visual perception (one of the few sorts of really recalcitrant data I know of). But these phenomena are miles from decisive. They show that perception and background information interact *somewhere* (something no one ever doubted); but whether modularity theory is tenable turns on the *locus* of this interaction (see **Forster**'s discussion), about which nothing or less is known. (By the way, prima facie, the fragmentary figure phenomena concern object recognition, not form perception. Object recognition could not be encapsulated on anyone's story; so again, the bearing of the findings on the claims for modularity is pretty obscure.)

I think that **Glucksberg** considerably underestimates the polemical resources available to the antiinteractionist position. This game isn't going to be won by flourishing a fact or two; if Glucksberg wants to play, he'll need appreciably subtler arguments.

I argued for the holism of central processes by analogy to the holism of scientific confirmation; **Glymour** has his doubts about the latter. And, of course, it would be absurd to hold that scientific confirmation is Quineian/isotropic if that implied (as Glymour thinks that perhaps I think it does) that "when we set about to get evidence pertinent to a hypothesis we are entertaining, we somehow consider every possible domain we could observe." That approach, as Glymour rightly says, is out of the question either as psychology or as philosophy of science. But this doesn't show that the claims about epistemic holism are "palpably false"; it just shows that, if there are holistic estimates of confirmation, they can't be computed by surveying the background of previously held beliefs seriatum. Somehow, they must all contrive to make their presence felt without each standing up and being counted. That, as they used to say in the '60s, is the problem, not the solution. That there *is* a problem and that we don't know the solution was, of course, the burden of my plaint in *Modularity.*

Glymour has what seems to me to be very interesting things to say about how to deal with the holism issue (see also the commentary by **Rey,** which takes a similar line): Maybe we could somehow get global properties (e.g., simplicity, coherence) to be reflected in local ones for purposes of case by case decisions about confirmation relations. Well, maybe we can, though Glymour's presentation is perforce very sketchy, and it's a little hard to see how he thinks the thing would go. So, for example, he says that one way we ensure consistency "is by isolation of predicates." But, in the next sentence, another means "is by establishing routes from one theory to another [hence, presumably, from one theoretical vocabulary to another] and verifying their consistency at appropriate checkpoints." This looks a lot like taking back with one hand what one gives with the other. Since there are routes from T1 to T2, the consistency of T1 with T2 (and with the intermediate theories that provide the routes) is after all at issue when questions of confirmation arise about either. How, then, do we distribute the burden of maintaining coherence with the data between the two theories? How do we tell which of the intertheoretic routes ought to be traversed in assessing a given confirmation relation? (The frame problem *again*!) And so forth. I don't say that it can't be done Glymour's way; and I'm light years from holding that it's not worth trying. But the mysteries do seem to me to be *very* deep.

I think, really, that there's an underlying clash of intuitions between **Glymour** and me about just how deep the problems about confirmation go. My guess is that they survive idealizing away from the motley of heuristics that we use to effect the de facto encapsulation of our working estimates of confirmation, both in science and in cognition. What I expect Glymour thinks is that, if you spell out these heuristics you've said all there is to say about confirmation, again both in science and in cognition. (I *know* that's what lots of people in artificial intelligence think.) We'll see, in a couple of hundred years or so, whose intuititions are right; but I don't understand why anyone would actually hope that it comes out Glymour's way. Nothing, it seems to me, is sadder than seeing what appeared to be a profound and beautiful problem trickle away into a pile of glitch. (For more on this, see the commentary by **Schwartz,** with whose view of these issues I am largely in agreement.)

The results **Hunt** alludes to need to be considered carefully or not at all, and I haven't space for a careful consideration. I remark only that: (a) even if some of the individual difference stuff comes out my way (compare the commentary by **Carroll**), I find arguments from individual differences to modularity unconvincing; and (b) Hunt seems to have confused my notion of modularity with Gall's. As I went to some pains to point out in *Modularity,* Gall takes it as a point of definition that vertical faculties cannot compete for horizontal computational resources (e.g., attention). But I'm neutral about this; I take the essential fact about modularity to be *informational* (not resource) encapsulation. So if the speed of information processing in a "process buried deep in the visual input module . . . was influenced by a central perceptual process," the former may nevertheless be modular by my criteria (though not, to repeat, by Gall's). (**Jusczyk & Cohen** make the same mistake in paragraph 4 of their commentary.)

Hunt wants to know why autonomous modules should need coordinating. Short answer: Because there are cases where their outputs conflict. See, for example, the coordination problem discussed by **Mattingly & Liberman;** when a language processing module and a general acoustic processor disagree about a signal, coordination is achieved by "some mechanism that . . . guarantee[s] the precedence of speech." According to the Mattingly–Liberman proposal, this coordination is effected not by a centra computational process but by fixed architectural relations between the modules (specifically, serial access to the input). I found the Mattingly–Liberman discussion extremely illuminating, but I think it unlikely that architectural solutions for coordination problems can work in the general case (see my reply to **Gardner**). Nor, I suppose, are Mattingly and Liberman committed to any such general claim.

Jusczyk & Cohen's main complaint seems to be that "it is hard to see how one can empirically refute" the claim that (e.g.) language processing is informationally encapsulated. Oh, would that this were so. But I fear that Jusczyk & Cohen must be suffering from a temporary failure of experimental imagination. Off hand, I can think of half a dozen experiments whose results would prejudce such claims one way or the other (Merrill Garrett and I are now in fact running some of them). And anyway, the interesting question isn't what's refutable or confirmable with the current armementarium of experimental techniques. The interesting question is: What is *true?* This, apparently, is a hard lesson for psychologists to learn; I wonder why.

In fact, as **Sidenberg** points out, there is already a body of experimental data on precisely the issue whose experimental tractability **Jusczyk & Cohen** explicitly question: the encapsulation – or otherwise – of lexical search. My own impression of these data is quite close to (in fact, has been much influenced by) the views of Seidenberg & Tanenhaus (in press): what looks like penetration in the first fine, careless rapture looks like an influence on "the integration of a word with the context, not [on] the recognition of the word itself" when you take a second, more jaundiced view. (I shall, by the way, have nothing more to say about the Seidenberg piece; I hope – and am strongly inclined to suspect – that every word of it is true.)

I don't know what to say about reading, the other issue that **Jusczyk & Cohen** raise. The curious connections between reading and the language mechanisms per se make it a very special case. But I should want to resist any general assimilation of the effects of modularity to those of overlearning or "expertise" (see the commentary by **Sternberg**). I think the differences are far more interesting than the similarities.

I doubt that **Kagan** believes that the world is flat, not even when he's driving in Cambridge traffic. On the other hand, I take the point that a lot of our problem solving is de facto myopic; we don't, on anybody's story, use all of what we know with optimal generality and efficiency (see my reply to *Glymour*). The question is: How deep does this sort of stupidity go? I think it's something one should idealize away from in studying what's important about cognition; Kagan apparently thinks that it *is* what's important about cognition. We'll see.

It is a mistake to suppose that cross-modal transfer indicates nonmodularity. This point is made with some emphasis in *Modularity* (see, for example, the discussion of the "McGurk effect").

I can't imagine what **Kagan** thinks follows from such observations as "the fact that I call a bright red autumn maple leaf a beautiful object . . . does not prevent my neighbor from regarding the same leaf as potential mulch." Or why he thinks that "[Fodor] wants the few critical features he assigns to the terms *modular* and *central* to be their only qualities." Quite aside from the use/mention confusion, this supposition is *grotesque.*

Whether or not the central processes are modular, might the integration of action be? I suspect (and suspected aloud in *Modularity*) that the answer might be yes. **Killeen**'s remarks confirm me in this suspicion.

On the other hand, it strikes me as clearly false that "mandatory operation seems but an extreme implication of encapsulation" (perhaps **Killeen** is failing to distinguish encapsulation vis à vis *information* from encapsulation vis-à-vis *utilities.* And the suggestion that Skinner is a modularity theorist simply boggles the mind!

Despite **Kinsbourne's** suggestion, there is, as far as I can see, next to no connection at all between the issues about "parallel" versus "stage" processing and the issues about modularity. Though it may be that "in the parallel model a subset of component outcomes [is] experienced in isolation" (whatever, exactly, that's supposed to mean), the results of the parallel computations *must* be made to interact somehow in constraining the perceptual analysis eventually achieved. Word recognition, for example, must constrain sentence recognition. You can't recognize an utterance as a token of "John bites" without recognizing that utterance as containing a token of "bites." In any event, the parallel/serial issues concern the character of *intra*modular computations, whereas the main issue – degree of encapsulation – is about the relation between modules and other processors. *Modularity* was explicitly agnostic about questions of the former sort.

What **Kinsbourne** says about neural architecture seems to me to be just what I say, only he says it in an optimistic tone of voice: that is, there is none for central processes, so far as anybody knows. On the other hand, "each *modality* [my emphasis] is now known to possess many point-to-point representations. . . ." What does Kinsbourne think we are disagreeing about?

I say to **Morton** what I say to **Glymour** and to **Kagan:** The issue is what one ought to idealize away from. Like Morton, I have to live with selective memory, contradictory beliefs, and irrationality. But I don't think that's what's at the heart of mentation. (It's not hard to build a stupid machine; what we're having trouble building is a *smart* one.) Most of the strategic part of science involves, in one way or other, getting the competence/performance distinction – the distinction between underlying regularities and surface perturbations.

I entirely agree that what's in the offing is a clash between "entire belief system[s]," including different evaluations of the magnitude (and the locus) of the success that cognitive science has achieved so far. There are well-known Kuhnian reasons for supposing that any science will experience this sort of upheavel from time to time. What's wrong with that? Does psychology *always* have to be boring?

If **Morton** has a plausible, nonequipotential neural model for belief fixation, could we please have a look at it? If he has an argument that identifying computational globality with nonmodularity is a confusion (or that "the neural net is, of course, the only device by which one could have even 'more or less' uninhibited passive access to the available data") could we please hear it? Or is it just that Morton is feeling a touch dyspeptic?

There is some confusion about **Rey**'s examples, a confusion for which, I'm afraid, my discussion in *Modarity* is partially to blame. Quineianism and isotropy – unlike globality – are, by definition, properties of mechanisms of belief fixation; and the difficulties we have in making models of them are intrinsic to this fact. Whether we understand elections and telephone systems is thus irrelevant to whether we are likely to understand central processes.

Anyhow, the way **Rey** views the problem is not unlike the way that **Glymour** does: What we should do is find local proxies for such global properties of belief systems as enter into estimates of confirmation. "This is not," however, "to say that it's easy to think of proxies . . . that will do the trick." And admittedly, we'll be in the soup if we can't, because "we simply don't seem to have a notion of nonlocal computation." We should, however, be of good cheer; the philosophy of science is in its "infancy" (it doesn't go back much beyond Aristotle). Surely something will turn up.

Rey's optimism sounds a lot like my pessimism. If this is what he's like when he's feeling cheerful, what is he like when he's blue?

Robinson says that it is "simply false" that utilities "cannot be inserted into even the earliest (receptor) stages of processing," but the only evidence he gives is that "we don't know enough . . . to assume that peripheral adaptations may [not?] occur" And then he accuses me of crossing "the line dividing supposition and legislation."

For the record: I think there is a lot of prima facie evidence for cognitive penetration of the putative modules. (New Look psychologists weren't irrational in holding the views they did – just misled). But I think most – maybe all – of that evidence can be explained away as postperceptual decision effects, as effects of information actually represented internal to the module, as effects of central process that duplicate modular ones (guessing in noisy situations for example), and so forth. Showing this in any detail will require a systematic reconsideration of the data that were alleged to support the continuity of perception with cognition; and it will require being more sophisticated about the interpretation of these data than New Look psychologists were wont to be. Whether such an undertaking can actually be brought off is anybody's guess; but it seems to me that there are now enough straws in the wind to warrant it.

Historico-pedantic footnote: actually, Reid's (1764) "direct realism" in perception comports quite badly with a *computational* account of faculties. His place in the history of the currently emerging faculty psychology is therefore unclear.

Schank & Hunter beg the question when they assume that the effect of information encapsulation is achieved by "indexing knowledge so that (only) relevant information becomes available at the right moment." I know of no serious proposal for such indexing. Neither, it appears, do Schank & Hunter, and they provide no argument that the mechanism of encapsulation is indexing rather than what I say it is: namely, the modularity of the processors.

I do *not* claim that "any system that makes reference to expectations cannot be modular." The question is whether the expectations referred to are represented *internal* to the module. (A grammar, for example, is a system of expectations about the structure of utterances; such a system is, in my view, accessed and exploited in speech perception. This is compatible with the informational encapsulation of parsing; on my view, the grammar must be internal to the speech processing module. All this was, I would have thought, spelled out pretty extensively in the book.)

Schank & Hunter provide no reason for believing their claim that "we have all sorts of expectations operating at every level of processing, even the lowest". Everything depends on considerations such as the locus of the effect (perceptual versus decision/postperceptual) and whether it is literally true that all sorts of expectations operate or whether only those that are represented module-internally do so. I'm afraid that unless psychologists of interactionist persuasion are prepared to exercise a degree of subtlety in their evaluation of such issues, the discussion of these really quite complicated issues isn't going to get very far.

Schank & Hunter's remarks about language strike me as unpersuasive, indeed, as unargued. I simply deny (what they simply assert) that "no utterance can make sense to an understander unless the understander knows something about the topic of the utterance." One way to construe making sense of an utterance is to assign satisfaction conditions to the utterance. The construction of algorithms for such assignments (including, as subsystems, algorithms for parsing) is one of the areas in which cognitive science – together with logic and formal semantics – seems to me to have made decisive progress. And it is unclear that much, indeed any, nonlinguistic information is accessible to such algorithms. Or again: Schank & Hunter make a claim for the nonexistence of "perceived ambiguity . . . in syntactically ambiguous but otherwise simple sentences," but I'm not sure what this claim is supposed to come to or why they think it's true. It can't mean that no one every notices the ambiguity of such sentences as "Everybody loves somebody," for that surely isn't true. Perhaps it means that syntactically ambiguous simple sentences in semantic context aren't more complicated than corresponding syntactically unambiguous sentences in those contexts. I don't know whether this is so, but let us suppose it is. Take-home exercise: Think up five models of parsing in which it is *not* semantically guided but which are compatible with this (presumptive) finding. Now think up another five. Now think about how hard it is to make good inferences from data to theory.

By the way, as *Modularity* remarks, the main problem about semantically guided syntactic parsing is how on earth it could be achieved, given the general noredundancy of form on content.

Schank & Hunter say that "the shift in emphasis to exploring the contents and processes of cognition" has produced "fascinating, albeit preliminary, results." But they don't say which results these are. They also think that I should be "thinking about methods" for solving the frame problem "instead of claiming that . . . it is impossible." I offer what I hope is a chastening observation: There are some problems that you can't solve because a basic idea is missing. In my view, for example, the problems about perception were unapproachable until Descartes (or somebody) hit on the idea of mental representation. Given that idea, it was possible to imagine how a mental event could be both causally implicated and semantically evaluable. Prior to that idea there was utter darkness; we couldn't even see with any clarity what problem it was that we wanted a theory of perception to solve. I think that not only are we lacking comparably basic ideas about confirmation (but also, by the way, about consciousness).

There isn't any way of finding basic ideas except by thinking (and thinking only works about once a century). Moral exhortation doesn't work, throwing money at the problem doesn't work, using bigger computers doesn't work – nothing works until somebody actually has the required idea. If I'm right that there's a basic idea that's lacking in our view of confirmation, then the rational thing to do is to work on questions where the general issue of confirmation doesn't arise. The modular processes appear to provide a plethora of such problems, so there's plenty to keep us busy. Let us, therefore, all give thanks.

I liked **Sternberg**'s piece, first because I'd never been compared to cheesecake before, *nutcake* having been the preferred culinary epithet, and second because, if I had to argue against modularity, I imagine I'd do it Sternberg's way: that is, by claiming that the apparent effects of modularity are actually the familiar effects of the novice-expert shift – of overlearning, in short. (**Schwartz,** by the way, makes a similar suggestion.)

But I don't believe a word of it. For example, **Sternberg** remarks that "the simple perceptual functions . . . are fully or largely automatized *by adulthood* [my emphasis]," thereby suggesting that they aren't automatized early on. Yet I know of nothing in the language development literature or in the literature on perceptual development that looks like a novice-to-expert transition (children are, as it were, always experts in the dialect of their developmental stage). Nor does it seem to me that the automatization of driving or solving physics problems is really much like the automatization of, say, phone perception. (However, reading – Sternberg's other example – may really be an intermediate case; see **Jusczyk & Cohen** on this.) To cite just one difference: You can, and do, bring driving and solving physics problems under close conscious control when your sense of the task suggests that it would be well to do so, for example, when it looks like a new kind of physics problem, or when there seems to be snow on the road. I see nothing in the (putatively) modular systems suggestive of this sort of penetration by appreciation of task demands. You can't voluntarily modulate your phone analysis procedures; the best you can do is try to pay more attention.

Clearly, however, the possibility that the appearance of modularity reduces to the effects of overlearning should be taken seriously; it suggests a line of research that rearguard interactionists might want to pursue. Conversely, one might consider the possibility that Mother Nature, having tried peripheral modular mechanisms and found them good, then contrived, via the novice-expert shift, to simulate some of the effects of modularity at the level of central systems. Mother Nature could thereby harvest the best fruits of both sorts of cognitive architecture while simultaneously sowing confusion among cognitive psychologists – two sorts of things that Mother Nature demonstrably likes to do.

Another reason why I liked the Sternberg piece was that it was the last one in the pile.

References

Ackerknecht, E. (1958) Contributions of Gall and the phrenologists to knowledge of brain function. In: *The brain and its function*, ed. F. N. L. Poynter. Blackwell. [CGG]

Ackerman, P. L., Schneider, W. & Wickens, C. D. (1984) Deciding the existence of a time-sharing ability: A combined methodological and theoretical approach. *Human Factors* 26(1):71–82. [EH]

Allport, D. A. (1977) On knowing the meaning of words we are unable to report: The effects of visual masking. In: *Attention and Performance 6*, ed. S. Dornic. Erlbaum. [MK]

Allport, G. W. (1937) *Personality: A psychological interpretation*. Holt. [CGG]

Anderson, J. R. (1983) *The architecture of cognition.* Harvard University Press. [rJAF, HG, EH, MSS]

Atkinson, R. C. & Shiffrin, R. M. (1968) Human memory: A proposed system and its control processes. In: *The psychology of learning and motivation,* vol. 2, ed. K. W. Spence & J. T. Spence. Academic Press. [BS]

Bekerian, D. A. & Bowers, J. M. (1983) Eyewitness testimony: Were we misled? *Journal of Experimental Psychology. Learning, Memory and Cognition* 9:139–145. [JM]

Borges, J. L. (1966) Pascal's sphere. In: *Other inquisitions.* Washington Square Press. [JCM]

Boring, E. G. (1950) *A history of experimental psychology.* 2d ed. Appleton-Century-Crofts. [CGG]

Breland, K. & Breland, M. (1966) *Animal behavior.* Macmillan. [PRK]

Bruner, J. (1973) On Perceptual Readiness. In: *Beyond the information given,* ed. J. Anglin. W. W. Norton & Co. [taJF]

Carpenter, G. A. & Grossberg, S. (1984) A neural theory of circadian rhythms: Split rhythms, after-effects, and motivational interactions. Submitted for publication. [SG]

Carroll, J. B. (1981) Ability and task difficulty in cognitive psychology. *Educational Researcher* 10:11–21. [RJS]

Cattell, R. B. (1971) *Abilities: Their structure, growth, and action.* Houghton Mifflin. [JBC]

Cheng, K. (1984) The primacy of metric properties in the rat's sense of place. Ph.D. dissertation, University of Pennsylvania. [CRG]

(in preparation) A purely geometric module in the rat's spatial representation. [CRG]

Chomsky, N. (1968) *Language and mind.* Harcourt Brace Jovanovich. [GR]

(1980) Rules and representation. *Behavioral and Brain Sciences* 3:1–62. [JM]

Clarke, E. & Dewhurst, K. (1972) *An illustrated history of brain function.* University of California Press. [CGG]

Coltheart, M., Patterson, K. & Marshall, J. C., eds. (1980) *Deep dyslexia.* Routledge and Kegan Paul. [MK]

Combe, G. (1824) Second dialogue between a philosopher of the old school and a phrenologist. *Phrenological Journal and Miscellany* 1:200–17. [JCM]

Corkin, S. (1968) Acquisition of motor skill after bilateral medial temporal-lobe excision. *Neuropsychologia* 6:255–65. [CGG]

Cowey, A. (1979) Cortical maps and visual perception. *Quarterly Journal of Experimental Psychology* 31:1–17. [MK]

Chain, S. & Fodor, J. D. (1984) How can grammars help parsers? In: *Natural language parsing: Psychological, computational and theoretical perspectives,* ed. D. R. Dowty, L. Karttunen & A. Zwicky. Cambridge University Press. [JDF]

Crawford, M. T. (1941) The cooperative solving by chimpanzees of problems requiring serial responses to color cues. *Journal of Social Psychology,* 13:259–80. [CRG]

Demos, E. V. (1974) Children's understanding in use of affect terms. Ph.D. dissertation. Harvard University. [JK]

Dewey, J. (1922) *Human nature and conduct.* Henry Holt and Co. [taJF]

Fahlman, S. A., Hinton, G. E. & Sejnowski, T. J. (1983) Massively parallel architectures for AI: NETL, Thistle, and Boltzmann machines. In: *Proceedings of the national conference on artificial intelligence.* National Conference on AI. [MSS]

Flanagan, O. (1984) *The science of the mind.* MIT/Bradford Books. [HG]

Flourens, P. (1846) *Phrenology examined.* Philadelphia: Hogan and Thompson. [JCM]

Fodor, J. A. (1965) *Psychological explanation.* Random House. [GR]

(1975) *The language of thought.* Thomas Crowell. [GR, PWJ]

(1981) *Representations.* MIT Press. [GR]

(1983) *The modularity of mind.* MIT Press [EH, MK, JCM]

(1984) Observation reconsidered. *Philosophy of Science* 51, 1:23–43. [taJF]

Fodor, J. A., Bever, T. G. & Garrett, M. F. (1974) *The psychology of language: An introduction to psycholinguistics and generative grammar.* McGraw-Hill. [rJAF, JDF]

Ford, M., Bresnan, J. & Kaplan, R. M. (1982) A competence-based theory of syntactic closure. In: *The mental representation of grammatical relations,* ed. J. Bresnan. MIT Press. [JDF]

Forster, K. I. (1979) Levels of processing and the structure of the language processor. In: *Sentence processing: Psycholinguistic studies presented to Merrill Garrett,* ed. W. E. Cooper & E. Walker. Erlbaum. [KIF, MSS]

(1981) Priming and the effects of sentence and lexical contexts on naming time: Evidence for autonomous lexical processing. *Quarterly Journal of Experimental Psychology* 33:465–95. [KIF]

Foss, D. J. (1982) A discourse on semantic priming. *Cognitive Psychology* 14:590–607. [MSS]

Frazier, L. (1978) *On comprehending sentences: Syntactic parsing strategies,* Ph.D. dissertation, University of Connecticut. [JDF]

Frazier, L., Clifton, C. & Randall, J. (1983) Filling gaps: Decision principles and structure in sentence comprehension. *Cognition* 13:187–222. [JDF]

Frazier, L. & Fodor, J. D. (1978) The sausage machine: A new two-stage parsing model. *Cognition* 6:291–325. [JDF]

Gallistel, C. R. (1980) *The organization of action: A new synthesis.* Erlbaum. [PRK]

Gardner, H. (1983) *Frames of mind: The theory of multiple intelligences.* Basic Books. [HG, EH, JM]

Goodman, K. S. (1970) Reading: A psycholinguistic guessing game. In: *Theoretical models and processes of reading,* ed. H. Sinder & R. B. Ruddell. International Reading Association. [MSS]

Gregory, R. (1970) *The intelligent eye.* McGraw-Hill. [taJF]

Gross, G. (1973) Visual functions of inferotemporal cortex. In: *Handbook of sensory physiology,* vol. 7, Pt. 3B, ed. R. Jung. Springer-Verlag. [CGG]

Grossberg, S. (1980) How does a brain build a cognitive code? *Psychological Review,* 87:1–51. [SG]

(1982a) The processing of expected and unexpected events during conditioning and attention: A psychophysiological theory. *Psychological Review* 89:529–72. [SG]

(1982b) *Studies of mind and brain: Neural principles of learning, perception, development, cognition, and motor control.* Reidel Press. [SG]

(1983) The quantized geometry of visual space: The coherent computation of depth, form, and lightness. *Behavioral and Brain Sciences* 6:625–92 [SG]

(1984a) Some psychophysiological and pharmacological correlates of a developmental, cognitive, and motivational theory. In: *Brain and information: Event related potentials,* ed. R. Karrer, J. Cohen, & P. Tueting. New York Academy of Sciences. [SG]

(1984b) Some normal and abnormal behavioral syndromes due to transmitter gating of opponent processes. *Biological Psychiatry,* in press. [SG]

(1984c) Unitization, automaticity, temporal order, and word recognition. *Journal of Experimental Psychology: Human Perception and Performance,* in press. [SG]

Head, H. (1926) *Aphasia and kindred disorders of speech.* Cambridge University Press. [CGG, HG]

Hearst, E. & Jenkins, H. M. (1974) *Sign-tracking: The stimulus-reinforcer relation and directed action.* The Psychonomic Society. [PRK]

Hebb, D. O. (1949). *The organization of behavior.* Wiley. [KIF]

Hirst, W., Spelke, E. S., Reaves, G. C., Caharack, G. & Neisser, U. (1980) Dividing attention without alternation or automaticity. *Journal of Experimental Psychology: General* 109:98–117. [EH]

Horn, J. L. (1978) Human ability systems. In: *Life-span development and behavior,* vol. 1, ed. P. B. Baltes, Academic Press. [JBC]

Hunt, E. (1978) Mechanics of verbal ability. *Psychological Review* 85(2):109–30. [EH]

(1980) Intelligence as an information-processing concept. *British Journal of Psychology* 71:449–74. [RJS]

(1983) On the nature of intelligence. *Science* 219:141–46. [EH]

Hunt, E., Davidson, J. & Lansman, M. (1981) Individual differences in long term memory access. *Memory and Cognition* 9(6):599–608. [EH]

Jacoby, L. L. (1983) Perceptual enhancement: Persistent effects of an experience. *Journal of Experimental Psychology: Learning, Memory and Cognition* 9:21–38. [JM]

Johnson-Laird, P. N. (1980) Mental models in cognitive science. *Cognitive Science* 4:71–115. [JM]

(1983) *Mental models: Towards a cognitive science of language, inference and consciousness.* Cambridge University Press. [JM]

Kagan, J. (1981) *The second year.* Harvard University Press. [JK]

Kahneman, D., Slovic, P. & Tiersky, A. (1982) *Judgment under uncertainty: Heuristics and biases.* Cambridge University Press. [JM]

Kant, I. (1787/1966) *Critique of pure reason.* Trans. F. M. Müller. Anchor Books. [GR]

Katz, J. & Fodor, J. A. (1963) The structure of a semantic theory. *Language* 39:170–210. [KIF]

Kerr, F. B., Condon, S. & McDonald, L. A. (1983) Cognitive spatial processing and the regulation of posture. *Proceedings of the Psychomic Society Annual Meeting* (abstract). [EH]

Kimball, J. (1973) Seven principles of surface structure parsing in natural language. *Cognition* 2:15–47. [JDF]

Krueger, J. & Osherson, D. (1980) On the psychology of structural similarity. In: *Nature of thought: Essays in honor of D. Hebb,* ed. P. W. Juscyzk & R. M. Klein. Erlbaum. [GR]

Kuhn, T. (1970) *The structure of scientific revolutions.* University of Chicago Press. [BS]

LaBerge, D. & Samuels, S. J. (1974) Toward a theory of automatic information processing in reading. *Cognitive Psychology* 6:293–323. [BS]

Lansman, M., Poltrock, S. E. & Hunt, E. (1983) Individual differences in the ability to focus and divide attention. *Intelligence* 7(3):299–312. [EH]

Lashley, K. S. (1951) The problem of serial order in behavior. In: *Cerebral mechanisms and behavior*, ed. L. A. Jeffress. Wiley. [PRK]

Lee, D. N. & Reddish, P. E. (1981). Plummeting gannets: A paradigm of ecological optics. *Nature* 293:293–94. [IGM]

Leeper, R. (1935) A study of neglected portion of learning – the development of sensory organization. *Journal of Genetic Psychology* 46:41–75. [SG]

Lewes, G. H. (1867, 1871) *The history of philosophy from Thales to Comte*. Longmans. [CGG]

Liberman, A. M., Isenberg, D. & Rakerd, B. (1981). Duplex perception of cues for stop consonants: Evidence for a phonetic mode. *Perception & Psychophysics* 30:133–43. [IGM]

Luria, A. R. (1966) *The higher cortical functions in man*. Basic Books. [HG, MK]

McCarthy, J. & Hayes, P. (1969) Some philosophical problems from the standpoint of artificial intelligence. In: *Machine intelligence*, ed. B. Meltzer & D. Michie. Elsevier. [BS]

McClelland, J. & Rumelhart, D. M. (1981) An interactive-activation model of context effects in letter perception. Part I: An account of basic findings. *Psychological Review* 88:375–407. [MSS]

McNemar, Q. (1964) Lost: Our intelligence? Why?. *American Psychologist* 19:871–82. [EH]

Mann, V. A. & Liberman, A. M. (1983). Some differences between phonetic and auditory modes of perception. *Cognition* 14:211–35. [IGM]

Marcel, A. J. (1974) Perception with and without awareness. Paper to the Experimental Psychology Society, Stirling. [MK]

Marcus, M. P. (1980) *A theory of syntactic recognition for natural language* MIT Press. [JDF]

Marshalek, B., Lohman, D. F. & Snow, R. E. (1983) The complexity continuum in the radex and hierarchial models of intelligence. *Intelligence* 7:107–28. [EH]

Marshall, J. C. (1980) On the biology of language acquisition. In: *Biological studies of mental processes*, ed. D. Caplan. MIT Press. [JCM]

(1984) Multiple perspectives on modularity. *Cognition* 7, in press. [JCM]

Meadows, J. C. (1974) Disturbed perception of colours associated with localized cerebral lesions. *Brain* 97:615–32. [CGG]

Mehler, J., Morton J. & Jusczyk, P. W. (1984) On reducing language to biology. *Cognitive Neuropsychology* 1:82–116. [JM, PWJ]

Merzenich, M. & Kaas, J. (1980) Principles of organization of sensory-perceptual systems in mammals. *Progress in Psychobiology and Physiological Psychology* 9:1–42. [MK]

Moore, B. R. (1973) The role of directed Pavlovian reactions in simple instrumental learning in the pigeon. In: *Constraints on Learning*, ed. R. A. Hinde & J. Hinde. Academic Press. [PRK]

Morton, J. & Patterson, K. (1980). A new attempt at an interpretation, or, an attempt at a new interpretation. In: *Deep dyslexia*, ed. M. Coltheart, K. Patterson & J. Marshall. Routledge and Kegan Paul. [JM]

Murray, E. A. & Mishkin, M. (1982) Amygdalectomy but not hippocampectomy impairs cross-modal delayed non-matching-to-sample in monkeys. *Neuroscience Abstracts* 8:23. [CGG]

Newcombe, F. & Marshall, J. C. (1981). On psycholinguistic classifications of the acquired dyslexias. *Bulletin of the Orton Society* 31:29–46. [JM]

Newell, A. (1982) The knowledge level. *Artificial Intelligence* 18:87–127. [BS]

Norman, D. A. (1981) Categorization of action slips. *Psychological Review* 88:1–15. [PRK]

Palmer, J. C., MacLeod, C. M., Hunt, E. & Davidson, J. (1984) Information processing correlates of reading. *Journal of Verbal Learning and Verbal Behavior*, in press. [EH]

Parkinson, J. K. & Mishkin, M. (1982) A selective mnemonic role for the hippocampus in monkeys: Memory for the location of objects. *Neuroscience Abstracts* 8:23. [CGG]

Pellegrino, J. W. & R. Glaser. (1980) Components of inductive reasoning. In: *Aptitude, learning, and instruction*, vol. 1, *Cognitive process analyses of aptitude*, ed. R. E. Snow, P.-A. Federico & W. Montague. Erlbaum. [RJS]

Pierce, C. S. (1901/1955) Abduction and induction. In: *Philosophical writings of Pierce*, ed. J. Buchler. Dover. Orig. pub. 1901. [GR]

Popper, K. R. & Eccles, J. C. (1977) *The self and its brain*. Springer-Verlag. [JCM]

Posner, M. (1978) *Chronometric studies of mind*. Erlbaum. [MK]

Posner, M. L. & Snyder, C. R. R. (1975) Attention and cognitive control. In: *Information processing and cognition*, ed. R. L. Solso. Erlbaum. [KIF, BS]

Rabinowicz, T. (1979) The differentiate maturation of the human cerebral cortex. In: *Human Growth*, vol. 3, ed. F. Falkner & J. M. Tanner. Plenum. [JK]

Rand, T. C. (1974). Dichotic release from masking for speech. *Journal of the Acoustical Society of America* 55:678–80. [IGM]

Ratcliff, J. (1983) Inference processes in the early stages of sentence comprehension: A study of the plausibility effect. Ph.D. dissertation, Monash University. [KIF]

Reid, Thomas. (1764) *An Inquiry into the human mind*. [JAF, DNR]

Remez, R. E., Rubin, P. E. Pisoni, D. B. & Carrell, T. D. (1981). Speech perception without traditional speech cues. *Science* 212:947–50. [IGM]

Repp, B. H., Milburn, C. & Ashkenas, J. (1983). Duplex perception: Confirmation of fusion. *Perception & Psychophysics* 33:333–37. [IGM]

Ribot, T. (1906) *Diseases of memory*. 5th Ed. Regan, Paul, Trench, Trubner, [CGG]

Robinson, D. N. (1976a; rev. ed. 1981) *An intellectual history of psychology*. Macmillan. [DNR]

(1976b) Thomas Reid's *Gestalt* psychology. *Philosophical Monographs* 3:44–54. [DNR]

(1979) *Systems of modern psychology: A critical sketch*. Columbia University Press. [DNR]

Robinson, D. N. & Beauchamp, T. L. (1978) Personal identity: Reid's answer to Hume. *The Monist* 61:325–39. [DNR]

Rock, I. (1983) *The logic of perception*. MIT Press. [taJF]

Rozin, P. (1976) The evolution of intelligence and access to the cognitive unconscious. *Progress in Psychobiology and Physiological Psychology* 6:245–80. [HG]

Rumelhart, D. E. (1980) Schemata: The building blocks of cognition. In: *Theoretical issues in reading comprehension*, ed. R. Spiro, B. Bruce & W. Brewer. Erlbaum. [JM]

Salmon, W. (1971) *Statistical explanation and statistical relevance*. University of Pittsburgh Press. [CGG]

Schank, R. C. (1972) Conceptual dependence: A theory of natural language understanding. *Cognitive Psychology* 3:552–631. [HG]

(1982) *Dynamic memory: A theory of reminding in computers and people*. Cambridge University Press. [RCS]

Schwartz, M. F. & Schwartz, B. (1984) In defense of organology: Jerry Fodor's *Modularity of mind*. *Cognitive Neuropsychology* 1:25–42. [BS]

Seidenberg, M. S. (in press) The time course of information activation and utilization in visual word recognition. In: *Reading research: Advances in theory and practice*, vol. 5, ed. D. Besner, T. G. Waller & G. E. MacKinnon. Academic Press. [MSS]

Seidenberg, M. S. & Tanenhaus, M. K. (in press). Modularity and lexical access. In: *Studies in cognitive science: Papers from the McGill workshops*, ed. I. Gopnik. Ablex. [rJAF, MSS]

Seidenberg, M. S., Waters, G. S., Sanders, M. & Langer, P. (1984) Pre- and post-lexical loci of contextual effects on word recognition. *Memory & Cognition* 12:315–28. [MSS]

Sellers, M. J. (1979) *The enhancement of memory in Costa Rican children*. Ph.D. dissertation. Harvard University. [JK]

Seymour, P. H. K. (1979) *Human visual cognition*. Collier Macmillan. [JM]

Shallice, T. (1981) Neurological impairment of cognitive processes. *British Medical Journal* 37:187–92. [JM]

Shettleworth, S. J. (1980) Reinforcement and the organization of behavior in golden hamsters: Brain stimulation reinforcement for seven action patterns. *Journal of Experimental Psychology: Animal Behavior Processes* 6:352–75. [PRK]

Shiffrin, R. & Schneider, W. (1977) Controlled and automatic human information processing: 2. Perceptual learning, automatic attending and a general theory. *Psychological Review* 84:127–90. [PWJ]

Skinner, B. F. (1935; 1972) The generic nature of stimulus and response. Reprinted in *Cumulative Record* (3rd ed). Appleton-Century-Crofts. [PRK]

Spearman, C. (1927) *The abilities of man*. Macmillan. [EH]

Spencer, H. (1851) *Social studies*. Chapman. [CGG]

Sperry, R. W. (1945). The problem of central nervous reorganization after nerve regeneration and muscle transposition: A critical review. *Quarterly Review of Biology* 20:311–69. [PRK]

Spurzheim, J. G. (1934) *Phrenology or the doctrine of the mental phenomena*. 3d Amer. Ed. Marsh, Capen and Lyon. [CGG]

Staddon, J. E. R. (1983) *Adaptive behavior and learning*. Cambridge University Press. [PRK]

Stanovich, K. E. (1980) Toward an interactive-compensatory model of individual differences in the development of reading fluency. *Reading Research Quarterly* 16:32–71. [EH, MSS]

Sternberg, R. J. (1981) The evolution of theories of intelligence. *Intelligence* 5:209–30. [RJS]

(1982) Reasoning, problem solving, and intelligence. In: *Handbook of human intelligence*, ed. R. J. Sternberg, Cambridge University Press. [JBC]

(1984). Toward a triarchic theory of human intelligence. *Behavioral and Brain Sciences* 7:269–315. [RJS]
Sternberg, R. J. & Powell, J. S. (1983) Comprehending verbal comprehension. *American Psychologist* 38:878–93. [EH]
Swinney, D. (1979) Lexical access during sentence comprehension: (Re)consideration of context effects. *Journal of Verbal Learning and Verbal Behavior* 18:645–59. [KIF]
Thurstone, L. L. (1947) *Multiple-factor analysis*. University of Chicago Press. [JBC]
Tinklepaugh, O. L. (1932) Multiple delayed reactions with chimpanzees and monkeys. *Journal of Comparative Psychology* 13:207–43. [CRG]
Turvey, M. T., Shaw, R. E., Reed, E. S., Mace, W. M. (1981) Ecological laws of perceiving and acting. In reply to Fodor and Pylyshyn. *Cognition* 5:237–304. [HG]
Tversky, A. & Kahneman, D. (1981) The framing of decisions and the psychology of choice. *Science* 211:453–58. [BS]
Valenstein, E. S. (1973) *Brain stimulation and motivation: Research and commentary*. Harper & Row. [PRK]
von der Heydt, R., Peterhans, E. & Baumgartner, G. (1984) Illusory contours and cortical neuron responses. *Science* 224:1260–62. [SG]
Wagner, S., et al. (1981). Metaphorical mapping in human infants. *Child Development* 52:728–31. [JK]
Wanner, E. & Maratsos, M. (1978) An ATN approach to comprehension. In: *Linguistic theory and psychological reality*, ed. M. Halle, J. Bresnan & G. A. Miller. MIT Press. [JDF]
Warren, R. (1970) Perceptual restoration of missing speech sounds. *Science* 167:392–93. [DC]
Weiss, P. (1941) Self-differentiation of the basic patterns of coordination. *Comparative Psychology Monographs* 17(4). [PRK]
Wong, E. & Weisstein, N. (1983) Sharp targets are detected better against a figure and blurred targets are detected better against a background. *Journal of Experimental Psychology: Human Perception and Performance* 9:194–202. [EH]
Woodger, J. H. (1952). *Biology and language*. Cambridge University Press. [JK]
Young, R. M. (1970) *Mind, brain and adaptation in the nineteenth century*. Clarendon. [CGG, JCM]
Zajonc, R. B. (1980) Feeling and thinking: Preferences need no inferences. *American Psychologist* 35:151–75. [MK]
Zeki, S. M. (1978) The cortical projections of foveal striate cortex in the rhesus monkey. *Journal of Psychology* 277:227–44. [MK]
Zihl, J., Von Cramon, D. & Mai, N. (1983) Selective disturbance of movement vision after bilateral brain damage. *Brain* 106:313–40. [CGG]