linguistic 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