3. The need for symbol systems: Productivity, systematicity, compositionality and inferential coherence

Classical psychological theories appeal to the constituent structure of mental representations to explain three closely related features of cognition: its productivity, its compositionality and its inferential coherence. The traditional argument has been that these features of cognition are, on the one hand, pervasive and, on the other hand, explicable only on the assumption that mental representations have internal structure. This argument—familiar in more or less explicit versions for the last thirty years or so— is still intact, so far as we can tell. It appears to offer something close to a demonstration that an empirically adequate cognitive theory must recognize not just causal relations among representational states but also relations of syntactic and semantic constituency; hence that the mind cannot be, in its general structure, a Connectionist network.

3.1. Productivity of thought

There is a classical productivity argument for the existence of combinatorial structure in any rich representational system (including natural languages and the language of thought). The representational capacities of such a system are, by assumption, unbounded under appropriate idealization; in particular, there are indefinitely many propositions which the system can encode.[22] However, this unbounded expressive power must presumably be achieved by finite means. The way to do this is to treat the system of representations as consisting of expressions belonging to a generated set. More precisely, the correspondence between a representation and the proposition it expresses is, in arbitrarily many cases, built up recursively out of correspondences between parts of the expression and parts of the proposition. But, of course, this strategy can operate only when an unbounded number of the expressions are non-atomic. So linguistic (and mental) representations must constitute *symbol systems* (in the sense of footnote 8). So the mind cannot be a PDP.

Very often, when people reject this sort of reasoning, it is because they doubt that human cognitive capacities are correctly viewed as productive. In

[22]This way of putting the productivity argument is most closely identified with Chomsky (e.g., Chomsky, 1965; 1968). However, one does not have to rest the argument upon a basic assumption of infinite generative capacity. Infinite generative capacity can be viewed, instead, as a consequence or a corollary of theories formulated so as to capture the greatest number of generalizations with the fewest independent principles. This more neutral approach is, in fact, very much in the spirit of what we shall propose below. We are putting it in the present form for expository and historical reasons.

the long run there can be no a priori arguments for (or against) idealizing to productive capacities; whether you accept the idealization depends on whether you believe that the inference from finite performance to finite capacity is justified, or whether you think that finite performance is typically a result of the interaction of an unbounded competence with resource constraints. Classicists have traditionally offered a mixture of methodological and empirical considerations in favor of the latter view.

From a methodological perspective, the least that can be said for assuming productivity is that it precludes solutions that rest on inappropriate tricks (such as storing all the pairs that define a function); tricks that would be unreasonable in practical terms even for solving finite tasks that place sufficiently large demands on memory. The idealization to unbounded productive capacity forces the theorist to separate the finite specification of a method for solving a computational problem from such factors as the resources that the system (or person) brings to bear on the problem at any given moment.

The empirical arguments for productivity have been made most frequently in connection with linguistic competence. They are familiar from the work of Chomsky (1968) who has claimed (convincingly, in our view) that the knowledge underlying linguistic competence is generative—i.e., that it allows us *in principle* to generate (/understand) an unbounded number of sentences. It goes without saying that no one does, or could, *in fact* utter or understand tokens of more than a finite number of sentence types; this is a trivial consequence of the fact that nobody can utter or understand more than a finite number of sentence tokens. But there are a number of considerations which suggest that, despite de facto constraints on performance, ones knowledge of ones language supports an unbounded productive capacity in much the same way that ones knowledge of addition supports an unbounded number of sums. Among these considerations are, for example, the fact that a speaker/hearer's performance can often be improved by relaxing time constraints, increasing motivation, or supplying pencil and paper. It seems very natural to treat such manipulations as affecting the transient state of the speaker's memory and attention rather than what he knows about—or how he represents—his language. But this treatment is available only on the assumption that the character of the subject's performance is determined by interactions between the available knowledge base and the available computational resources.

Classical theories are able to accommodate these sorts of considerations because they assume architectures in which there is a functional distinction between memory and program. In a system such as a Turing machine, where the length of the tape is not fixed in advance, changes in the amount of available memory *can be affected without changing the computational structure*

of the machine; viz., by making more tape available. By contrast, in a finite state automaton or a Connectionist machine, adding to the memory (e.g., by adding units to a network) alters the connectivity relations among nodes and thus does affect the machine's computational structure. Connectionist cognitive architectures cannot, by their very nature, support an expandable memory, so they cannot support productive cognitive capacities. The long and short is that if productivity arguments are sound, then they show that the architecture of the mind can't be Connectionist. Connectionists have, by and large, acknowledged this; so they are forced to reject productivity arguments.

The test of a good scientific idealization is simply and solely whether it produces successful science in the long term. It seems to us that the productivity idealization has more than earned its keep, especially in linguistics and in theories of reasoning. Connectionists, however, have not been persuaded. For example, Rumelhart and McClelland (1986a, p. 119) say that they "... do not agree that [productive] capabilities are of the essence of human computation. As anyone who has ever attempted to process sentences like 'The man the boy the girl hit kissed moved' can attest, our ability to process even moderate degrees of center-embedded structure is grossly impaired relative to an ATN [Augmented Transition Network] parser .... What is needed, then, is not a mechanism for flawless and effortless processing of embedded constructions ... The challenge is to explain how those processes that others have chosen to explain in terms of recursive mechanisms can be better explained by the kinds of processes natural for PDP networks."

These remarks suggest that Rumelhart and McClelland think that the fact that center-embedding sentences are hard is somehow an *embarrassment* for theories that view linguistic capacities as productive. But of course it's not since, according to such theories, performance is an effect of interactions between a productive competence and restricted resources. There are, in fact, quite plausible Classical accounts of why center-embeddings ought to impose especially heavy demands on resources, and there is a reasonable amount of experimental support for these models (see, for example, Wanner & Maratsos, 1978).

In any event, it should be obvious that the difficulty of parsing center-embeddings can't be a consequence of their recursiveness per se since there are many recursive structures that are strikingly easy to understand. Consider: 'this is the dog that chased the cat that ate the rat that lived in the house that Jack built.' The Classicist's case for productive capacities in parsing rests on the transparency of sentences like these.[23] In short, the fact that center-em-

[23]McClelland and Kawamoto (1986) discuss this sort of recursion briefly. Their suggestion seems to be that parsing such sentences doesn't really require recovering their recursive structure: "... the job of the parser

bedded sentences are hard perhaps shows that there are some recursive structures that we can't parse. But what Rumelhart and McClelland need if they are to deny the productivity of linguistic capacities is the much stronger claim that there are no recursive structures that we can parse; and this stronger claim would appear to be simply false.

Rumelhart and McClelland's discussion of recursion (pp. 119–120) nevertheless repays close attention. They are apparently prepared to concede that PDPs can model recursive capacities only indirectly—viz., by implementing Classical architectures like ATNs; so that *if* human cognition exhibited recursive capacities, that would suffice to show that minds have Classical rather than Connectionist architecture at the psychological level. "We have not dwelt on PDP implementations of Turing machines and recursive processing engines *because we do not agree with those who would argue that such capacities are of the essence of human computation*" (p. 119, our emphasis). Their argument that recursive capacities *aren't* "of the essence of human computation" is, however, just the unconvincing stuff about center-embedding quoted above.

So the Rumelhart and McClelland view is apparently that if you take it to be independently obvious that some cognitive capacities are productive, then you should take the existence of such capacities to argue for Classical cognitive architecture and hence for treating Connectionism as at best an implementation theory. We think that this is quite a plausible understanding of the bearing that the issues about productivity and recursion have on the issues about cognitive architecture; in Section 4 we will return to the suggestion that Connectionist models can plausibly be construed as models of the implementation of a Classical architecture.

In the meantime, however, we propose to view the status of productivity arguments for Classical architectures as moot; we're about to present a different sort of argument for the claim that mental representations need an articulated internal structure. It is closely related to the productivity argument, but it doesn't require the idealization to unbounded competence. Its assumptions

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[with respect to right-recursive sentences] is to spit out phrases in a way that captures their *local* context. Such a representation may prove sufficient to allow us to reconstruct the correct bindings of noun phrases to verbs and prepositional phrases to *nearby* nouns and verbs" (p. 324; emphasis ours). It is, however, by no means the case that all of the semantically relevant grammatical relations in readily intelligible embedded sentences are local in surface structure. Consider: '*Where* did the man who owns the cat that chased the rat that frightened the girl say that he was going to move to (X)?' or '*What* did the girl that the children loved to listen to promise your friends that she would read (X) to them?' Notice that, in such examples, a binding element (italicized) can be arbitrarily displaced from the position whose interpretation it controls (marked 'X') without making the sentence particularly difficult to understand. Notice too that the 'semantics' doesn't determine the binding relations in either example.

should thus be acceptable even to theorists who—like Connectionists—hold that the finitistic character of cognitive capacities is intrinsic to their architecture.

3.2. Systematicity of cognitive representation

The form of the argument is this: Whether or not cognitive capacities are really *productive*, it seems indubitable that they are what we shall call 'systematic'. And we'll see that the systematicity of cognition provides as good a reason for postulating combinatorial structure in mental representation as the productivity of cognition does: You get, in effect, the same conclusion, but from a weaker premise.

The easiest way to understand what the systematicity of cognitive capacities amounts to is to focus on the systematicity of language comprehension and production. In fact, the systematicity argument for combinatorial structure in *thought* exactly recapitulates the traditional Structuralist argument for constituent structure in sentences. But we pause to remark upon a point that we'll re-emphasize later; linguistic capacity is a paradigm of systematic cognition, but it's wildly unlikely that it's the only example. On the contrary, there's every reason to believe that systematicity is a thoroughly pervasive feature of human and infrahuman mentation.

What we mean when we say that linguistic capacities are *systematic* is that the ability to produce/understand some sentences is *intrinsically* connected to the ability to produce/understand certain others. You can see the force of this if you compare learning languages the way we really do learn them with learning a language by memorizing an enormous phrase book. The point isn't that phrase books are finite and can therefore exhaustively specify only *non*-productive languages; that's true, but we've agreed not to rely on productivity arguments for our present purposes. Our point is rather that you can learn *any part of a phrase book without learning the rest.* Hence, on the phrase book model, it would be perfectly possible to learn that uttering the form of words 'Granny's cat is on Uncle Arthur's mat' is the way to say (in English) that Granny's cat is on Uncle Arthur's mat, and yet have no idea at all how to say that it's raining (or, for that matter, how to say that Uncle Arthur's cat is on Granny's mat). Perhaps it's self-evident that the phrase book story must be wrong about language acquisition because a speaker's knowledge of his native language is never like that. You don't, for example, find native speakers who know how to say in English that John loves the girl but don't know how to say in English that the girl loves John.

Notice, in passing, that systematicity is a property of the mastery of the syntax of a language, not of its lexicon. The phrase book model really does

fit what it's like to learn the *vocabulary* of English since when you learn English vocabulary you acquire a lot of basically *independent* capacities. So you might perfectly well learn that using the expression 'cat' is the way to refer to cats and yet have no idea that using the expression 'deciduous conifer' is the way to refer to deciduous conifers. Systematicity, like productivity, is the sort of property of cognitive capacities that you're likely to miss if you concentrate on the psychology of learning and searching lists.

There is, as we remarked, a straightforward (and quite traditional) argument from the systematicity of language capacity to the conclusion that sentences must have syntactic and semantic structure: If you assume that sentences are constructed out of words and phrases, and that many different sequences of words can be phrases of the same type, the very fact that one formula is a sentence of the language will often imply that other formulas must be too: in effect, systematicity follows from the postulation of constituent structure.

Suppose, for example, that it's a fact about English that formulas with the constituent analysis 'NP Vt NP' are well formed; and suppose that 'John' and 'the girl' are NPs and 'loves' is a Vt. It follows from these assumptions that 'John loves the girl,' 'John loves John,' 'the girl loves the girl,' and 'the girl loves John' must all be sentences. It follows too that anybody who has mastered the grammar of English must have linguistic capacities that are systematic in respect of these sentences; he *can't but* assume that all of them are sentences if he assumes that any of them are. Compare the situation on the view that the sentences of English are all atomic. There is then no structural analogy between 'John loves the girl' and 'the girl loves John' and hence no reason why understanding one sentence should imply understanding the other; no more than understanding 'rabbit' implies understanding 'tree'.[24]

On the view that the sentences are atomic, the systematicity of linguistic capacities is a mystery; on the view that they have constituent structure, the systematicity of linguistic capacities is what you would predict. So we should prefer the latter view to the former.

Notice that you can make this argument for constituent structure in sentences without idealizing to astronomical computational capacities. There are productivity arguments for constituent structure, but they're concerned with our ability—in principle—to understand sentences that are arbitrarily long. Systematicity, by contrast, appeals to premises that are much nearer home;

[24]See Pinker (1984, Chapter 4) for evidence that children never go through a stage in which they distinguish between the internal structures of NPs depending on whether they are in subject or object position; i.e., the dialects that children speak are always systematic with respect to the syntactic structures that can appear in these positions.

such considerations as the ones mentioned above, that no speaker understands the form of words 'John loves the girl' except as he also understands the form of words 'the girl loves John'. The assumption that linguistic capacities are productive "in principle" is one that a Connectionist might refuse to grant. But that they are systematic *in fact* no one can plausibly deny.

We can now, finally, come to the point: the argument from the systematicity of linguistic capacities to constituent structure in sentences is quite clear. *But thought is systematic too*, so there is a precisely parallel argument from the systematicity of thought to syntactic and semantic structure in mental representations.

What does it mean to say that thought is systematic? Well, just as you don't find people who can understand the sentence 'John loves the girl' but not the sentence 'the girl loves John,' so too you don't find people who can *think the thought* that John loves the girl but can't think the thought that the girl loves John. Indeed, in the case of verbal organisms the systematicity of thought *follows from* the systematicity of language if you assume—as most psychologists do—that understanding a sentence involves entertaining the thought that it expresses; on that assumption, nobody *could* understand both the sentences about John and the girl unless he were able to think both the thoughts about John and the girl.

But now if the ability to think that John loves the girl is intrinsically connected to the ability to think that the girl loves John, that fact will somehow have to be explained. For a Representationalist (which, as we have seen, Connectionists are), the explanation is obvious: Entertaining thoughts requires being in representational states (i.e., it requires tokening mental representations). And, just as the systematicity of language shows that there must be structural relations between the sentence 'John loves the girl' and the sentence 'the girl loves John,' so the systematicity of thought shows that there must be structural relations between the mental representation that corresponds to the thought that John loves the girl and the mental representation that corresponds to the thought that the girl loves John;[25] namely, the two mental representations, like the two sentences, *must be made of the same parts*. But if this explanation is right (and there don't seem to be any others on offer), then mental representations have internal structure and there is a

[25]It may be worth emphasizing that the structural complexity of a mental representation is not the same thing as, and does *not* follow from, the structural complexity of its propositional content (i.e., of what we're calling "the thought that one has"). Thus, Connectionists and Classicists can agree to agree that *the thought that P&Q* is complex (and has the thought that *P* among its parts) while agreeing to disagree about whether mental representations have internal syntactic structure.

language of thought. So the architecture of the mind is not a Connectionist network.[26]

To summarize the discussion so far: Productivity arguments infer the internal structure of mental representations from the presumed fact that nobody has a *finite* intellectual competence. By contrast, systematicity arguments infer the internal structure of mental representations from the patent fact that nobody has a *punctate* intellectual competence. Just as you don't find linguistic capacities that consist of the ability to understand sixty-seven unrelated sentences, so too you don't find cognitive capacities that consist of the ability to think seventy-four unrelated thoughts. Our claim is that this isn't, in either case, an accident: A linguistic theory that allowed for the possibility of punctate languages would have gone not just wrong, but *very profoundly* wrong. And similarly for a cognitive theory that allowed for the possibility of punctate minds.

But perhaps not being punctate is a property only of the minds of language users; perhaps the representational capacities of infraverbal organisms do have just the kind of gaps that Connectionist models permit? A Connectionist might then claim that he can do everything "up to language" on the assumption that mental representations lack combinatorial syntactic and semantic structure. Everything up to language may not be everything, but it's a lot. (On the other hand, a lot may be a lot, but it isn't everything. Infraverbal cognitive architecture mustn't be so represented as to make the eventual acquisition of language in phylogeny and in ontogeny require a miracle.)

It is not, however, plausible that only the minds of verbal organisms are systematic. Think what it would mean for this to be the case. It would have to be quite usual to find, for example, animals capable of representing the state of affairs $a\mathbf{R}b$, but incapable of representing the state of affairs $b\mathbf{R}a$. Such animals would be, as it were, $a\mathbf{R}b$ sighted but $b\mathbf{R}a$ blind since, presumably, the representational capacities of its mind affect not just what an or-

[26]These considerations throw further light on a proposal we discussed in Section 2. Suppose that the mental representation corresponding to the thought that John loves the girl is the feature vector {*+John-subject; +loves; +the-girl-object*} where '*John-subject*' and '*the-girl-object*' are atomic features; as such, they bear no more structural relation to '*John-object*' and '*the-girl-subject*' than they do to one another or to, say, '*has-a-handle*'. Since this theory recognizes no structural relation between '*John-subject*' and '*John-object*', it offers no reason why a representational system that provides the means to express one of these concepts should also provide the means to express the other. This treatment of role relations thus makes a mystery of the (presumed) fact that anybody who can entertain the thought that John loves the girl can also entertain the thought that the girl loves John (and, mutatis mutandis, that any natural language that can express the proposition that John loves the girl can also express the proposition that the girl loves John). This consequence of the proposal that role relations be handled by "role specific descriptors that represent the conjunction of an identity and a role" (Hinton, 1987) offers a particularly clear example of how failure to postulate internal structure in representations leads to failure to capture the systematicity of representational systems.

ganism can think, but also what it can perceive. In consequence, such animals would be able to learn to respond selectively to *a*R*b* situations but quite *un*able to learn to respond selectively to *b*R*a* situations. (So that, though you could teach the creature to choose the picture with the square larger than the triangle, you couldn't for the life of you teach it to choose the picture with the triangle larger than the square.)

It is, to be sure, an empirical question whether the cognitive capacities of infraverbal organisms are often structured that way, but we're prepared to bet that they are not. Ethological cases are the exceptions that prove the rule. There *are* examples where salient environmental configurations act as 'gestalten'; and in such cases it's reasonable to doubt that the mental representation of the stimulus is complex. But the point is precisely that these cases are *exceptional*; they're exactly the ones where you expect that there will be some special story to tell about the ecological significance of the stimulus: that it's the shape of a predator, or the song of a conspecific ... etc. Conversely, when there is no such story to tell you expect structurally similar stimuli to elicit correspondingly similar cognitive capacities. That, surely, is the least that a respectable principle of stimulus generalization has got to require.

That infraverbal cognition is pretty generally systematic seems, in short, to be about as secure as any empirical premise in this area can be. And, as we've just seen, it's a premise from which the inadequacy of Connectionist models as cognitive theories follows quite straightforwardly; as straightforwardly, in any event, as it would from the assumption that such capacities are generally productive.

3.3. Compositionality of representations

Compositionality is closely related to systematicity; perhaps they're best viewed as aspects of a single phenomenon. We will therefore follow much the same course here as in the preceding discussion: first we introduce the concept by recalling the standard arguments for the compositionality of natural languages. We then suggest that parallel arguments secure the compositionality of mental representations. Since compositionality requires combinatorial syntactic and semantic structure, the compositionality of thought is evidence that the mind is not a Connectionist network.

We said that the systematicity of linguistic competence consists in the fact that "the ability to produce/understand some of the sentences is intrinsically connected to the ability to produce/understand certain of the others". We now add that which sentences are systematically related is not arbitrary from a semantic point of view. For example, being able to understand 'John loves the girl' goes along with being able to understand 'the girl loves John', and

there are correspondingly close semantic relations between these sentences: in order for the first to be true, John must bear to the girl the very same relation that the truth of the second requires the girl to bear to John. By contrast, there is no intrinsic connection between understanding either of the John/girl sentences and understanding semantically unrelated formulas like 'quarks are made of gluons' or 'the cat is on the mat' or '2 + 2 = 4'; it looks as though semantical relatedness and systematicity keep quite close company.

You might suppose that this covariance is covered by the same explanation that accounts for systematicity per se; roughly, that sentences that are systematically related are composed from the same syntactic constituents. But, in fact, you need a further assumption, which we'll call the 'principle of compositionality': insofar as a language is systematic, a lexical item must make approximately the same semantic contribution to each expression in which it occurs. It is, for example, only insofar as 'the' 'girl', 'loves' and 'John' make the same semantic contribution to 'John loves the girl' that they make to 'the girl loves John' that understanding the one sentence implies understanding the other. Similarity of constituent structure accounts for the semantic relatedness between systematically related sentences only to the extent that the semantical properties of the shared constituents are context-independent.

Here it's idioms that prove the rule: being able to understand 'the', 'man', 'kicked' and 'bucket' isn't much help with understanding 'the man kicked the bucket', since 'kicked' and 'bucket' don't bear their standard meanings in this context. And, just as you'd expect, 'the man kicked the bucket' is *not* systematic even with respect to syntactically closely related sentences like 'the man kicked over the bucket' (for that matter, it's not systematic with respect to the 'the man kicked the bucket' read literally).

It's uncertain exactly how compositional natural languages actually are (just as it's uncertain exactly how systematic they are). We suspect that the amount of context induced variation of lexical meaning is often overestimated because other sorts of context sensitivity are misconstrued as violations of compositionality. For example, the difference between 'feed the chicken' and 'chicken to eat' must involve an *animal/food* ambiguity in 'chicken' rather than a violation of compositionality since if the context 'feed the ...' could *induce* (rather than select) the meaning *animal*, you would expect 'feed the veal', 'feed the pork' and the like.[27] Similarly, the difference between 'good book', 'good rest' and 'good fight' is probably not meaning shift but syncategorematicity. 'Good *NP*' means something like *NP that answers to the*

[27]We are indebted to Steve Pinker for this point.

relevant interest in NPs: a good book is one that answers to our interest in books (viz., it's good to read); a good rest is one that answers to our interest in rests (viz., it leaves one refreshed); a good fight is one that answers to our interest in fights (viz., it's fun to watch or to be in, or it clears the air); and so on. It's because the meaning of 'good' is syncategorematic and has a variable in it for relevant interests, that you can know that a good flurg is a flurg that answers to the relevant interest in flurgs without knowing what flurgs are or what the relevant interest in flurgs is (see Ziff, 1960).

In any event, the main argument stands: systematicity depends on compositionality, so to the extent that a natural language is systematic it must be compositional too. This illustrates another respect in which systematicity arguments can do the work for which productivity arguments have previously been employed. The traditional argument for compositionality is that it is required to explain how a finitely representable language can contain infinitely many nonsynonymous expressions.

Considerations about systematicity offer one argument for compositionality; considerations about entailment offer another. Consider predicates like '... is a brown cow'. This expression bears a straightforward semantical relation to the predicates '... is a cow' and '... is brown'; viz., that the first predicate is true of a thing if and only if both of the others are. That is, '... is a brown cow' severally entails '... is brown' and '... is a cow' and is entailed by their conjunction. Moreover—and this is important—this semantical pattern is not peculiar to the cases cited. On the contrary, it holds for a very large range of predicates (see '... is a red square,' '... is a funny old German soldier,' '... is a child prodigy;' and so forth).

How are we to account for these sorts of regularities? The answer seems clear enough; '... is a brown cow' entails '... is brown' because (a) the second expression is a constituent of the first; (b) the syntactical form '(adjective noun)$_N$' has (in many cases) the semantic force of a conjunction, and (c) 'brown' retains its semantical value under simplification of conjunction. Notice that you need (c) to rule out the possibility that 'brown' means *brown* when in it modifies a noun but (as it might be) *dead* when it's a predicate adjective; in which case '... is a brown cow' wouldn't entail '... is brown' after all. Notice too that (c) is just an application of the principle of composition.

So, here's the argument so far: you need to assume some degree of compositionality of English sentences to account for the fact that systematically related sentences are always semantically related; and to account for certain regular parallelisms between the syntactical structure of sentences and their entailments. So, beyond any serious doubt, the sentences of English must be compositional to some serious extent. But the principle of compositionality governs the semantic relations between words *and the expressions of which*

they are constituents. So compositionality implies that (some) expressions *have* constituents. So compositionality argues for (specifically, presupposes) syntactic/semantic structure in sentences.

Now what about the compositionality of mental representations? There is, as you'd expect, a bridging argument based on the usual psycholinguistic premise that one uses language to express ones thoughts: Sentences are used to express thoughts; so if the ability to use some sentences is connected with the ability to use certain other, semantically related sentences, then the ability to think some thoughts must be correspondingly connected with the ability to think certain other, semantically related thoughts. But you can only think the thoughts that your mental representations can express. So, if the ability to think certain thoughts is interconnected, then the corresponding representational capacities must be interconnected too; specifically, the ability to be in some representational states must imply the ability to be in certain other, semantically related representational states.

But then the question arises: *how could* the mind be so arranged that the ability to be in one representational state is connected with the ability to be in others that are semantically nearby? What account of mental representation would have this consequence? The answer is just what you'd expect from the discussion of the linguistic material. Mental representations must have internal structure, just the way that sentences do. In particular, it must be that the mental representation that corresponds to the thought that John loves the girl contains, as its parts, the same constituents as the mental representation that corresponds to the thought that the girl loves John. That would explain why these thoughts are *systematically* related; *and, to the extent that the semantic value of these parts is context-independent, that would explain why these systematically related thoughts are also semantically related.* So, by this chain of argument, evidence for the compositionality of sentences is evidence for the compositionality of the representational states of speaker/hearers.

Finally, what about the compositionality of infraverbal thought? The argument isn't much different from the one that we've just run through. We assume that animal thought is largely systematic: the organism that can perceive (hence learn) that *a***R***b* can generally perceive (/learn) that *b***R***a*. But, systematically related thoughts (just like systematically related sentences) are generally semantically related too. It's no surprise that being able to learn that the triangle is above the square implies being able to learn that the square is above the triangle; whereas it would be *very* surprising if being able to learn the square/triangle facts implied being able to learn that quarks are made of gluons or that Washington was the first President of America.

So, then, what explains the correlation between systematic relations and

semantic relations in infraverbal thought? Clearly, Connectionist models don't address this question; the fact that a network contains a node labelled X has, so far as the constraints imposed by Connectionist architecture are concerned, *no implications at all* for the labels of the other nodes in the network; in particular, it doesn't imply that there will be nodes that represent thoughts that are semantically close to X. This is just the semantical side of the fact that network architectures permit arbitrarily punctate mental lives.

But if, on the other hand, we make the usual Classicist assumptions (viz., that systematically related thoughts share constituents and that the semantic values of these shared constituents are context independent) the correlation between systematicity and semantic relatedness follows immediately. For a Classicist, this correlation is an 'architectural' property of minds; it couldn't but hold if mental representations have the general properties that Classical models suppose them to.

What have Connectionists to say about these matters? There is some textual evidence that they are tempted to deny the facts of compositionality wholesale. For example, Smolensky (1988) claims that: "Surely ... we would get quite a different representation of 'coffee' if we examined the difference between 'can with coffee' and 'can without coffee' or 'tree with coffee' and 'tree without coffee'; or 'man with coffee' and 'man without coffee' ... context insensitivity is not something we expect to be reflected in Connectionist representations ....".

It's certainly true that compositionality is not generally a feature of Connectionist representations. Connectionists can't acknowledge the facts of compositionality because they are committed to mental representations that don't have combinatorial structure. But to give up on compositionality is to take 'kick the bucket' as a model for the relation between syntax and semantics; and the consequence is, as we've seen, that you make the systematicity of language (and of thought) a mystery. On the other hand, to say that 'kick the bucket' is aberrant, and that the right model for the syntax/semantics relation is (e.g.) 'brown cow', is to start down a trail which leads, pretty inevitably, to acknowledging combinatorial structure in mental representation, hence to the rejection of Connectionist networks as cognitive models.

We don't think there's any way out of the need to acknowledge the compositionality of natural languages and of mental representations. However, it's been suggested (see Smolensky, op cit.) that while the principle of compositionality is false (because content isn't context invariant) there is nevertheless a "family resemblance" between the various meanings that a symbol has in the various contexts in which it occurs. Since such proposals generally aren't elaborated, it's unclear how they're supposed to handle the salient facts about systematicity and inference. But surely there are going to

be serious problems. Consider, for example, such inferences as

(i) Turtles are slower than rabbits.
(ii) Rabbits are slower than Ferraris.
.....
(iii) Turtles are slower than Ferraris.

The soundness of this inference appears to depend upon (a) the fact that the same relation (viz., *slower than*) holds between turtles and rabbits on the one hand, and rabbits and Ferraris on the other; and (b) the fact that that relation is transitive. If, however, it's assumed (contrary to the principle of compositionality) that 'slower than' means something different in premises (i) and (ii) (and presumably in (iii) as well)—so that, strictly speaking, the relation that holds between turtles and rabbits is *not* the same one that holds between rabbits and Ferraris—then it's hard to see why the inference should be valid.

Talk about the relations being 'similar' only papers over the difficulty since the problem is then to provide a notion of similarity that will guaranty that if (i) and (ii) are true, so too is (iii). And, so far at least, no such notion of similarity has been forthcoming. Notice that it won't do to require just that the relations all be similar in respect of their *transitivity*, i.e., that they all be transitive. On that account, the argument from 'turtles are slower than rabbits' and 'rabbits are furrier than Ferraris' to 'turtles are slower than Ferraris' would be valid since 'furrier than' is transitive too.

Until these sorts of issues are attended to, the proposal to replace the compositional principle of context invariance with a notion of "approximate equivalence ... across contexts" (Smolensky, 1988) doesn't seem to be much more than hand waving.

3.4. The systematicity of inference

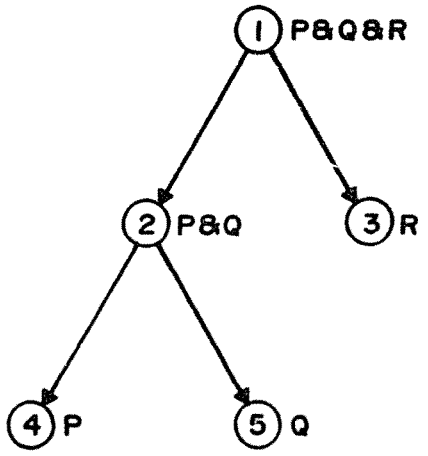
In Section 2 we saw that, according to Classical theories, the syntax of mental representations mediates between their semantic properties and their causal role in mental processes. Take a simple case: It's a 'logical' principle that conjunctions entail their constituents (so the argument from $P\&Q$ to P and to Q is valid). Correspondingly, it's a psychological law that thoughts that $P\&Q$ tend to cause thoughts that P and thoughts that Q, all else being equal. Classical theory exploits the constituent structure of mental representations to account for both these facts, the first by assuming that the combinatorial semantics of mental representations is sensitive to their syntax and the second by assuming that mental processes apply to mental representations in virtue of their constituent structure.

A consequence of these assumptions is that Classical theories are commit-

ted to the following striking prediction: inferences that are of similar logical type ought, pretty generally,[28] to elicit correspondingly similar cognitive capacities. You shouldn't, for example, find a kind of mental life in which you get inferences from *P&Q&R* to *P* but you don't get inferences from *P&Q* to *P*. This is because, according to the Classical account, this logically homogeneous class of inferences is carried out by a correspondingly homogeneous class of psychological mechanisms: The premises of both inferences are expressed by mental representations that satisfy the same syntactic analysis (viz., $S_1\&S_2\&S_3\&\ \ldots\ S_n$); and the process of drawing the inference corresponds, in both cases, to the same formal operation of detaching the constituent that expresses the conclusion.

The idea that organisms should exhibit similar cognitive capacities in respect of logically similar inferences is so natural that it may seem unavoidable. But, on the contrary: there's nothing in principle to preclude a kind of cognitive model in which inferences that are quite similar from the logician's point of view are nevertheless computed by quite different mechanisms; or in which some inferences of a given logical type are computed and other inferences of the same logical type are not. Consider, in particular, the Connectionist account. A Connectionist can certainly model a mental life in which, if you can reason from *P&Q&R* to *P*, then you can also reason from *P&Q* to *P*. For example, the network in (Figure 3) would do:

Figure 3. *A possible Connectionist network which draws inferences from P&Q&R to P and also draws inferences from P&Q to P.*



[28]The hedge is meant to exclude cases where inferences of the same logical type nevertheless differ in complexity in virtue of, for example, the length of their premises. The inference from (A∨B∨C∨D∨E) and (−B&−C&−D&−E) to A is of the same logical type as the inference from A∨B and −B to A. But it wouldn't be very surprising, or very interesting, if there were minds that could handle the second inference but not the first.

But notice that *a Connectionist can equally model a mental life in which you get one of these inferences and not the other.* In the present case, since there is no structural relation between the *P&Q&R* node and the *P&Q* node (remember, all nodes are atomic; don't be misled by the node *labels*) there's no reason why a mind that contains the first should also contain the second, or vice versa. Analogously, there's no reason why you shouldn't get minds that simplify the premise *John loves Mary and Bill hates Mary* but no others; or minds that simplify premises with 1, 3, or 5 conjuncts, but don't simplify premises with 2, 4, or 6 conjuncts; or, for that matter, minds that simplify only premises that were acquired on Tuesdays ... etc.

In fact, the Connectionist architecture is *utterly indifferent* as among these possibilities. That's because it recognizes no notion of syntax according to which thoughts that are alike in inferential role (e.g., thoughts that are all subject to simplification of conjunction) are expressed by mental representations of correspondingly similar syntactic form (e.g., by mental representations that are all syntactically conjunctive). So, the Connectionist architecture tolerates gaps in cognitive capacities; it has no mechanism to enforce the requirement that logically homogeneous inferences should be executed by correspondingly homogeneous computational processes.

But, we claim, you don't find cognitive capacities that have these sorts of gaps. You don't, for example, get minds that are prepared to infer *John went to the store* from *John and Mary and Susan and Sally went to the store* and from *John and Mary went to the store* but not from *John and Mary and Susan went to the store*. Given a notion of logical syntax—the very notion that the Classical theory of mentation requires to get its account of mental processes off the ground—it is a *truism* that you don't get such minds. Lacking a notion of logical syntax, it is a *mystery* that you don't.

3.5. Summary

It is perhaps obvious by now that all the arguments that we've been reviewing—the argument from systematicity, the argument from compositionality, and the argument from influential coherence—are really much the same: If you hold the kind of theory that acknowledges structured representations, it must perforce acknowledge representations with *similar* or *identical* structures. In the linguistic cases, constituent analysis implies a taxonomy of sentences by their syntactic form, and in the inferential cases, it implies a taxonomy of arguments by their logical form. So, if your theory also acknowledges mental processes that are structure sensitive, then it will predict that similarly structured representations will generally play similar roles in thought. A theory that says that the sentence 'John loves the girl' is made

out of the same parts as the sentence 'the girl loves John', and made by applications of the same rules of composition, will have to go out of its way to explain a linguistic competence which embraces one sentence but not the other. And similarly, if a theory says that the mental representation that corresponds to the thought that *P&Q&R* has the same (conjunctive) syntax as the mental representation that corresponds to the thought that *P&Q* and that mental processes of drawing inferences subsume mental representations in virtue of their syntax, it will have to go out of its way to explain inferential capacities which embrace the one thought but not the other. Such a competence would be, at best, an embarrassment for the theory, and at worst a refutation.

By contrast, since the Connectionist architecture recognizes no combinatorial structure in mental representations, gaps in cognitive competence should proliferate arbitrarily. It's not just that you'd expect to get them from time to time; it's that, on the 'no-structure' story, *gaps are the unmarked case.* It's the *systematic* competence that the theory is required to treat as an embarrassment. But, as a matter of fact, inferential competences are *blatantly* systematic. So there must be something deeply wrong with Connectionist architecture.

What's deeply wrong with Connectionist architecture is this: Because it acknowledges neither syntactic nor semantic structure in mental representations, it perforce treats them not as a generated set but as a list. But lists, qua lists, have no structure; any collection of items is a possible list. And, correspondingly, on Connectionist principles, any collection of (causally connected) representational states is a possible mind. So, as far as Connectionist architecture is concerned, there is nothing to prevent minds that are arbitrarily unsystematic. But that result is *preposterous.* Cognitive capacities come in structurally related clusters; their systematicity is pervasive. All the evidence suggests that *punctate minds can't happen.* This argument seemed conclusive against the Connectionism of Hebb, Osgood and Hull twenty or thirty years ago. So far as we can tell, nothing of any importance has happened to change the situation in the meantime.[29]

[29]Historical footnote: Connectionists are Associationists, but not every Associationist holds that mental representations must be unstructured. Hume didn't, for example. Hume thought that mental representations are rather like pictures, and pictures typically have a compositional semantics: the parts of a picture of a horse are generally pictures of horse parts.

On the other hand, allowing a compositional semantics for mental representations doesn't do an Associationist much good so long as he is true to this spirit of his Associationism. The virtue of having mental representations with structure is that it allows for structure sensitive operations to be defined over them; specifically, it allows for the sort of operations that eventuate in productivity and systematicity. Association is not, however, such an operation; all *it* can do is build an internal model of redundancies in experience by

A final comment to round off this part of the discussion. It's possible to imagine a Connectionist being prepared to admit that while systematicity doesn't *follow from*—and hence is not explained by—Connectionist architecture, it is nonetheless *compatible* with that architecture. It is, after all, perfectly possible to follow a policy of building networks that have *a*R*b* nodes only if they have *b*R*a* nodes ... etc. There is therefore nothing to stop a Connectionist from stipulating—as an independent postulate of his theory of mind—that all biologically instantiated networks are, de facto, systematic.

But this misses a crucial point: It's not enough just to stipulate systematicity; one is also required to specify a mechanism that is able to enforce the stipulation. To put it another way, it's not enough for a Connectionist to agree that all minds are systematic; he must also explain *how nature contrives to produce only systematic minds.* Presumably there would have to be some sort of mechanism, over and above the ones that Connectionism per se posits, the functioning of which insures the systematicity of biologically instantiated networks; a mechanism such that, in virtue of its operation, every network that has an *a*R*b* node also has a *b*R*a* node ... and so forth. There are, however, no proposals for such a mechanism. Or, rather, there is just one: The only mechanism that is known to be able to produce pervasive systematicity is Classical architecture. And, as we have seen, Classical architecture is not compatible with Connectionism since it requires internally structured representations.

4. The lure of Connectionism

The current popularity of the Connectionist approach among psychologists and philosophers is puzzling in view of the sorts of problems raised above; problems which were largely responsible for the development of a syntax-based (proof theoretic) notion of computation and a Turing-style, symbol-processing notion of cognitive architecture in the first place. There are, however, a number of apparently plausible arguments, repeatedly encountered

altering the probabilities of transitions among mental states. So far as the problems of productivity and systematicity are concerned, an Associationist who acknowledges structured representations is in the position of having the can but not the opener.

Hume, in fact, cheated: he allowed himself not just Association but also "Imagination", which he takes to be an 'active' faculty that can produce new concepts out of old parts by a process of analysis and recombination. (The idea of a unicorn is pieced together out of the idea of a horse and the idea of a horn, for example.) Qua associationist Hume had, of course, no right to active mental faculties. But allowing imagination in gave Hume precisely what modern Connectionists don't have: an answer to the question how mental processes can be productive. The moral is that if you've got structured representations, the temptation to postulate structure sensitive operations and an executive to apply them is practically irresistible.

in the literature, that stress certain limitations of conventional computers as models of brains. These may be seen as favoring the Connectionist alternative. We will sketch a number of these before discussing the general problems which they appear to raise.

- *Rapidity of cognitive processes in relation to neural speeds: the "hundred step" constraint.* It has been observed (e.g., Feldman & Ballard, 1982) that the time required to execute computer instructions is in the order of nanoseconds, whereas neurons take tens of milliseconds to fire. Consequently, in the time it takes people to carry out many of the tasks at which they are fluent (like recognizing a word or a picture, either of which may require considerably less than a second) a *serial* neurally-instantiated program would only be able to carry out about 100 instructions. Yet such tasks might typically require many thousands—or even millions—of instructions in present-day computers (if they can be done at all). Thus, it is argued, the brain must operate quite differently from computers. In fact, the argument goes, the brain must be organized in a highly parallel manner ("massively parallel" is the preferred term of art).
- *Difficulty of achieving large-capacity pattern recognition and content-based retrieval in conventional architectures.* Closely related to the issues about time constraints is the fact that humans can store and make use of an enormous amount of information—apparently without effort (Fahlman & Hinton, 1987). One particularly dramatic skill that people exhibit is the ability to recognize patterns from among tens or even hundreds of thousands of alternatives (e.g., word or face recognition). In fact, there is reason to believe that many expert skills may be based on large, fast recognition memories (see Simon & Chase, 1973). If one had to search through one's memory serially, the way conventional computers do, the complexity would overwhelm any machine. Thus, the knowledge that people have must be stored and retrieved differently from the way conventional computers do it.
- *Conventional computer models are committed to a different etiology for "rule-governed" behavior and "exceptional" behavior.* Classical psychological theories, which are based on conventional computer ideas, typically distinguish between mechanisms that cause regular and divergent behavior by postulating systems of explicit unconscious rules to explain the former, and then attributing departures from these rules to secondary (performance) factors. Since the divergent behaviors occur very frequently, a better strategy would be to try to account for both types of behavior in terms of the same mechanism.

- *Lack of progress in dealing with processes that are nonverbal or intuitive.* Most of our fluent cognitive skills do not consist in accessing verbal knowledge or carrying out deliberate conscious reasoning (Fahlman & Hinton, 1987; Smolensky, 1988). We appear to know many things that we would have great difficulty in describing verbally, including how to ride a bicycle, what our close friends look like, and how to recall the name of the President, etc. Such knowledge, it is argued, must not be stored in linguistic form, but in some other "implicit" form. The fact that conventional computers typically operate in a "linguistic mode", inasmuch as they process information by operating on syntactically structured expressions, may explain why there has been relatively little success in modeling implicit knowledge.
- *Acute sensitivity of conventional architectures to damage and noise.* Unlike digital circuits, brain circuits must tolerate noise arising from spontaneous neural activity. Moreover, they must tolerate a moderate degree of damage without failing completely. With a few notable exceptions, if a part of the brain is damaged, the degradation in performance is usually not catastrophic but varies more or less gradually with the extent of the damage. This is especially true of memory. Damage to the temporal cortex (usually thought to house memory traces) does not result in selective loss of particular facts and memories. This and similar facts about brain damaged patients suggests that human memory representations, and perhaps many other cognitive skills as well, are *distributed* spatially, rather than being neurally localized. This appears to contrast with conventional computers, where hierarchical-style control keeps the crucial decisions highly localized and where memory storage consists of an array of location-addressable registers.
- *Storage in conventional architectures is passive.* Conventional computers have a passive memory store which is accessed in what has been called a "fetch and execute cycle". This appears to be quite unlike human memory. For example, according to Kosslyn and Hatfield (1984, pp. 1022, 1029):

> In computers the memory is static: once an entry is put in a given location, it just sits there until it is operated upon by the CPU .... But consider a very simple experiment: Imagine a letter *A* over and over again ... then switch to the letter *B*. In a model employing a Von Neumann architecture the 'fatigue' that inhibited imaging the *A* would be due to some quirk in the way the CPU executes a given instruction .... Such fatigue should generalize to all objects imaged because the routine responsible for imaging was less effective. But experiments have demonstrated that this is not true: specific objects become more difficult to image, not all objects. This

> finding is more easily explained by an analogy to the way invisible ink fades of its own accord ...: with invisible ink, the representation itself is doing something—there is no separate processor working over it ... .

- *Conventional rule-based systems depict cognition as "all-or-none".* But cognitive skills appear to be characterized by various kinds of continuities. For example:
- *Continuous variation in degree of applicability of different principles*, or in the degree of relevance of different constraints, "rules", or procedures. There are frequent cases (especially in perception and memory retrieval), in which it appears that a variety of different constraints are brought to bear on a problem simultaneously and the outcome is a combined effect of all the different factors (see, for example, the informal discussion by McClelland, Rumelhart & Hinton, 1986, pp. 3–9). That's why "constraint propagation" techniques are receiving a great deal of attention in artificial intelligence (see Mackworth, 1987).
- *Nondeterminism of human behavior:* Cognitive processes are never rigidly determined or precisely replicable. Rather, they appear to have a significant random or stochastic component. Perhaps that's because there is randomness at a microscopic level, caused by irrelevant biochemical or electrical activity or perhaps even by quantum mechanical events. To model this activity by rigid deterministic rules can only lead to poor predictions because it ignores the fundamentally stochastic nature of the underlying mechanisms. Moreover, deterministic, all-or-none models will be unable to account for the gradual aspect of learning and skill acquisition.
- *Failure to display graceful degradation.* When humans are unable to do a task perfectly, they nonetheless do something reasonable. If the particular task does not fit exactly into some known pattern, or if it is only partly understood, a person will not give up or produce nonsensical behavior. By contrast, if a Classical rule-based computer program fails to recognize the task, or fails to match a pattern to its stored representations or rules, it usually will be unable to do anything at all. This suggests that in order to display graceful degradation, we must be able to represent prototypes, match patterns, recognize problems, etc., in various *degrees.*
- *Conventional models are dictated by current technical features of computers and take little or no account of the facts of neuroscience.* Classical symbol processing systems provide no indication of how the kinds of processes that they postulate could be realized by a brain. The fact that this gap between high-level systems and brain architecture is so large might be an indication that these models are on the wrong track.

> Whereas the architecture of the mind has evolved under the pressures of natural selection, some of the Classical assumptions about the mind may derive from features that computers have only because they are explicitly designed for the convenience of programmers. Perhaps this includes even the assumption that the description of mental processes at the cognitive level can be divorced from the description of their physical realization. At a minimum, by building our models to take account of what is known about neural structures we may reduce the risk of being misled by metaphors based on contemporary computer architectures.

Replies: Why the usual reasons given for preferring a Connectionist architecture are invalid

It seems to us that, as arguments against Classical cognitive architecture, all these points suffer from one or other of the following two defects.

(1) The objections depend on properties that are not in fact intrinsic to Classical architectures, since there can be perfectly natural Classical models that don't exhibit the objectionable features. (We believe this to be true, for example, of the arguments that Classical rules are explicit and Classical operations are 'all or none'.)

(2) The objections are true of Classical architectures insofar as they are implemented on current computers, but need not be true of such architectures when differently (e.g., neurally) implemented. They are, in other words, directed at the implementation level rather than the cognitive level, as these were distinguished in our earlier discussion. (We believe that this is true, for example, of the arguments about speed, resistance to damage and noise, and the passivity of memory.)

In the remainder of this section we will expand on these two points and relate them to some of the arguments presented above. Following this analysis, we will present what we believe may be the most tenable view of Connectionism; namely that it is a theory of how (Classical) cognitive systems might be implemented, either in real brains or in some 'abstract neurology'.

Parallel computation and the issue of speed

Consider the argument that cognitive processes must involve large scale parallel computation. In the form that it takes in typical Connectionist discussions, this issue is irrelevant to the adequacy of Classical *cognitive* architec-

ture. The "hundred step constraint", for example, is clearly directed at the implementation level. All it rules out is the (absurd) hypothesis that cognitive architectures are implemented in the brain in the same way as they are implemented on electronic computers.

If you ever have doubts about whether a proposal pertains to the implementation level or the symbolic level, a useful heuristic is to ask yourself whether what is being claimed is true of a conventional computer—such as the DEC VAX—at *its* implementation level. Thus although most algorithms that run on the VAX are serial,[30] at the implementation level such computers are 'massively parallel'; they quite literally involve simultaneous electrical activity throughout almost the entire device. For example, every memory access cycle involves pulsing every bit in a significant fraction of the system's memory registers—since memory access is essentially a destructive read and rewrite process, the system clock regularly pulses and activates most of the central processing unit, and so on.

The moral is that the absolute speed of a process is a property *par excellence* of its implementation. (By contrast, the *relative* speed with which a system responds to different inputs is often diagnostic of distinct processes; but this has always been a prime empirical basis for deciding among alternative algorithms in information processing psychology). Thus, the fact that individual neurons require tens of miliseconds to fire can have no bearing on the predicted speed at which an algorithm will run *unless there is at least a partial, independently motivated, theory of how the operations of the functional architecture are implemented in neurons.* Since, in the case of the brain, it is not even certain that the firing[31] of neurons is invariably the relevant implementation property (at least for higher level cognitive processes like learning and memory) the 100 step "constraint" excludes nothing.

Finally, absolute constraints on the number of serial steps that a mental process can require, or on the time that can be required to execute them, provide weak arguments against Classical architecture because Classical architecture in no way excludes parallel execution of multiple symbolic processes. Indeed, it seems extremely likely that many Classical symbolic processes

[30]Even in the case of a conventional computer, whether it should be viewed as executing a serial or a parallel algorithm depends on what virtual machine' is being considered in the case in question. After all, a VAX *can* be used to simulate (i.e., to implement) a virtual machine with a parallel architecture. In that case the relevant algorithm would be a parallel one.

[31]There are, in fact, a number of different mechanisms of neural interaction (e.g., the "local interactions" described by Rakic, 1975). Moreover, a large number of chemical processes take place at the dendrites, covering a wide range of time scales, so even if dendritic transmission were the only relevant mechanism, we still wouldn't know what time scale to use as our estimate of neural action in general (see, for example, Black, 1986).

are going on in parallel in cognition, and that these processes interact with one another (e.g., they may be involved in some sort of symbolic constraint propagation). Operating on symbols can even involve "massively parallel" organizations; that might indeed imply new architectures, but they are all *Classical* in our sense, since they all share the Classical conception of computation as symbol-processing. (For examples of serious and interesting proposals on organizing Classical processors into large parallel networks, see Hewett's, 1977, *Actor* system, Hillis', 1985, "Connection Machine", as well as any of a number of recent commercial multi-processor machines.) The point here is that an argument for a network of parallel computers is not in and of itself either an argument against a Classical architecture or an argument for a Connectionist architecture.

Resistance to noise and physical damage (and the argument for distributed representation)

Some of the other advantages claimed for Connectionist architectures over Classical ones are just as clearly aimed at the implementation level. For example, the "resistance to physical damage" criterion is so obviously a matter of implementation that it should hardly arise in discussions of cognitive-level theories.

It is true that a certain kind of damage-resistance appears to be incompatible with localization, and it is also true that representations in PDP's are distributed over groups of units (at least when "coarse coding" is used). But distribution over units achieves damage-resistance only if it entails that representations are also *neurally* distributed.[32] However, neural distribution of representations is just as compatible with Classical architectures as it is with Connectionist networks. In the Classical case all you need are memory registers that distribute their contents over physical space. You can get that with fancy storage systems like optical ones, or chemical ones, or even with registers made of Connectionist nets. Come to think of it, we already had it in the old style "ferrite core" memories!

[32]Unless the 'units' in a Connectionist network really are assumed to have different spatially-focused loci in the brain, talk about distributed representation is likely to be extremely misleading. In particular, if units are merely *functionally* individuated, any amount of distribution or functional entities is compatible with any amount of spatial compactness of their neural representations. But it is not clear that units do, in fact, correspond to any anatomically identifiable locations in the brain. In the light of the way Connectionist mechanisms are designed, it may be appropriate to view units and links as functional/mathematical entities (what psychologists would call "hypothetical constructs") whose neurological interpretation remains entirely open. (This is, in fact, the view that some Connectionists take; see Smolensky, 1988.) The point is that distribution over mathematical constructs does not buy you damage resistance; only *neural* distribution does!

The physical requirements of a Classical symbol-processing system are easily misunderstood. (Confounding of physical and functional properties is widespread in psychological theorizing in general; for a discussion of this confusion in relation to metrical properties in models of mental imagery, see Pylyshyn 1981.) For example, conventional architecture requires that there be distinct symbolic expressions for each state of affairs that it can represent. Since such expressions often have a structure consisting of concatenated parts, the adjacency relation must be instantiated by *some* physical relation when the architecture is implemented (see the discussion in footnote 9). However, since the relation to be physically realized is *functional* adjacency, there is no necessity that physical instantiations of adjacent symbols be *spatially* adjacent. Similarly, although complex expressions are made out of atomic elements, and the distinction between atomic and complex symbols must somehow be physically instantiated, there is no necessity that a token of an atomic symbol be assigned a smaller region in space than a token of a complex symbol; even a token of a complex symbol of which it is a constituent. In Classical architectures, as in Connectionist networks, functional elements can be physically distributed or localized to any extent whatever. In a VAX (to use our heuristic again) pairs of symbols may certainly be functionally adjacent, but the symbol tokens are nonetheless spatially spread through many locations in physical memory.

In short, the fact that a property (like the position of a symbol within an expression) is functionally local has no implications one way or the other for damage-resistance or noise tolerance unless the functional-neighborhood metric corresponds to some appropriate *physical* dimension. When that is the case, we may be able to predict adverse consequences that varying the physical property has on objects localized in functional space (e.g., varying the voltage or line frequency might damage the left part of an expression). But, of course, the situation is exactly the same for Connectionist systems: even when they are resistant to spatially-local damage, they may not be resistant to damage that is local along some other physical dimensions. Since spatially-local damage is particularly frequent in real world traumas, this may have important practical consequences. But so long as our knowledge of how cognitive processes might be mapped onto brain tissue remains very nearly nonexistent, its message for cognitive science remains moot.

"Soft" constraints, continuous magnitudes, stochastic mechanisms, and active symbols

The notion that "soft" constraints which can vary continuously (as degree of activation does), are incompatible with Classical rule-based symbolic systems

is another example of the failure to keep the psychological (or symbol-processing) and the implementation levels separate. One can have a Classical rule system in which the decision concerning which rule will fire resides in the functional architecture and depends on continuously varying magnitudes. Indeed, this is typically how it is done in practical "expert systems" which, for example, use a Bayesian mechanism in their production-system rule-interpreter. The soft or stochastic nature or rule-based processes arises from the interaction of deterministic rules with real-valued properties of the implementation, or with noisy inputs or noisy information transmission.

It should also be noted that rule applications need not issue in "all or none" behaviors since several rules may be activated at once and can have interactive effects on the outcome. Or, alternatively, each of the activated rules can generate independent parallel effects, which might get sorted out later—depending say, on which of the parallel streams reaches a goal first. An important, though sometimes neglected point about such aggregate properties of overt behavior as continuity, "fuzziness", randomness, etc., is that they need not arise from underlying mechanisms that are themselves fuzzy, continuous or random. It is not only possible in principle, but often quite reasonable in practice, to assume that apparently variable or nondeterministic behavior arises from the interaction of multiple deterministic sources.

A similar point can be made about the issue of "graceful degradation". Classical architecture does not require that when the conditions for applying the available rules aren't precisely met, the process should simply fail to do anything at all. As noted above, rules could be activated in some measure depending upon how close their conditions are to holding. Exactly what happens in these cases may depend on how the rule-system is implemented. On the other hand, it could be that the failure to display "graceful degradation" really is an intrinsic limit of the current class of models or even of current approaches to designing intelligent systems. It seems clear that the psychological models now available are inadequate over a broad spectrum of measures, so their problems with graceful degradation may be a special case of their general unintelligence: They may simply not be smart enough to know what to do when a limited stock of methods fails to apply. But this needn't be a principled limitation of Classical architectures: There is, to our knowledge, no reason to believe that something like Newell's (1969) "hierarchy of weak methods" or Laird, Rosenberg and Newell's (1986) "universal subgoaling", is in principle incapable of dealing with the problem of graceful degradation. (Nor, to our knowledge, has any argument yet been offered that Connectionist architectures are in principle capable of dealing with it. In fact current Connectionist models are every bit as graceless in their modes of failure as ones based on Classical architectures. For example, contrary to some claims,

models such as that of McClelland and Kawamoto, 1986, fail quite unnaturally when given incomplete information.)

In short, the Classical theorist can view stochastic properties of behavior as emerging from interactions between the model and the intrinsic properties of the physical medium in which it is realized. It is essential to remember that, from the Classical point of view, overt behavior is par excellence an interaction effect, and symbol manipulations are supposed to be only one of the interacting causes.

These same considerations apply to Kosslyn and Hatfield's remarks (quoted earlier) about the commitment of Classical models to 'passive' versus 'active' representations. It is true, as Kosslyn and Hatfield say, that the representations that Von Neumann machines manipulate 'don't *do* anything' until a CPU operates upon them (they don't decay, for example). But, even on the absurd assumption that the mind has *exactly* the architecture of some contemporary (Von Neumann) computer, it is obvious that its behavior, and hence the behavior of an organism, is determined not just by the logical machine that the mind instantiates, but also by the protoplasmic machine in which the logic is realized. Instantiated representations *are* therefore bound to be active, even according to Classical models; the question is whether the kind of activity they exhibit should be accounted for by the cognitive model or by the theory of its implementation. This question is empirical and must not be begged on behalf of the Connectionist view. (As it is, for example, in such passages as "The brain itself does not manipulate symbols; the brain is the medium in which the symbols are floating and in which they trigger each other. There is no central manipulator, no central program. There is simply a vast collection of 'teams'—patterns of neural firings that, like teams of ants, trigger other patterns of neural firings ... . We feel those symbols churning within ourselves in somewhat the same way we feel our stomach churning." (Hofstadter, 1983, p. 279). This appears to be a serious case of *Formicidae in machina:* ants in the stomach of the ghost in the machine.)

Explicitness of rules

According to McClelland, Feldman, Adelson, Bower, and McDermott (1986, p. 6), "... Connectionist models are leading to a reconceptualization of key psychological issues, such as the nature of the representation of knowledge ... . One traditional approach to such issues treats knowledge as a body of rules that are consulted by processing mechanisms in the course of processing; in Connectionist models, such knowledge is represented, often in widely distributed form, in the connections among the processing units."

As we remarked in the Introduction, we think that the claim that most

psychological processes are rule-implicit, and the corresponding claim that divergent and compliant behaviors result from the same cognitive mechanisms, are both interesting and tendentious. We regard these matters as entirely empirical and, in many cases, open. In any case, however, one should not confuse the rule-implicit/rule-explicit distinction with the distinction between Classical and Connectionist architecture.[33]

This confusion is just ubiquitous in the Connectionist literature: it is universally assumed by Connectionists that Classical models are committed to claiming that regular behaviors must arise from explicitly encoded rules. But this is simply untrue. Not only is there no reason why Classical models are required to be rule-explicit but—as a matter of fact—arguments over which, if any, rules are explicitly mentally represented have raged for decades *within* the Classicist camp. (See, for relatively recent examples, the discussion of the explicitness of grammatical rules in Stabler, 1985, and replies; for a philosophical discussion, see Cummins, 1983.) The one thing that Classical theorists do agree about is that it *can't* be that *all* behavioral regularities are determined by explicit rules; at least some of the causal determinants of compliant behavior *must* be *im*plicit. (The arguments for this parallel Lewis Carroll's observations in "What the Tortoise Said to Achilles"; see Carroll 1956.) All other questions of the explicitness of rules are viewed by Classicists as moot; and every shade of opinion on the issue can be found in the Classicist camp.

The basic point is this: not all the functions of a Classical computer can be encoded in the form of an explicit program; some of them must be wired in. In fact, the entire program can be hard-wired in cases where it does not need to modify or otherwise examine itself. In such cases, Classical machines can be *rule implicit* with respect to their programs, and the mechanism of their state transitions is entirely subcomputational (i.e., subsymbolic).

[33]An especially flagrant example of how issues about architecture get confused with issues about the explicitness of rules in the Connectionist literature occurs in PDP, Chapter 4, where Rumelhart and McClelland argue that PDP models provide "... a rather plausible account of how we can come to have innate 'knowledge'. To the extent that stored knowledge is assumed to be in the form of explicit, inaccessible rules ... it is hard to see how it could 'get into the head' of the newborn. It seems to us implausible that the newborn possesses elaborate symbol systems and the systems for interpreting them required to put these explicit, inaccessible rules to use in guiding behavior. On our account, we do not need to attribute such complex machinery. If the innate knowledge is simply the prewired connections, it is encoded from the start in just the right way to be of use by the processing mechanisms." (p. 42). A priorizing about what it does and doesn't seem likely that newborns possess strikes us as a bad way to do developmental cognitive psychology. But Rumelhart and McClelland's argument is doubly beside the point since a Classicist who shares their prejudices can perfectly well avail himself of the same solution that they endorse. Classical architecture does *not* require "complex machinery" for "interpreting" explicit rules since classical machines do not *require* explicit rules at all. Classical architecture is therefore *neutral* on the Empiricism/Nativism issue (and so is Connectionism, as Rumelhart and McClelland elsewhere correctly remark).

What *does* need to be explicit in a Classical machine is not its program but the symbols that it writes on its tapes (or stores in its registers). These, however, correspond not to the machine's rules of state transition but to its data structures. Data structures are *the objects that the machine transforms, not the rules of transformation.* In the case of programs that parse natural language, for example, Classical architecture requires the explicit representation of the structural descriptions of sentences, but is entirely neutral on the explicitness of grammars, contrary to what many Connectionists believe.

One of the important inventions in the history of computers—the stored-program computer—makes it *possible* for programs to take on the role of data structures. But nothing in the architecture *requires* that they always do so. Similarly, Turing demonstrated that there exists an abstract machine (the so-called Universal Turing Machine) which can simulate the behavior of any target (Turing) machine. A Universal machine is "rule-explicit" about the machine it is simulating (in the sense that it has an explicit representation of that machine which is sufficient to specify its behavior uniquely). Yet the target machine can perfectly well be "rule-implicit" with respect to the rules that govern *its* behavior.

So, then, you can't attack Classical theories of cognitive architecture by showing that a cognitive process is rule-implicit; Classical architecture *permits* rule-explicit processes but does *not* require them. However, you *can* attack Connectionist architectures by showing that a cognitive process is rule *explicit* since, by definition, Connectionist architecture precludes the sorts of logico-syntactic capacities that are required to encode rules and the sorts of executive mechanisms that are required to apply them.[34]

If, therefore, there should prove to be persuasive arguments for rule explicit cognitive processes, that would be very embarrassing for Connectionists. A natural place to look for such arguments would be in the theory of the acquisition of cognitive competences. For example, much traditional work in linguistics (see Prince & Pinker, 1988) and all recent work in mathematical learning theory (see Osherson, Stov, & Weinstein, 1984), assumes that the characteristic output of a cognitive acquisition device is a recursive rule system (a grammar, in the linguistic case). Suppose such theories prove to be well-founded; then that would be incompatible with the assumption that the cognitive architecture of the capacities acquired is Connectionist.

[34]Of course, it *is* possible to simulate a "rule explicit process" in a Connectionist network by first implementing a Classical architecture in the network. The slippage between networks as architectures and as implementations is ubiquitous in Connectionist writings, as we remarked above.

On "Brain style" modeling

The relation of Connectionist models to neuroscience is open to many interpretations. On the one hand, people like Ballard (1986), and Sejnowski (1981), are explicitly attempting to build models based on properties of neurons and neural organizations, even though the neuronal units in question are idealized (some would say more than a little idealized: see, for example the commentaries following the Ballard, 1986, paper). On the other hand, Smolensky (1988) views Connectionist units as mathematical objects which can be given an interpretation in either neural or psychological terms. Most Connectionists find themselves somewhere in between, frequently referring to their approach as "brain-style" theorizing.[35]

Understanding both psychological principles *and* the way that they are neurophysiologically implemented is much better (and, indeed, more empirically secure) than only understanding one or the other. That is not at issue. The question is whether there is anything to be gained by designing "brain style" models that are uncommitted about how the models map onto brains.

Presumably the point of "brain style" modeling is that theories of cognitive processing should be influenced by the facts of biology (especially neuroscience). The biological facts that influence Connectionist models appear to include the following: neuronal connections are important to the patterns of brain activity; the memory "engram" does not appear to be spatially local; to a first approximation, neurons appear to be threshold elements which sum the activity arriving at their dendrites; many of the neurons in the cortex have multidimension "receptive fields" that are sensitive to a narrow range of values of a number of parameters; the tendency for activity at a synapse to cause a neuron to "fire" is modulated by the frequency and recency of past firings.

Let us suppose that these and similar claims are both true and relevant to the way the brain functions—an assumption that is by no means unproblematic. The question we might then ask is: What follows from such facts that is relevant to inferring the nature of the cognitive architecture? The unavoidable answer appears to be, very little. That's not an a priori claim. The degree of relationship between facts at different levels of organization of a system is an empirical matter. However, there is reason to be skeptical about whether the sorts of properties listed above are reflected in any more-or-less direct

[35]The PDP Research Group views its goal as being "to replace the 'computer metaphor' as a model of the mind with the 'brain metaphor' ..." (Rumelhart & McClelland, 1986a, Ch. 6, p. 75). But the issue is not at all which metaphor we should adopt; metaphors (whether 'computer' or 'brain') tend to be a license to take one's claims as something less than serious hypotheses. As Pylyshyn (1984a) points out, the claim that the mind has the architecture of a Classical computer is not a metaphor but a literal empirical hypothesis.

way in the structure of the system that carries out reasoning.

Consider, for example, one of the most salient properties of neural systems: they are networks which transmit activation culminating in state changes of some quasi-threshold elements. Surely it is not warranted to conclude that reasoning consists of the spread of excitation among representations, or even among semantic components of representations. After all, a VAX is also correctly characterized as consisting of a network over which excitation is transmitted culminating in state changes of quasi-threshold elements. Yet at the level at which it processes representations, a VAX is *literally* organized as a Von Neumann architecture.

The point is that the structure of "higher levels" of a system are rarely isomorphic, or even similar, to the structure of "lower levels" of a system. No one expects the theory of protons to look very much like the theory of rocks and rivers, even though, to be sure, it is protons and the like that rocks and rivers are 'implemented in'. Lucretius got into trouble precisely by assuming that there must be a simple correspondence between the structure of macrolevel and microlevel theories. He thought, for example, that hooks and eyes hold the atoms together. He was wrong, as it turns out.

There are, no doubt, cases where special empirical considerations suggest detailed structure/function correspondences or other analogies between different levels of a system's organization. For example, the input to the most peripheral stages of vision and motor control *must* be specified in terms of anatomically projected patterns (of light, in one case, and of muscular activity in the other); and independence of structure and function is perhaps less likely in a system whose input or output must be specified somatotopically. Thus, at these stages it is reasonable to expect an anatomically distributed structure to be reflected by a distributed functional architecture. When, however, the cognitive process under investigation is as abstract as reasoning, there is simply no reason to expect isomorphisms between structure and function; as, indeed, the computer case proves.

Perhaps this is all too obvious to be worth saying. Yet it seems that the commitment to "brain style" modeling leads to many of the characteristic Connectionist claims about psychology, and that it does so via the implicit—and unwarranted—assumption that there ought to be similarity of structure among the different levels of organization of a computational system. This is distressing since much of the psychology that this search for structural analogies has produced is strikingly recidivist. Thus the idea that the brain is a neural network motivates the revival of a largely discredited Associationist psychology. Similarly, the idea that brain activity is anatomically distributed leads to functionally distributed representations for concepts which in turn leads to the postulation of microfeatures; yet the inadequacies of feature-

based theories of concepts are well-known and, to our knowledge, micro-feature theory has done nothing to address them (see Bolinger, 1965; J.D. Fodor, 1977). Or again, the idea that the strength of a connection between neurons is affected by the frequency of their co-activation gets projected onto the cognitive level. The consequence is a resurgence of statistical models of learning that had been widely acknowledged (both in Psychology and in AI) to be extremely limited in their applicability (e.g., Minsky & Papert, 1972, Chomsky, 1957).

So although, *in principle*, knowledge of how the brain works could direct cognitive modeling in a beneficial manner, *in fact* a research strategy has to be judged by its fruits. The main fruit of "brain style modeling" has been to revive psychological theories whose limitations had previously been pretty widely appreciated. It has done so largely because assumptions about the structure of the brain have been adopted in an all-too-direct manner as hypotheses about cognitive architecture; it's an instructive paradox that the current attempt to be thoroughly modern and 'take the brain seriously' should lead to a psychology not readily distinguishable from the worst of Hume and Berkeley. The moral seems to be that one should be deeply suspicious of the heroic sort of brain modeling that purports to address the problems of cognition. We sympathize with the craving for biologically respectable theories that many psychologists seem to feel. But, given a choice, truth is more important than respectability.

Concluding comments: Connectionism as a theory of implementation

A recurring theme in the previous discussion is that many of the arguments for Connectionism are best construed as claiming that cognitive architecture is *implemented* in a certain kind of network (of abstract "units"). Understood this way, these arguments are neutral on the question of what the cognitive architecture is.[36] In these concluding remarks we'll briefly consider Connectionism from this point of view.

Almost every student who enters a course on computational or information-processing models of cognition must be disabused of a very general mis-

[36]Rumelhart and McClelland maintain that PDP models are more than *just* theories of implementation because (1) they add to our understanding of the problem (p. 116), (2) studying PDPs can lead to the postulation of different macrolevel processes (p. 126). Both these points deal with the heuristic value of "brain style" theorizing. Hence, though correct in principle, they are irrelevant to the crucial question whether Connectionism is best understood as an attempt to model neural implementation, or whether it really does promise a "*new* theory of the mind" incompatible with Classical information-processing approaches. It is an empirical question whether the heuristic value of this approach will turn out to be positive or negative. We have already commented on our view of the recent history of this attempt.

understanding concerning the role of the physical computer in such models. Students are almost always skeptical about "the computer as a model of cognition" on such grounds as that "computers don't forget or make mistakes", "computers function by exhaustive search," "computers are too logical and unmotivated," "computers can't learn by themselves; they can only do what they're told," or "computers are too fast (or too slow)," or "computers never get tired or bored," and so on. If we add to this list such relatively more sophisticated complaints as that "computers don't exhibit graceful degradation" or "computers are too sensitive to physical damage" this list will begin to look much like the arguments put forward by Connectionists.

The answer to all these complaints has always been that the *implementation*, and all properties associated with the particular realization of the algorithm that the theorist happens to use in a particular case, is irrelevant to the psychological theory; only the algorithm and the representations on which it operates are intended as a psychological hypothesis. Students are taught the notion of a "virtual machine" and shown that *some* virtual machines *can* learn, forget, get bored, make mistakes and whatever else one likes, providing one has a theory of the origins of each of the empirical phenomena in question.

Given this principled distinction between a model and its implementation, a theorist who is impressed by the virtues of Connectionism has the option of proposing PDP's as theories of implementation. But then, far from providing a revolutionary new basis for cognitive science, these models are in principle neutral about the nature of cognitive processes. In fact, they might be viewed as advancing the goals of Classical information processing psychology by attempting to explain how the brain (or perhaps some idealized brain-like network) might realize the types of processes that conventional cognitive science has hypothesized.

Connectionists do sometimes explicitly take their models to be theories of implementation. Ballard (1986) even refers to Connectionism as "the implementational approach". Touretzky (1986) clearly views his BoltzCONS model this way; he uses Connectionist techniques to implement conventional symbol processing mechanisms such as pushdown stacks and other LISP facilities.[37]

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[37]Even in this case, where the model is specifically designed to implement Lisp-like features, some of the rhetoric fails to keep the implementation-algorithm levels distinct. This leads to talk about "emergent properties" and to the claim that even when they implement Lisp-like mechanisms, Connectionist systems "can compute things in ways in which Turing machines and von Neumann computers can't." (Touretzky, 1986). Such a claim suggests that Touretzky distinguishes different "ways of computing" not in terms of different algorithms, but in terms of different ways of implementing the same algorithm. While nobody has proprietary rights to terms like "ways of computing", this is a misleading way of putting it; it means that a DEC machine has a "different way of computing" from an IBM machine even when executing the identical program.

Rumelhart and McClelland (1986a, p. 117), who are convinced that Connectionism signals a radical departure from the conventional symbol processing approach, nonetheless refer to "PDP implementations" of various mechanisms such as attention. Later in the same essay, they make their position explicit: Unlike "reductionists," they believe "... that new and useful concepts emerge at different levels of organization". Although they then defend the claim that one should understand the higher levels "... through the study of the interactions among lower level units", the basic idea that there *are* autonomous levels seems implicit everywhere in the essay.

But once one admits that there really are cognitive-level principles distinct from the (putative) architectural principles that Connectionism articulates, there seems to be little left to argue about. Clearly it is pointless to ask whether one should or shouldn't do cognitive science by studying "the interaction of lower levels" as opposed to studying processes at the cognitive level since we surely have to do *both*. Some scientists study geological principles, others study "the interaction of lower level units" like molecules. But since the fact that there are genuine, autonomously-stateable principles of geology is never in dispute, people who build molecular level models do not claim to have invented a "new theory of geology" that will dispense with all that old fashioned "folk geological" talk about rocks, rivers and mountains!

We have, in short, no objection at all to networks as potential implementation models, nor do we suppose that any of the arguments we've given are incompatible with this proposal. The trouble is, however, that if Connectionists do want their models to be construed this way, then they will have to radically alter their practice. For, it seems utterly clear that most of the Connectionist models that have actually been proposed must be construed as theories of cognition, not as theories of implementation. This follows from the fact that it is intrinsic to these theories to ascribe representational content to the units (and/or aggregates) that they postulate. And, as we remarked at the beginning, a theory of the relations among representational states is ipso facto a theory at the level of cognition, not at the level of implementation. It has been the burden of our argument that when construed as a cognitive theory, rather than as an implementation theory, Connectionism appears to have fatal limitations. The problem with Connectionist models is that all the reasons for thinking that they might be true are reasons for thinking that they couldn't be *psychology*.

Conclusion

What, in light of all of this, are the options for the further development of Connectionist theories? As far as we can see, there are four routes that they could follow:

(1) Hold out for unstructured mental representations as against the Classical view that mental representations have a combinatorial syntax and semantics. Productivity and systematicity arguments make this option appear not attractive.

(2) Abandon network architecture to the extent of opting for structured mental *representations* but continue to insist upon an Associationistic account of the nature of mental *processes*. This is, in effect, a retreat to Hume's picture of the mind (see footnote 29), and it has a problem that we don't believe can be solved: Although mental representations are, on the present assumption, structured objects, *association is not a structure sensitive relation.* The problem is thus how to reconstruct the semantical coherence of thought without postulating psychological processes that are sensitive to the structure of mental representations. (Equivalently, in more modern terms, it's how to get the causal relations among mental representations to mirror their semantical relations without assuming a proof-theoretic treatment of inference and—more generally—a treatment of semantic coherence that is syntactically expressed, in the spirit of proof-theory.) This is the problem on which traditional Associationism foundered, and the prospects for solving it now strike us as not appreciably better than they were a couple of hundred years ago. To put it a little differently: if you need structure in mental representations anyway to account for the productivity and systematicity of minds, why not postulate mental processes that are structure sensitive to account for the coherence of mental processes? Why not be a Classicist, in short.

In any event, notice that the present option gives the Classical picture a lot of what it wants: viz., the identification of semantic states with relations to structured arrays of symbols and the identification of mental processes with transformations of such arrays. Notice too that, as things now stand, this proposal is Utopian since there are no serious proposals for incorporating syntactic structure in Connectionist architectures.

(3) Treat Connectionism as an implementation theory. We have no principled objection to this view (though there are, as Connectionists are discovering, technical reasons why networks are often an awkward way to implement Classical machines). This option would entail rewriting quite a lot of the polemical material in the Connectionist literature, as well as redescribing

what the networks are doing as operating on symbol structures, rather than spreading activation among semantically interpreted nodes.

Moreover, this revision of policy is sure to lose the movement a lot of fans. As we have pointed out, many people have been attracted to the Connectionist approach because of its promise to (a) do away with the symbol level of analysis, and (b) elevate neuroscience to the position of providing evidence that bears directly on issues of cognition. If Connectionism is considered simply as a theory of how cognition is neurally implemented, it may constrain cognitive models no more than theories in biophysics, biochemistry, or, for that matter, quantum mechanics do. All of these theories are also concerned with processes that *implement* cognition, and all of them are likely to postulate structures that are quite different from cognitive architecture. The point is that 'implements' is transitive, and it goes all the way down.

(4) Give up on the idea that networks offer (to quote Rumelhart & McClelland, 1986a, p. 110) "... a reasonable basis for modeling cognitive processes in general". It could still be held that networks sustain *some* cognitive processes. A good bet might be that they sustain such processes as can be analyzed as the drawing of statistical inferences; as far as we can tell, what network models really are is just analog machines for computing such inferences. Since we doubt that much of cognitive processing does consist of analyzing statistical relations, this would be quite a modest estimate of the prospects for network theory compared to what the Connectionists themselves have been offering.

This is, for example, one way of understanding what's going on in the argument between Rumelhart and McClelland (1986b) and Prince and Pinker (1988), though neither paper puts it in quite these terms. In effect, Rumelhart and McClelland postulate a mechanism which, given a corpus of pairings that a 'teacher' provides as data, computes the statistical correlation between the phonological form of the ending of a verb and the phonological form of its past tense inflection. (The magnitude of the correlations so computed is analogically represented by the weights that the network exhibits at asymptote.) Given the problem of inflecting a new verb stem ending in a specified phonological sequence, the machine chooses the form of the past tense that was most highly correlated with that sequence in the training set. By contrast, Prince and Pinker argue (in effect) that more must be going on in learning past tense morphology than merely estimating correlations since the statistical hypothesis provides neither a close fit to the ontogenetic data nor a plausible account of the adult competence on which the ontogenetic processes converge. It seems to us that Pinker and Prince have, by quite a lot, the best of this argument.

There is an alternative to the Empiricist idea that all learning consists of a kind of statistical inference, realized by adjusting parameters; it's the Rationalist idea that some learning is a kind of theory construction, effected by framing hypotheses and evaluating them against evidence. We seem to remember having been through this argument before. We find ourselves with a gnawing sense of deja vu.

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Résumé

Cet article étudie les différences entre modèles connectionistes et modèles classiques de la structure cognitive. Nous pensons que, bien que les deux types de modèles stipulent l'existence d'états mentaux représentationnels, la différence essentielle est que seuls les modèles classiques requièrent l'existence d'un niveau de représentation symbolique—un "langage de la pensée"—, c'est-à-dire d'états représentationnels possédant une structure syntaxique et sémantique. Nous examinons ensuite différents arguments qui militent en faveur de l'existence de représentations mentales ayant ces propriétés. Certains de ces arguments reposent sur la "systématicité" des représentations mentales, c'est-à-dire sur le fait que les capacités cognitives exhibent toujours certaines symétries, de sorte que la capacité d'entretenir certaines pensées implique la capacité d'entretenir d'autres pensées apparentées par leur contenu sémantique. Nous pensons que ces arguments montrent de manière convainquante que l'architecture de l'esprit/du cerveau n'est pas connectioniste au niveau cognitif. Nous nous demandons ensuite s'il est possible d'interpréter le connectionisme comme une analyse des structures neuronales (ou des structures neurologiques "abstraites") dans lesquelles est réalisée l'architecture cognitive classique. Nous examinons plusieurs des arguments avancés habituellement en défense du connectionisme, et en concluons que ceux-ci n'ont de sens que dans cette interprétation.