Comparative Pattern Recognition Studies and the FLN: The Case for Careful Restraint

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I

Hauser, Chomskey and Fitch (hereafter HCF) [4] urge that comparative studies of the cognitive capacities of humans and nonhumans are crucial to our coming to understand the nature and evolutionary history of the human capacity for language-use. I suspect this is true; studies cut from the mold of Fitch and Hauser's study [2] of computational constraints on syntactic processing in cotton-top tamarins will be the source of much reliable evidence for reasoning about the evolution of the FLN. Nonetheless I here argue that the utility of comparative aural pattern recognition studies is far more constrained than many have thought.

Contrary to an assumption underlying Fitch and Hauser's study [2], a creature's success or failure in recognizing patterns in strings of auditory signals does not straightforwardly inform us as to that creature's syntactic processing abilities. Their contrary assumption goes unchallenged in the excellent critiques of Fitch and Hauser (hereafter F&H) [2] given by Perruchet and Rey (hereafter P&R) [6] and by Rogers and Pullum (hereafter R&P) [7], and a similar assumption can be seen in the reasoning of Gentner et al [3]. It is a prima facie plausible assumption, likely to replicate itself in inevitable future aural pattern recognition experiments; therefore if in fact it does constitute mistaken methodology, it is crucial that this error be recognized and avoided. I here argue that the relationship between such pattern recognition data and linguistically relevant syntactic processing capability is more complex than has been appreciated heretofore; the conclusions that may legitimately be drawn from such data are correspondingly constrained.

II

Consider the following scenario. A flock of wild jubjubs are trapped and brought into the laboratory where they are tested, à la F&H [2], for their ability to recognize an aural pattern of grammar R, where grammar R is constituted by a set of recursive rules. The strings of R are constituted by two classes of syllables that are spoken by humans, and which we have confirmed to be individually salient and perceptually discriminable for the jubjubs. In fact, our study of the jubjubs resembles F&H's study of the tamarins precisely, with the exception that we have (somehow or other) accounted for and defused the criticisms of P&R and R&P. The jubjubs, like F&H's cotton-top tamarins, fail to learn R; after being familiarized with strings of R, they manifest no surprise when confronted with violations of R. Furthermore, we are assured of the basic reliability of our conclusion regarding their failure to learn the pattern, because a control group of jubjubs succeeded in learning nonrecursive grammar F. Given this result, can we conclude, as F&H did with their tamarins, that the jubjubs lack the cognitive mechanisms necessary to process recursive structures isomorphic to R? Unfortunately not. Three concerns block this inference.

1

Firstly, there remains the possibility that jubjubs possess a domain-specific ability to process recursion. For example, jubjubs may have the ability to comprehend recursively structured social relationships – even relationships isomorphic to R – and yet lack the cognitive means to apply this ability to patterns of just any sort of stimulus. More to the point, jubjubs may have a capacity to process recursion in *communication*. But the utterances given as stimuli were semantically empty syllables spoken by human voices. If jubjubs have a domain-specific capacity for recursion in communication, then there is no reason to suppose that capacity would be activated by the study

in question. At best, our study shows that jubjubs lack a *domain-general* capacity for recursion.

Did F&H intend to test only for domain-general recursion, or did they intend to test for the presence of any capacity for recursion, whatever the domain? The latter interpretation would surely be uncharitable; their methodology is clearly unsuited to determining whether cotton-top tamarins have, for example, a domain-specific capacity for cognizing hierarchically structured social relationships – a capacity that F&H not only recognize as possible, but suggest may have preceded the human capacity for language[2, p. 377].

Taking F&H to be arguing, as I think is most charitable, that tamarins lack domain-general recursion, one might wonder how they are justified in concluding that tamarins lack a capacity for recursion homologous to our own. After all, even if tamarins do lack domain-general recursion, doesn't this leave open the possibility that they possess a capacity for recursion specific to a communicative domain? In the case of F&H, the answer is that this possibility is *not* left open, for completely independent reasons; the view they share with Chomsky on the evolution of recursive processing ability commits them to the idea that the FLN *is* domain-general capacity for recursion. HCF [4] suggests that the capacity of recursion evolved for other than communicative benefits, and then broadened to become domain-general, at which point human language became possible. Opposed to this is the view (of such as Steven Pinker) that human syntax evolved as a specifically communicative adaptation. Under this latter view, a capacity for recursion homologous to that of humans might, for a given creature, plausibly be restricted to the domain of communication (perhaps along with some other narrow domains). HCF's view does not preclude domain-specific recursive processing, but it *does* preclude recursive processing specific to the domain of *communication*.

Thus, F&H's methodology is adequate to their conclusion only if their side of the adaptationist debate is the right one – that is, roughly speaking, if the FLN is a

spandrel and not an adaptation for communicative benefit. But surely that issue is sufficiently controversial that F&H should avoid such a commitment in their comparative methodology. At any rate, at present there is the danger that an opponent of HCF on the adaptationist issue might accept F&H's conclusion that tamarins lack the capacity for recursion in communicative domains without realizing that she has thereby tacitly presupposed that the FLN is a spandrel. Without this presupposition, our captive jubjubs – just like F&H's tamarins – may fully well possess the capacity for recursively structured communication. Despite our stipulated immunity to previous criticisms of F&H [2], as far as the data of our study are concerned, our jubjubs may speak as fluent English as any of their captors. Likewise for F&H's tamarins.

Fans of pattern recognition as a means for gauging syntactic ability, beware. None of the above denies that extremely useful data can come of such studies, but one must take care in precisely what conclusions the data warrant. Those conclusions may turn out to be narrower than many seem to have hoped; perhaps, as I argue below, even narrower than I have suggested heretofore.

2

Suppose that, sensitive to the above concerns, we retool our study (somehow or other) to render our stimuli communicative in order to test this specific domain (perhaps by correlating grammaticality with the presence of food); alternatively, suppose my above concerns are wildly misguided, and in fact F&H [2]'s methodology does constitute a test for a specifically communicative capacity for recursion. We still have the concern of whether we are testing our jubjubs with appropriate stimuli. This problem is graver for testing domain-specific than for domain-general capacities. Recall that, following F&H, our stimuli are strings consisting of two classes of syllables; one spoken by a man, the other by a woman, to enhance discriminative perceptual salience. For testing a domain-general capacity to recognize hierarchically structured patterns, this may seem

unproblematic. But if we are dealing with a capacity specific to communication, then the jubjub will require some means of determining whether a sound is communicative; that is, if the capacity only activates in response to communication, then it must have some litmus test for what constitutes communication.

For example, it is wholly plausible that, the jubjub by hypothesis possessing a module that picks apart patterns in utterances, that module will individuate utterances at least in part by individuating utterers. (If I were a language-module, that's what *I* would do.) Each string of F&H's stimuli, alternating between female voice and male voice, seems to me to evoke a terse conversation rather than a unified utterance. That doesn't interfere with my ability to discover the recursive structure of the strings; *I'm* a domain-general recursive cognizer. But the jubjubs are being tested for a domain-specific capacity for recursion, which might not be so insulated from this problem.

Nor would relying on a single utterer for our stimuli evade such concerns. If we take seriously the idea that we might be dealing with a domain-specific capacity, then we must take seriously the idea that the capacity evolved for use in the jubjub's communicative life. Plausibly, no human-sounding utterance will be treated as a communicative signal; plausibly, only the kinds of sounds uttered by the jubjub will catch the interest of that communicative homunculus (jubjunculus?) in its brain.

But relying on jubjub calls for our stimuli would bring unwelcome, interfering effects to our jubjubs' reactions. Such calls may have semantic values that affect the jubjub's behavior. If we emit three male jubjub alarm calls followed by the same jubjub's mating call, the subject jubjub may look because the string violates the familiarization stimuli's pattern, or it may look simply because it hears alarm calls, or more alarm calls than are usually given, or because it's baffled by the presence of a simultaneously terrified and flirtatious jubjub.

Whereas my earlier criticism in §II.1, like the criticisms of R&P and P&R, may be surmountable in future pattern learning studies of syntax, this one threatens in-

tractability. Of course, the criticism does not weigh against the possibility of testing for domain-general recursion, but it does suggest that such studies may be ineffective in the area of recursion specific to communicative domains. The problem, to summarize, is that to use semantically empty stimuli one must use sounds that risk failing to activate the jubjub's hypothesized communicative module, whereas to use preexisting communicative jubjub signs risks infecting one's data with unwanted variables affecting jubjub behavior.

This concludes the concerns of this paper that are based on the worry that the subject of our pattern learning study might possess a capacity for recursion specific to the domain of communication. Before moving on to the next section, I want to explore a possible objection to my §II.1 and II.2 concerns. The objection might go as follows: As criticisms of F&H, these arguments are ad hoc. F&H produce evidence that cotton-top tamarins lack the ability to process recursive structure of utterances, by way of showing that cotton-top tamarins cannot process the simplest kind of recursive structure we can find, in a format in which that syntax is practically handed to them on a silver platter. This is strong evidence that cotton-top tamarins are just inept at recursion. Your response (my response) is to say that perhaps the tamarins, though inept at this easiest sort of recursively structured task we can find, might nonetheless be great at recursion in *other* kinds of situations. Sure, they *might*, but (so the objection continues) why should we think that there's a significant chance that they in fact are?

The charge of ad hocness falls flat for two reasons. One is that, as noted above, it is a live debate whether the FLN is a spandrel or an adaptation for communicative benefit. But if it is a live option that the FLN is such an adaptation, and if (as HCF themselves argue [4]) the FLN consists (wholly or partly) in the capacity for recursion, then it is ipso facto a live option that capacity for recursion can be specific to the communicative domain. Therefore my concerns are not ad hoc.

6

The other reason the charge of ad hocness falls flat is that we have independent evidence that the capacity for recursion can occur as a domain-specific ability. Cheney and Seyfarth [1, p. 268] argue that "[b]aboons... create a nested hierarchy in which others are placed in a linear rank order and simultaneously grouped according to matrilineal kinship in a manner that preserves ranks both within and across families" [p. 268], while simultaneously holding that baboons lack the cognitive resources necessary to apply recursion to communicative utterances. They are far from alone in attributing a social domain-specific capacity for recursion to a nonhuman primate. And again, it is explicitly suggested in both HCF [4] and F&H [2] that humans themselves may have first enjoyed the capacity for recursion as specific to the domain of number, or spatial or social representations. Given that domain-specific capacities for recursion are accepted as live possibilities on all sides of these debates, it makes sense to respond to F&H with the suggestion that their study has left untouched a plausible scenario of domain-specificity that is contrary to their conclusions.

3

In this section I offer a third concern blocking that inference; a criticism of F&H's methodology (rather than a constraint on what may be concluded from their methodology). The worry is that the study fails to provide a relevantly symmetrical situation for its humans and its cotton-top tamarins; the tamarins, I argue, are given a significantly more difficult task.

We wish to compare jubjubs' and humans' abilities to learn R; therefore, we expose each group to a set of appropriate familiarization stimuli. Having gathered our data, we find that humans succeeded in the task, and jubjubs failed. Since exposure to the stimuli sufficed for the humans to learn R, we conclude that the humans have access to cognitive resources which are relevant to learning recursively structured patterns; the most plausible cognitive resource playing this role is just our capacity for domain-

general recursion. In order to reach F&H 's conclusion, we must make the intermediary conclusion that:

    i.   Mere exposure to the relevant strings *would* suffice for jubjubs to learn R if they did in fact possess the relevant domain-general capacity for recursion.

But jubjubs did not learn R. Taking this datum together with (i), we reach:

    ii. Jubjubs lack the cognitive capacity for domain-general recursion.

For any experiment methodologically mirroring F&H [2], the reasoning leading to (i) will fail – therefore, the reasoning leading to (ii) will fail. This is because the humans were given an illicit advantage in two ways.

Firstly, F&H report that the humans they studied were specifically asked to attend to the familiarization strings [2, supplement]. The tamarins were purely passively exposed to the stimuli. Secondly, the humans, aware of their situation as subjects, aware of the status of the familiarization strings as stimuli in an experiment, and aware that most likely their cognitive or perceptual abilities were being in some way tested, were in possession of background knowledge sufficient to infer that there was very likely some sort of pattern – or, at least, *something* interesting to figure out – in the stimuli.

Perhaps these methodological features seem unlikely to influence the data collected. If so, it is informative to consider what sort of changes we would have to make to guard against such influence when we study our jubjubs. We might, on the one hand, attempt to offer the jubjubs information and motivation commensurate with that available to the human subjects. On the other hand, we might seek to deprive our humans of the informational and motivational advantages that F&H's humans enjoyed over their tamarins. The latter seems like a much more feasible methodological goal; it is unclear how to achieve the former, and furthermore unclear how we would reliably determine when we *have* succeeded in achieving it. The jubjubs may lack the concepts necessary for analogous background information, or the social cognition necessary for analogous

social pressures. Furthermore, if we try to make the stimuli interesting somehow or other, we risk infecting our data with unwanted variables. If our stimuli are (wittingly or unwittingly) imbued with informative content, then not only does the nature of that content threaten to interfere with the jubjub's reactions, but we are left uncertain whether we tested for a domain-general capacity for recursion or a capacity specific to the domain of that content. For these reasons, the far likelier methodological fix is to deprive the humans of the relevant motivations and knowledge, rather than to supply it to the jubjubs. Therefore, we can construct our comparison of jubjubs and humans more symmetrically by constructing something like the following experimental scenario.

> Our human subjects are unaware that they are under study. We contrive it that our subjects are near to (and aware of) one of our jubjubs (or whatever bird we wish to substitute for them). However, we are careful not to present the jubjub (or substitute bird) as something crying out for intense observation. Concealed near the jubjub is a loudspeaker, on which we play first our familiarization strings, and then our test stimuli. The strings are composed of two classes of sounds, each constituted by a particular type of jubjub call; A calls and B calls, which are perceptually discriminable for humans. After familiarization, we play our test stimuli, recording the human's looking-time after each utterance. Amount of time spent looking at the jubjub after an utterance will be taken as evidence that the human's expectations were violated.

The above-characterized method is relevantly similar to that applied to F&H's tamarins, maintaining the symmetry that is necessary to avoid my above criticism of asymmetry and permissibly support the inference (should the data cooperate) that humans have access to capacities of recursion that jubjubs lack. However, notice that it is far from obvious (in fact it seems to me distinctly unlikely) that in this case, the humans' performance will manifest the relevant sort of pattern recognition – perhaps not even in the case of our control grammar *F*.

If, as is possible, this method of studying humans' aural pattern recognition abilities does reflect our ability to learn (both finite state and) recursively structured grammars,

and our jubjubs fail their version of the study, then we may indeed permissibly conclude that humans possess cognitive capacities of recursion that jubjubs lack. On the other hand, if, as seems highly plausible, humans' recursive abilities are not reflected in these data, then we must conclude that the method is insensitive to the presence or absence of a domain-general capacity for recursion (which humans clearly possess). This is perhaps unsurprising; after all, plausibly, reliable detection of a capacity for recursion requires that the subject's attention be directed to the stimulus in just the sort of way that F&H directed their humans to attend to the stimuli. Pattern detection, especially for recursive patterns, is probably not a cognitively cheap activity; it is unrealistic to expect such a capacity to apply itself constantly to all sensory input. Therefore, to sum up the situation at this point: if the above-characterized method agrees with the data F&H gathered on their humans, then and only then are we licensed to draw the kinds of conclusions F&H draw concerning domain-general capacities for recursion from such data. Anyone moved by HCF's endorsement of comparative pattern learning studies therefore has powerful reasons to hope that a such a revised methodology can successfully create a relevant symmetry for humans and nonhumans while confirming humans' capacities for recursion. Whether this hope can be borne out remains to be seen. Much depends on how reliably human recursive capacities would be reflected in the data of studies in which subjects are deprived of the relevant background information and motivation – in other words, when they are placed on equal footing with F&H 's tamarins.

III

HCF are persuasive in their recommendations of a comparative approach to studying the FLN and the FLB, and pattern-learning studies are a crucial tool in that kit. Certainly it won't be long until some nonhuman species becomes the next jubjub.

Much time, effort and energy of researchers and jubjubs might be wasted if such studies are undertaken without a clear idea of what they are capable of supporting, what they are incapable of supporting, and how thoroughly they interact with presuppositions like F&H's commitment to the view of the FLN as a spandrel. I have attempted to point out ways that the methodology and conclusions of studies in the vein of F&H [2] and P&R [6] should be constrained in the future. F&H's hidden, illicit assumption of the FLN as a spandrel should at least be made explicit and at best avoided. Avoiding that assumption has the consequence that this sort of study may be unable to reliably indicate the presence or absence of a communicative domain-specific capacity for recursion. Independent of that issue, reliable results from any such comparative study must enforce relevant symmetry on the situations of its human and nonhuman subjects. Further study is required to probe how debilitating this constraint is. I have indicated that the damage may potentially be extensive indeed, possibly even to the point of rendering untrustworthy any attempt to use comparative pattern learning studies to access the nature or evolutionary history of the FLN.

I hope, however, that I have also helped to point the way to avoiding these heavy costs, and how to navigate a way to reliably connect performance in pattern recognition to the relevant cognitive capacities. However, while I do not share Richard Lewontin's utter pessimism [5] about our ability to improve our understanding of the evolutionary history of cognitive mechanisms, I agree with his sentiment that we must be prepared to accept that a certain path of inquiry may simply be blocked, no matter how interesting the questions involved and how badly we wish to answer them. Sensitivity to the concerns I have expressed here will help to maximize researchers' ability to proceed along those paths.

References

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