Disorderly nature

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Sabbagh & Gelman have written an unusually thorough and careful commentary, and furthermore, I agree with virtually everything they say. Most of what I want to do in this commentary (given the space) is to try to situate these issues in a broader perspective.

To start with, I'll assume that my sympathy for statistical analytic processes as an important part of children's language acquisition is well-established (Maratsos & Chalkley, 1980; Maratsos, 1998), even if I think there are qualifications and limits of some sorts that are necessary (Maratsos, 1998). Simultaneously, I strongly agree with the implications of many of Sabbagh & Gelman's remarks: these mechanisms may be variously biased or specially set for various activities, and probably are not the only thing going on, either.

I think, aside from methodological and empirical necessity, the obvious, important, emotional-ideological influence on much of this work was the polar opposing influence of Chomsky's largely anti-experience doctrines. By the usual laws of oppositional thinking, they encourage one to think of a veridical, accurate experiential reflector that takes in experience impartially (an 'inductive vacuum cleaner' in the words of Maratsos & Chalkley, 1980), and has a basically standard, 'objective' means of using the predictive patterns implicit in the input. This is why the fact that connectionist systems seem to have to be somewhat specially wired for each learning task seems opposed to the basic thrust: the basic thrust is to be 'veridical' (accurately reflecting input) and 'non-biased' in any way, in as pure an opposition to Chomsky as can be imagined.

My own feeling of the limits of this point of view began while working on Maratsos & Chalkley (1980), especially in trying to deal with grammatical relations like grammatical subject (which I still think are useful concepts in dealing with most languages). This section of the paper, which with good reason is rarely cited, is confused in an underlying way. This is because it seemed impossible to deal with these grammatical relations without having children impose something like meaningful predicate-argument analysis on the data. What is proposed there is really a kind of distributional analysis of the properties of arguments of predicates; but the basic notion of predicate and argument is not itself particularly 'objective' or 'statistical' looking, even if one can do statistics on it. Nor have grammatical relations received

much subsequent treatment in the statistical-distributional literature, which copes much more easily with word classes like verb.

Predicate–argument analysis really was an *ad hoc* intrusion into the framework of the paper, yet it seemed to us that grammatical relations (and in the end many hierarchical relations) depended on it. (I also found myself not being satisfied that the distributed-looking analyses at the end of that chapter, which have a family resemblance to later connectionist analyses, could deal with operations on hierarchical units, such as relativization operations on embedded sentences. This still seems to me a completely severe problem, despite enterprising simulations of aspects of the problem by Elman).

In the 1980's, having thought myself into various binds while trying to analyse these issues further, I spent a good deal of time reading more widely in evolutionary, animal, and cognitive and social psychological literature, hoping that broader perspectives might suggest how to resolve some problems in thinking about grammar. These readings did not afford me the same epiphanetic resolutions they seem to provide some of our prominent scholars. While the world of evolution is a world of outcomes that work well enough, it is not a world of elegantly designed, uniformly implemented mechanisms. Basically, evolution seems to be a haphazard process in which anything of any nature that happens to fit with what is there, and works well enough, is acceptable (See Lewin, 1984). There is very little premium for design elegance or uniformity. Speaking with the Chair of Cambridge's experimental psychology department, an animal comparative psychologist, I said the animal and evolutionary literature looked to me as though there were very few constraints on what a system such as language or grammar could be like: what could be innate or not; how mechanisms and processes could be structured or mixed or not. That's what he thought. In social and cognitive psychology, I also find little evidence of a highly accurate, veridical organism whose natural tendency is to reflect accurately the statistical nature of the input, at least in many problems. Most child psychologists in social development, for example, are anti-nativists (this is ideological, I think). But I have asked them whether they think idealization is a common social phenomenon ('my mother was wonderful'; 'my country is wonderful'; 'my love is perfect'), and of course they agree. But idealization is by definition not a veridical reflection of the input; it is a distortion. Yet it is one of the basic human social interpretative mechanisms. It must be innate. Group phenomena are not 'veridical.' Much adult cognitive psychology is largely devoted to inaccuracies in probabilistic analysis of input information (e.g. Kahnemann, Slovic & Tversky, 1982). Perceptual systems include neurons which have the job of exaggerating light-dark contrasts. The list goes on a long time.

In fact, my general reading of these data was the following paradoxical

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one: people who like the idea of an input-veridical, statistical language-learning child are generally trying to 'save' the child from faculty-specificity. But in fact, if children learn language by doing such highly neutral, statistically veridical analyses of the input, this apparently would make language acquisition unique among other human faculties, which generally show moderate to heavy amounts of selectivity and bias. Sabbagh & Gelman, in fact, clearly incline to a partly disorderly use of statistics: different biases for different tasks. Furthermore, they strongly suggest, the world of acquisition mechanisms may not be just statistical learning vs. innate pre-knowledge. As is true in the animal literature, they suggest there might be partly experience-open, partly biased mechanisms for how construction and learning are done in particular tasks. More mess. But my reading of the broader literature (unless language really is unique) is that nature does not care about our natural fussiness (partly a function of our limited consciousness, which cannot deal with too many things at once). It probably does not care about whether grammar is all innate, all 'learned,' or some partly *ad hoc* mixture that works well enough.

This makes life methodologically and interpretively less convenient for us than it might; one might argue that polarization makes research. But in the end, methodological, intellectual, and social convenience should not be confused for reasonable Bayesian attempts to estimate truth, however uncertainly.

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