Subjectively, my experience of life is one of having to make a series of choices between given alternatives and it is this experience of doubt, indecision, temptation, that seems more important and memorable than the actions I take. Further, if I make a choice which I consider the wrong one, I can never believe, no matter how strong the temptation to make it, that it was inevitable, that I could not and should not have made the opposite choice. But when I look at others, I cannot see them making choices; I can only see what they actually do and, if I know them well, it is rarely that I am surprised, that I could not have predicted, given his character and upbringing, how so-and-so would behave.


What Auden says about individuals is true also of research programs. Those who participate in one and who are convinced of its general correctness may cite certain achievements of the program in its support; but more important to them is its potential. Critics outside the program, however, see only what has actually been done. “Look,” they say, “the major claim of the program has already been decisively disconfirmed. Now do you admit that this program has been misguided and is going nowhere?” “Not at all!,” reply the insiders, “The claim you mention (a) has not been disconfirmed, (b) has already been revised to overcome your (mainly trivial) objections, (c) is only a working hypothesis that is leading us to ask interesting questions, and (d) is not that central to our program anyway.”

Questions about why individual researchers make the choices they do, though quite important to the actual conduct of a scientific field, tend to be the province of historians and philosophers of science. The practitioners discuss them, it is usually over a beer after a conference, not in the pages of a journal. But over three recent issues of Natural Language & Linguistic Theory (NLLT) we find a remarkable exchange about just these questions.

The trigger for this discussion was an incendiary Topic...Comment piece by Lappin, Levine and Johnson (2000a) (henceforth “LLJ”) titled “The structure of unscientific revolutions.” LLJ propose to discuss (2000a: 665) “a number of important issues concerning the nature of theory construction and scientific discussion in the field.” They have come to the conclusion that there is, as they state in their first reply to critics, LLJ (2000b: 873), “a disturbing situation that holds in much of the field.”

I have to confess that, too, from time to time have felt that there is something wrong with the nature of scientific discussion in the field. I feel this most acutely when I read an article on a topic I have worked on that doesn’t cite me. “Something is wrong,” I think, as I scan the reference list that has no names between Drachmann and Dressler, “with the nature of scientific discussion in the field.” Or, when I do find my name cited, only to see that it is in a footnote that reads in its entirety, “See also Dresher for another view.” or “Despite Dresher.” or even “Pace Dresher.”, that’s when I tend to reflect on what appears to me to be a disturbing lack of scholarly standards in the part of the field I work in. At such times, I can’t help but think that things must be different in mathematics or theoretical physics.
So for me, my complaints about the nature of scientific discussion in the field can be simply summarized as, “Why don’t you cite me?” But since everybody else apart from myself seems to be cited sufficiently—indeed, more than they deserve—I don’t find there to be anything particularly wrong with the field, setting aside for the moment my own case.

But that’s just me. I have no doubt that others, who have thought more deeply about these issues, are able to take a wider, more inclusive view of the matter. Thus, LLJ (2000a: 667) characterize the problem in the field as follows: “What is altogether mysterious from a purely scientific point of view is the rapidity with which a substantial number of investigators, who had significant research commitments in the Government-Binding [GB] framework, have abandoned that framework, and much of its conceptual inventory, virtually overnight” in favour of the minimalist program (MP), a program that LLJ find to be no better than, and in many ways inferior to, GB. They conclude that these investigators have adopted MP not for scientific reasons, but “simply on the basis of Chomsky’s personal authority.” They conclude that at least a portion of the field “is labouring under the manufacture of consent.”

At first glance one might suppose that LLJ are GB supporters who would like to see a return to GB. This is not the case at all. They write (LLJ 2000b: 873) that it was not their intention to defend GB, nor do any of them work regularly in that model. Rather, their work in syntax has tended to be in HPSG; in addition, between them they have published widely in semantics, computational linguistics, and natural language processing.

Now, if I had the theoretical views that LLJ have, and if I were the author of the critique of MP contained in Johnson and Lappin 1997 and 1999, I wouldn’t have asked why GB researchers abandoned GB for MP; I would wonder why they didn’t all abandon GB and MP in favour of, say, HPSG. But that’s me, and so it would just be a long-winded version of “Why don’t you cite me?”

LLJ are more advanced than that. In the conclusion of LLJ 2000a: 670, they write, “It should come as no surprise, then, that linguists who exhibit so little respect for the basic theoretical assumptions that guide their own work should be relatively immune from interest in work done in alternative theoretical paradigms.” In other words, their question is not “Why don’t you cite me?” but rather “Why don’t you cite yourselves?”

Now, to a small-minded observer such as myself, to whom discussion of scientific practice in the field always comes down to “Why don’t you cite me?”, LLJ’s twist on this question would appear to have a tinge of irony. After all, they begin by observing (2000a: 665) that “GB represented a move towards the constraint-based approach that characterizes many non-derivational theories, such as HPSG and LFG.” MP, in their view, is a move away from such approaches. So, if in some sense GB stands in for the sort of theories they work in, the question “Why don’t you cite yourselves?” serves as a stand-in for “Why don’t you cite me?” Therefore, if I were a GB-turned-MP syntactician, I would have interpreted their piece as asking, “Why aren’t you doing HPSG?”

But that’s just me. The syntacticians who took up LLJ’s challenge all answered the question that was posed. That is, they all presented reasons why they moved from GB to MP. For Holmberg (2000), it had to do with problems in the GB account of Tough-movement, as well as the MP treatment of fully inflected forms, which makes for a better analysis of word order in Scandinavian. Reuland (2000) stresses the continuity between GB and MP, so that moving from one to another does not entail “abandoning” GB or its results. He also (2000: 844–845) points to what he calls the “heuristic power” of a program or theory, its ability to inspire researchers to search for new solutions. Heuristic power “is a property which you can recognize a theory to have only by working in it.” Similarly Roberts (2000), Piattelli-Palmarini (2000), and Uriagereka (2000) summarize the processes by which they adopted elements of MP.

LLJ (2000b) are not mollified by these explanations. Perhaps their critics were answering the wrong question after all. Thus, with respect to Homberg’s problem with Tough-movement, LLJ (2000b: 886) write, “Holmberg never seems to entertain the possibility that the problem he notes is
avoided in most major non-derivational theoretical frameworks like LFG, GPSG, HPSG, CG, and TAG”. Scandinavian fully inflected forms in the lexicon? “This has been a standard feature of local-constraint grammars like LFG, GPSG, and HPSG since at least the early 1980s...” And so on. A second round ensued a year later (Reuland 2001, Roberts 2001, Uriagereka 2001, and LLJ 2001), but little was settled there.

I don’t know how many other theories Holmberg considered when he was reflecting on Tough-movement and Scandinavian word order. Here is a possible topic for future issues of NLLT, perhaps. But I’m not sure that our job requires him, or anyone else, to account for the paths he didn’t follow. If there are better analyses than his to be had, there is nothing keeping them from being published.

Is there, then, a problem with the sociology of the field? Well, none of the participants in this debate cited me, so to that extent, yes, the crisis continues. But that just has to do with me. More positively, I think the exchange has shown on the contrary a rather sophisticated grasp of the nature of scientific practice on the part of all concerned. There is no room here to dwell on it, but in the course of the debate a vast array of examples from the history of science were unearthed and flung around. Some of this may be “trendy ‘para-scientific’ chit-chat,” to borrow a phrase from LLJ (2000a: 667), but it sure is impressive. Based on these testimonies, it appears to me that those who moved from GB to MP may be wrong, but they are not irrational.

*     *     *

The scene is the great courtyard of the renowned MIT school of linguistics. Students and visitors from all over the world are practicing their skills. Suddenly, three masked figures dressed as ninjas appear in the centre of the courtyard and create a disturbance.

“So this is the famous MIT school of linguistics? We can see better science in any kindergarten in Palo Alto.”

A great commotion ensues. They go on, “Your notions of optimality are meaningless.”

They leap up onto a table, and pull out swords and brandish them (ffft ffft ffft!) to fend off the gathering crowd as they speak.

“Minimization/maximization principles [in physics] are derived [ffft ffft!] from deeper physical properties of the particles (waves, vectors, etc.) which satisfy them [ffft ffft ffft!]. They follow from the subatomic structure and attributes of these particles, and are not themselves basic elements of the theory [ffft!]”.

MP linguist 1 steps forward. He draws a sword and swings it about. Ffft ffft ffft ffft ffft! He lunges at the strangers.

“Radical reinterpretations of minimization principles in physics have shown [ffft!] that the minimization of trajectories in real space can be profitably salvaged abstracting to probability densities [ffft ffft!], and that minimal action conceived in terms of forces and displacements can be salvaged by abstraction to hamiltonians [ffft!] and suitable multidimensional spaces [ffft ffft!].”

The three intruders vault from the table over the heads of the linguists surrounding them. They do a sumersault off the wall of the courtyard. MP linguist 2 appears in front of them wielding a stick which he holds with two hands. Fap fap fap!

“Our principles are comparable to the Second Law of Thermodynamics [fap!] and the Slaving Principle in synergetics [fap fap!]”.

The strangers defend themselves with their own poles.

“There is no connection [fap!] between your principle of entropy and the Second Law of Thermodynamics.”

They spring up onto the roof, MP linguist 2 bounding up after them. They joust across the rooftops.
“[T]here is a connection discussed in the computational literature between information and negative entropy, and that entropy is routinely used in the life sciences, as per the advice of no less than Schrödinger.”

“You rely] on a vague analogy with thermodynamic entropy, a physical state variable S(X) involving the energies of physical particles in motion (measured in units of energy per degree of absolute temperature, e.g., joules/Kelvin). At best [you make] use only of the information-theoretic notion of entropy, H(X)... Technically, these two concepts can be related, but establishing this relation requires the introduction of temperature and Boltzmann’s constant...”

They fly into the air. MP linguist 3 flies up after them. He flings his Occam’s Razor at them. They respond with Ockham’s Razor.

“You have talent,” says MP linguist 5, “but it needs to be channeled. They should come study here.”

“What do you think that was all about?” asked MP linguist 6.

“Look,” says MP linguist 7, “they left a note.”

The MP linguists gather round to view the note. In flowing script, it reads:

“Why don’t you cite me?”

Citations

1. LLJ (2000a: 666)
3. Uriagereka (2001: 897)
4. LLJ (2001: 913–914)
5. Roberts (2001: 888)
6. LLJ (2001: 907)
7. Reuland (2001: 884)

References


