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THE METRIC OF OPEN-MINDEDNESS

Some authors, over the years, in linguistics and outside linguistics, in the United States and outside the United States, have taken pleasure in depicting Chomsky as embarrassingly surrounded by a compact international cohort of zealots, generativist integralists who pugnaciously defend the latest version of the Master's theories, only to be corrected by the Master himself the following year, when he puts forth new and radical revisions. At that point, according to these authors, the cohort momentarily sinks into disarray, then quickly regroups, and starts defending the new version with equal vehemence und unruffled compactness. Strangely enough, such authors sometimes tend to show moderate sympathy for the previous version of the theory, accusing the zealots of accepting the new switch for no reason. Well, no reason other than blind deference to the authority of the Master. Exposing this recurrent pattern allegedly proves that the whole generative enterprise is unscientific, to put it mildly. The article by Lappin, Levine, and Johnson (henceforth 'LLJ') offers precisely another such piece of indictment, artfully ignoring the rich internal dissent among generativists about many central aspects of the minimalist program, as repeatedly pointed out in Juan Uriagereka's book Rhyme and Reason (Uriagereka 1998) and in my foreword to that book (i.e. the central targets of their critique). Uriagereka's reply to LLJ takes care of the most essential points, in detail and, in my opinion, quite persuasively. Being merely the author of the foreword to Juan's book, I think it is appropriate to be brief. LLJ quote a passage of mine, in which I compare the level of analysis and the depth of the theories offered by generative grammar to those in physics and biology. Since their aim is to show that the minimalist program is (somehow) both wrong and inconsistent, the parallel that I draw with these sciences irks them beyond endurance. They react with a bizarre argument, aimed to show at the same time that (a) the parallel with physics and biology in preposterous (these are incomparably more mature disciplines than generative grammar), and (b) theories proposed by physicists and biologists are often wrong too. I do not think that these two arguments can be run



Natural Language & Linguistic Theory **18:** 859–862, 2000. © 2001 Kluwer Academic Publishers. Printed in the Netherlands. together, or at any rate not with the anti-generativist thrust that LLJ wish to impress on them, but more on this in a moment. Before we proceed I wish to stress that they omit any mention of the later sections of my foreword, which make the second of their arguments otiose. For instance, I state: "Nothing ensures that there will not be further important changes in the theory in the near future. In fact everything suggests that, happily, there will be. That's how all natural sciences advance and grow. Chomsky revealingly insists that it is to be *hoped* that linguistic theory will change the next time a bright graduate student walks into the office to discuss his or her work" (page xxxiv; emphasis in the original). This is perfectly compatible with their point (b) above, but, it seems to me, hard to reconcile with their accusations of bigotry. LLJ ends with the innuendo that, while Chomsky himself remains prudently open-minded about the ultimate destiny of the minimalist program, we (the zealots) perversely camouflage it as a final and absolute truth. But such innuendo is unsupported by what Juan and I have actually written. The very last section of my foreword, in fact, examines in some detail "what would remain of the theory even if important aspects of it one day proved to be incorrect, or only very approximately correct" (p. xxxv). The last sentence of my foreword is: "Chances are some aspect is, at least in the main, already correct" (p.xxxvi). No inordinate bigotry here either, it seems to me. As Juan amply shows in his reply, it is possible (I have to insist on the word 'possible') that the theory is, at least in the main, at least provisionally, correct. The final sections of my foreword are an exercise in deriving some consequences from this sheer possibility. Nothing anyone has shown so far, and notably nothing in LLJ, constitutes a proof that this partial, possibly temporary, success is an impossibility. LLJ's selective filtering of quotes gives the reader the false impression of an integralist defense of the present theory in its present stage. It is a defense of the possibilities that the minimalist program has opened, as far from integralism as Chomsky's own writing are.

Now to some specifics. Who would want to deny that even the greatest physicists have erred, and that physics has often progressed by overturning their theories and their insights? Why waste our time on such platitudes? The point is, rather, that in some cases even revolutions in physics have left intact some central aspects of the superseded theories. Minimization principles come to the fore precisely in this respect. Against the critique leveled by LLJ that optimization principles are only meaningful when the magnitude to be optimized is exactly pre-defined, and always left rigid, radical reinterpretations of minimization principles in physics have shown that the minimization of trajectories in real space can be profitably salvaged abstracting to probability densities, and that minimal action conceived in terms of forces and displacements can be salvaged by abstraction to hamiltonians and suitable multidimensional spaces. Nor is it always the case that the magnitude to which optimization principles apply must be defined independently of the principles themselves. Just to take one example, the very notion of 'subjective expected utility', a function mapping a person's consistent ranking of preferences among possible outcomes onto real numbers, was introduced by Von Neumann and Morgenstern precisely in order to derive a maximization principle. The best (most rational) decision is the one that maximizes subjective expected utility. The same maximization principle was then applied to vastly revised and generalized definitions of expected utility.

One more remark, before closing. LLJ, running through open doors, push the case of illustrious physicists who have abandoned well established theories and conceptions, under the impact of new, growing and incompatible data. The lesson they want to derive is a further twist in their already convoluted argument. Generative grammar is nowhere in the same league as physics, or biology, and it is extravagant, nay perverse, to claim otherwise (I am the main culprit). At any rate, even if it were, physicists and biologists are the first not to be afraid of revising their best theories. LLJ's suggestion is that we, the zealots, instead of brandishing physics and biology in the pursuit of linguistic terrorism, ought to take example from the best minds in physics and biology, and be ready to revise our theories. I have specified already that the book and the preface both make it clear that we are, indeed, disposed to do so whenever necessary. But one passage in their paper cries out for special comment. LLJ cite the case of Arthur Compton and the scattering of electrons. "It was the irreducible discrepancy between his classically predicted results and the observed angle-dependent frequency of the output radiation spectrum that led him to adopt a quantum mechanical account, which yielded a precise and accurate prediction of the observed spectrum". The lesson here, according to LLJ, is one of bowing to the best theory, in the teeth of one's previous theoretical inclinations. In fact, they (correctly) specify that Compton "was not a devotee of quantum physics when he begun research on the problem". Well, listen to this. In the early 1950s, a linguist was pursuing a thorough analysis of grammatical structures within a well consolidated tradition, namely that of phrase-structure grammars (PSG). We can use for this case almost exactly the words that LLJ apply to Compton: "He used considerable technical gifts to defend [the] classical [theory]". But to no avail. The classical theory (in our case PSG) could not accommodate the new data, nor many of the older data, appropriately reformulated. "Only this dramatic experimental evidence... caused [him]

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to give up the classical view". The parallel is striking, I think. Compton switched to quantum physics. Chomsky too switched to a different theory, introducing a new and more powerful class of grammars, transformational grammars, which (indeed) "yielded a precise and accurate prediction of" the data, and allowed to predict many more, in a variety of languages. This revolution, not unlike quantum physics, has been the spark from which further revolutions have been ignited, up to and including the present day. Whether Chomsky's case is akin to, or radically different from, the case of Compton, or that of molecular biology around the mid 1950s, is a matter of subjective similarity judgments. Uriagereka and I (and others as well) think there is a lot in common between such cases. LLJ think otherwise. It is their right. What is unsupported, and tendentious, and more than a bit unfair, is their claim that one similarity metric (theirs) is objective, while the other (ours) is extravagant. There is no metric for similarity metrics.

REFERENCES

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