"Because the stakes are so low"[1]

A review article on Randy Allen Harris, *The Linguistics Wars: Chomsky, Lakoff, and the battle over deep structure*, second edition. Oxford University Press, xvi + 547 pp., 2021.

Geoffrey Sampson

www.grsampson.net

Academics commonly offer their theories to the public as if they are the product of disinterested scholarship in which logic and evidence alone are relevant and personalities count for nothing. Some of them believe that that is the only way academic discourse *should* be expressed. Another point of view is that this style amounts to claiming an unfair advantage in the battle of ideas, by concealing the fact that theories have often been shaped in part by non-rational considerations. Newcomers to a field may find it difficult to evaluate the current state of play if they are kept in the dark about the clashes of personality which helped to mould it.

Randy Harris's book, a new edition of one originally published in 1993, takes the second point of view to an extreme. He offers us an unbuttoned history of generative linguistics, focusing particularly on the period between roughly 1965 and 1980 which was marked by particularly savage warfare between two camps, called "interpretative semantics" and "generative semantics" and led respectively by Noam Chomsky and George Lakoff, and he gives us a very full picture of the emotional and ethical differences which existed alongside and partly explained their intellectual disagreements. This is linguistics red in tooth and claw. Harris describes, for instance, a "vitriolic" public debate between Lakoff and Ray Jackendoff in a plenary session of the 1969 Linguistic Society of America conference, which ended in the exchange "Well, fuck you" —"Well, fuck you, George". The temperature fell much closer to normal after the 1970s, but the conflicts of those years continue to have reverberations today, and Harris's new edition brings the story up to date in the early 21st century. (It also profits, naturally, from many comments the author received on the material in his first edition.)

The first thing to say about *The Linguistics Wars* is that the level of detail is remarkable. One would think that the author had himself been involved in the battles he describes. In fact he was too young for that, but apart from reading the publications of the time extensively he has interviewed fifteen of the leading protagonists, and corresponded at length with many more. (Harris avoids expressing a position of his own on any of the issues, which is probably crucial to the success of the book. If the material had been set out in terms of linguists moving towards or away from the truth according to Harris, it would have read as just a belated salvo in a war that is long over.)

1 My title alludes to a well-known saying about academic politics. One version was quoted by Lawrence Hussman in the Summer 1979 *Antioch Review*: "Thus the remark attributed to the California politico Jess Unruh that academic politics are the dirtiest politics because the stakes are so low." (The same saying has also been attributed in various sources to several other individuals.)

The 1950s and 1960s were a time when physics and other natural sciences enjoyed great prestige (much more so than they do today), so when general linguistics began to be a recognized discipline, it branded itself "the scientific study of language". Generative grammar gained traction in the academic world by claiming to provide a scientific theory of syntax – which had wider appeal than phonology, the aspect of language with which earlier linguists had been mainly concerned, because syntax is more closely related than phonology to human thought and reason.

The trouble was, few academics who studied language and languages had much concept of what a "science" is, and they were unqualified to assess the claim of generative grammar to be one.

In reality, from the word go there was nothing scientific about generative grammar. The essence of science is that it makes falsifiable assertions – claims which can be checked against reality and refuted if they are mistaken. Chomsky's *Syntactic Structures* (1957) assumed that any language can be treated as an infinitely-numerous set of grammatical sentences, and discussed algebraic systems – "grammars" – which "generate" sets of sentences, in the same sense as an equation $(x - a)^2 + (y - b)^2 = r^2$ generates the infinitely-numerous set of geometrical points which constitute a circle. Chomsky used these concepts in *Syntactic Structures* to present two falsifiable theories about human language. He defined finite-state and phrase-structure grammar types, each of which is capable of generating some sets of sentences but not others: so that the statements "Every human language is generated by some finite-state grammar", or "Every human language is generated by some phrase-structure grammar", are both falsifiable – they are falsified if it can be shown that some real human language is one of the sets of sentences for which no grammar of the respective type is available. But Chomsky introduced these concepts of formal grammar only in order to argue that the corresponding theories are indeed false – English and other languages are impossible to define using either the finite-state or the phrase-structure formalisms. The nearest *Syntactic Structures* came to offering a falsifiable theory which might be true was to adumbrate a concept of "transformational grammar" which was left undefined, although some examples of transformational rules for English were given, and there were many references to a fuller statement in an unpublished MS, *The Logical Structure of Linguistic Theory*.

This concept of a hierarchy of different grammar-types which define different ranges of formal languages had emerged from collaboration between Chomsky and his mentor the French mathematician Marcel-Paul Schützenberger, who spent some time at the IBM research centre at Yorktown Heights, New York State. (When Chomsky wrote about the material for more technical readerships, he either co-authored with Schützenberger or attributed the relevant theorems to Schützenberger and other mathematicians; see e.g. Chomsky 1963, Schützenberger 1963: 246, Chomsky and Schützenberger 1967.) Formal grammar theory matters in computer science, because where a programming language such as Pascal or Perl fits into the hierarchy of formal languages affects the design of compiler software which translates a computer program out of that language into machine instructions. On the other hand IBM had no reason to concern itself with applying these categories to human languages, which I take to have been Chomsky's side of the collaboration. When the *Logical Structure of Linguistic Theory* MS, which should have turned "transformational grammar" into a specific falsifiable theory and was all Chomsky's own work, was eventually published in 1975, it became hard to escape the conclusion

that the solid maths of the grammar hierarchy must have been very much Schützenberger's responsibility (though linguists took to calling it the "Chomsky hierarchy"), because *The Logical Structure of Linguistic Theory* was a floundering, mathematically semi-literate compilation (Sampson 1979). There was no falsifiable theory of transformational grammar in *The Logical Structure of Linguistic Theory* – but linguists did not notice that, because few of them read the book when it was published. Randy Harris calls it "the fountainhead of Chomskyan linguistics" but does not say explicitly that he has read it. If he had, he would be the only person other than me that I have ever known to do so; many linguists have mentioned it briefly as underpinning Chomsky's ideas, but so far as I remember I have never seen anyone quote a specific passage or page-reference from it.

Harris quotes one leading generative semanticist, "Haj" Ross, as saying, in an interview with Israel Shenker (1972), "There's no question that Chomsky is a genius and has ... created a field of mathematics which didn't exist before". Ross can only have been referring to the grammar hierarchy, so his remark illustrates just how wildly linguists were mis-evaluating Chomsky's achievement.

And insofar as Chomsky did in later writings give informal explanations of what he meant by "transformational rules", these seemed incompatible with the examples he had offered. Randy Harris rightly describes the *Syntactic Structures* transformation Affix Hopping, which accounts for the possible sequences of English auxiliary verbs in a strikingly neat way, as "one of [the] most rhetorically successful rules in Chomsky's system". But transformations were supposed to be rules which modify morpheme sequences exclusively by reference to the grammatical tree structure dominating the morphemes. That is why, for instance, a young child learning how to form questions in English allegedly knows innately that the rule which fronts a verb in the corresponding statement might be a rule which applies to the main verb of the statement (as it in fact is), but could not be a rule which applies, say, to its first verb (Chomsky 1976: 30–3).[2] But Affix Hopping ignores tree-structure, and simply applies to any string of morphemes of the relevant categories. So, Affix Hopping was responsible for winning many readers over to the "theory of transformational grammar", yet insofar as there ever was a theory of transformational grammar, Affix Hopping refuted it.

Furthermore, linguists other than Chomsky (e.g. Stanley Peters and Robert Ritchie 1973) tried to sharpen up the concept of transformational grammar sufficiently to check that there are formal languages which are not definable within the formalism, i.e. that it was in principle falsifiable; and they found that it wasn't: *any* formal language that can be defined at all can be defined by some transformational grammar. So I was wrong to say that Affix Hopping refuted the theory: there was no theory to refute.

Much later, Chomsky claimed that "generative grammar" was never about defining languages as sets of grammatical sentences anyway; "The concepts 'well-formed' and 'grammatical' ... played virtually no role in early work on generative grammar except in informal exposition, or since" (Chomsky 1995: 213 n. 7). Harris sees this remark as worth quoting because it is so plainly untrue. In the opening

2 Chomsky thought that children must know this innately, because for some reason he believed that evidence showing which kind of rule is used in English would be too rare in practice for the average child to encounter. But utterances with the relevant structure are in fact fairly frequent in the kind of speech children hear (Sampson 2005: 43–7, 79–89).

pages of *Syntactic Structures* Chomsky had written "The fundamental aim in the linguistic analysis of a language L is to separate the *grammatical* sequences which are the sentences of L from the *ungrammatical* sequences which are not sentences of L and to study the structure of the grammatical sequences" (Chomsky's italics). Harris quotes Geoffrey Pullum as describing Chomsky's 1995 remark as a "direct falsehood". He points out that this kind of discrepancy between the public record and Chomsky's statements about his own and other linguists' work has been a recurrent problem, and he spends several pages on Chomsky's uneasy relationship with veracity; as Margaret Boden put it, Chomsky "cannot be relied on to tell the truth". Explanations for this differ. According to Lakoff, Chomsky "fights dirty when he argues", while Paul Postal "came to the conclusion that everything he says is false. He will lie just for the fun of it". Harris himself "is more comfortable with the view that arrogance gives [Chomsky] an unreliability with the truth than ... with the view that he lies for sport", and he raises the question whether Chomsky may be "delusional".

But withdrawing grammaticality from the range of empirical facts about which Chomsky's theory claims to make predictions is interesting also because it was one of many ways in which Chomsky's writings after *Syntactic Structures* made his generative theory a clear case of what the philosopher of science Imre Lakatos called "degenerating problemshifts" (Lakatos 1970: 118): theories which defend themselves against adverse data by reducing their range of testable predictions, so that they become increasingly invulnerable to refutation at the cost of becoming empty. Karl Marx's theory of history, for instance, is often seen as a degenerating problemshift (Popper 1969: 37). It began with a respectable falsifiable hypothesis: all societies were bound to evolve through the sequence feudal–capitalist–socialist–communist, with those societies in which capitalism was most fully developed, such as Britain and the USA, reaching the communist stage earlier than societies such as Russia or China which were still feudal or only in the earliest stage of capitalism. The reason why Marxism later became vague was that its originally clear predictions were equally clearly falsified. If generative grammar had been a falsifiable theory, excluding grammaticality from its range of predictions would have made it much less so. (But since in fact generative grammar began empty, it could not become emptier.)

And very few of the many linguists described by Randy Harris as proposing revisions, yielding an alphabet soup of novel grammar formalisms – Lexical-Functional Grammar, Daughter-Dependency Grammar, Arc Pair Grammar, and so forth – had much interest in turning the theory into something that could be taken seriously as a "scientific study of syntax". They were not sophisticated enough about science to appreciate that there was a problem.

Some of the would-be "scientific" productions of generative linguists were reminiscent of the cargo cults found on various Pacific islands in the Second World War (and earlier). The untutored natives saw Japanese or Americans arriving to build airstrips, after which aeroplanes landed bringing quantities of desirable goods; so the natives imitated as best they could, clearing strips and lashing "control towers" together from bamboo, in the hope that they too would receive bounty from the sky. David Johnson and Paul Postal's theory of Arc Pair Grammar (Johnson and Postal 1980), which Randy Harris sees as having been perhaps "the most precisely and rigorously formalized generative model", was developed at a time when one of the hottest elements of theoretical physics was Richard Feynman's "Feynman diagrams", which model the possible interactions of electrons and photons as tracks meeting and

separating, as a step towards calculating the probabilities of overall outcomes. Tracks in Feynman diagrams are typically curvy rather than straight, because particles do not have to travel in straight lines – in Feynman's quantum physics, they go every possible way simultaneously. Some tracks are shown as simple lines, others as wavy lines, to distinguish the paths of electrons, which have direction (first here, then there) from those of photons, which are neutral between coming and going. Linguists have typically diagrammed sentence structures as trees with straight branches, but lines in arc pair grammar diagrams are drawn curvy. Arc Pair Grammar lines come in two varieties: simple lines, versus wavy (or at least sawtoothed). Arc Pair Grammar diagrams must surely have been inspired by the quantum-theory precedent. But the difference is that Feynman's diagrams enable accurate predictions about empirical facts; Feynman (1985: 7) told us that the theory yielded a result for the magnetic moment of the electron which matched observation to ten significant figures – "If you were to measure the distance from Los Angeles to New York to this accuracy, it would be exact to the thickness of a human hair." I am not aware of any facts which Arc Pair Grammar explains and which would not be explained without it. This, and many other works in the generative grammar tradition, were pure cargo-cult linguistics.[3]

Generative grammar cannot be a scientific study, because the syntax of human languages is not a phenomenon to which scientific method applies. Phonology (which is all most linguists before Chomsky were concerned with) deals with behaviours which, although learned, are not under conscious control, and can reasonably be described by falsifiable generalizations. A German who wants to put the verb *geben*, "give", into the imperative does not need to think "when the ending *-en* is knocked off, the /b/ of the root will be word-final so I need to devoice it and say [gip] rather than [gib]" – unless the speaker has studied phonetics he probably will not even be aware that he uses different sounds in the different environments. But in the syntactic domain, speakers can and do think about what they want to say and how they are going to say it. The language offers a variety of well-beaten grammatical tracks, but it is always open to a speaker to choose some novel path – and if he is rhetorically successful, others may follow and the novel way of putting words together may itself become well-trodden. Perhaps some speakers rarely or never exercise this freedom, but plenty of others do use it. To quote John Taylor (2012: 285),

> speakers are by no means restricted by the generalizations that they (may) have made over the data. A robust finding from our investigation is that speakers are happy to go beyond the generalizations and the instances that they sanction. Speakers, in other words, are prone to *innovate* with respect to previous usage, using words in ways not already sanctioned by previous experience, extending the usage range of idioms and constructions ...

Consequently the idea of a falsifiable definition of the range of grammatical sentences of a language does not make sense. Extending that range is a normal part of language behaviour, not something special to be sidelined under the heading "diachronic linguistics".

3 I cannot guarantee that my brief account of Feynman diagrams is completely faithful to the theory – quantum physics is far from my beat. But Johnson and Postal's graphs are not related to the content of physics. The resemblance to Feynman diagrams is purely cosmetic, lending this kind of linguistics a little of the glamour which justifiably belongs to quantum theory.

This was concealed by Chomsky's peculiar use of the term "creative", which linguists accepted without questioning it. Chomsky has frequently called language behaviour "creative" to mean merely that speakers produce new examples from an infinitely large range of possibilities, rather than repeating one or another of a finite set of sentences. But if that makes behaviour "creative", then someone who spends his time multiplying pairs of numbers together would have to be described as acting creatively. There are certainly infinitely many different pairs of integers, and someone who has been taught arithmetic can multiply any of them (subject, the same as in the linguistic case, to constraints on memory and so forth). Normally, that would be seen as uncreative behaviour. In everyday life, outside linguistics, we call an activity "creative" only if new examples typically have an unexpected quality: they somehow fall at least slightly outside the range of past examples (Sampson 2016). In that everyday sense, generative grammar is founded on the assumption that syntactic behaviour is *un*creative. But in reality syntactic behaviour, like other human behaviours under the control of conscious intelligence, is creative in the full, everyday sense. So it is not open to scientific description.

Programming languages are called "languages", and much of Chomsky's early writing was in computer science journals. But, in reality, programming languages make very unsuitable analogues of human languages. Programming languages certainly do have definite grammars – they would not work if they didn't. Human languages do not. Computer scientists realized before the 1960s were out that the hope of relating the Chomsky or Schützenberger hierarchy to human language was pretty much a dead end (Hopcroft and Ullman 1969: 8–9). But linguists never explicitly recognized that.

Harris shows that the conflict between interpretative and generative semantics mutated over time into a very different kind of disagreement from the one which began it. Initially, it was a technical dispute about the directionality of the algebraic rules used to "generate" the sentences of a language. The interpretative position was that the core of a generative grammar was a system which, for each sentence, defined an abstract structure corresponding to its syntax; and two other systems separately converted this structure into, respectively, a representation of its phonetic realization, and into some kind of representation of its meaning (though both camps were very naive about what it would be to "represent meaning"). The generative semanticists wanted to start at the meaning end, and have a single system converting between meaning and pronunciation. However, the generative semanticists began noticing all kinds of properties of real-life usage which, it seemed, could not be handled by a generative grammar whichever way its rules ran – properties like dependence of meaning on non-linguistic context, irony, or the aesthetic value of literary and other language. Chomsky recognized (1976: ch. 4) that describing language and languages involves distinguishing between "problems" which may in principle be solvable, and "mysteries" which it seems hopeless to address in our current state of understanding, and reasonably enough he took the line that he wanted to concentrate on problems and set mysteries aside. But generative semanticists seemed to feel that linguistics in Chomsky's style actually got in the way of accounting for the aspects of language that interested them, and perhaps tacitly implied that those aspects were unreal.

Harris shows that to a large extent the clash was less a clash of ideas than of personalities. Chomsky's written style was strait-laced, but the generative

semanticists went in for juvenile frivolity – their papers were full of gratuitously bawdy or otherwise skittish examples. In one entirely typical case quoted by Harris, Jim McCawley, writing as "Yuck Foo", invited the reader to "Consider the idiomatic sense of *shove X up Y's ass*. As is well known, Y must be coreferential to the indirect object of the next higher clause ...". This was the hippie, flower-power era; like the hippies, who relished mocking and taunting authority figures while spending no time thinking about the actual problems of governing a country or even running a town council, the generative semanticists were for ever coming up with quirky linguistic facts that seemed problematic for their opponents, but did little to make a detailed reality of their own alternative concept of linguistic description. Harris tells us that Lakoff "taught his students to approach linguistics by engaging in anti-establishment thinking and trafficking in counterevidence. Somewhere, however, the imperative to save the phenomena [philosophy-of-science jargon for 'account for the empirical facts'] was misplaced, and much Generative Semantics energy was increasingly devoted simply to celebrating anomalies." It was difficult for onlookers not to be sceptical. Harris quotes Howard Lasnik: "You have to take your field seriously. You can't convey to the world that it's like a standup routine in a nightclub".

From a wider perspective, though, generative versus interpretative semantics was an internal disagreement among academics who were all fundamentally on the same side. The generative semantics people pointed to many linguistic phenomena for which the concept "rule-governed" seemed more obviously inappropriate than it does for syntax, but they did not repudiate the programme of defining languages via the methods of science. Perhaps a human language was not a sharply-bounded set of grammatical sentences – but then we might hope for better language-definitions via, say, "Haj" Ross's concept of gradient category "squishes", or some other way. Some generative semanticists did go further; Harris quotes Jim McCawley as writing in 1973 that "generative semanticists have come to dispute the notion that one can speak coherently of a string of words ... as being grammatical or ungrammatical or having a degree of grammaticality", and in 1982 "I hang my head in shame at seeing how many times I have spoken of sentences as being 'grammatical' or 'ungrammatical' " – that is, for McCawley at that point it was not just that there was more to a language than grammaticality, but that grammaticality was not a meaningful concept. (This is of course a separate issue from Chomsky's absurd claim, quoted earlier, that – irrespective of whether grammaticality is a meaningful concept – his own work did not use it.) But that just raises the question what McCawley did see the aim of linguistics as being. Coming up with numerous examples, often amusing examples, of utterances which were problematic for the generative-grammar framework (a legitimate scientific move as far as it went) did not in itself amount to creating an alternative programme for the discipline of linguistics. And in particular, "generative semanticists" repudiated the label "generative grammar" but not the word "generative": so how did they envisage their alternative kind of linguistics "generating" anything?

Eventually, George Lakoff announced a new school of "cognitive linguistics", opposed not just to generative grammar but to generative linguistics as a whole – a 1991 article was titled "Cognitive versus generative linguistics", and a new journal, *Cognitive Linguistics*, was first issued the previous year. As readers will know, cognitive linguistics succeeded in establishing itself as a school which exists to this day. Cognitive linguistics rejected the "generative" label, but it did not reject the idea of

linguistics as an empirical science; quite the contrary, Lakoff contrasted his new approach with generative linguistics by stressing that his approach was "empirical", a "scientific study of language", as opposed to the "speculative philosophy" of the generative programme (1991: 53, 62).

However, even sympathetic outsiders have in practice found it hard to pin down just what the content of cognitive linguistics is supposed to be, beyond "generative linguistics is wrong". Most expositions of cognitive linguistics begin by talking about the importance of metaphorical usage, but that in itself is hardly a novel contribution. Randy Harris is eloquent about Lakoff's extraordinary overestimate of his own intellectual significance in this area, quoting him as telling a *New York Times* reporter in the mid 2000s that "nobody challenged Aristotle [about metaphors] even though he was wrong." Harris continues:

> Variations of this theme – that the entirety of scholarship has suffered from a fundamental error, but Lakoff is here to set things right – are familiar in his writing and his talks, as are his blithe claims about the empirical underpinnings of his own work ... Lakoff may still not even realize he is *wrong* when he makes such claims about his "discovery" of the role of metaphor and analogic thought in the history of ideas, or realize how outrageous it is to claim he surpasses the entire history of Western thought on metaphor.

I tried to educate myself about cognitive linguistics by reading a 2015 account of it by a man who has held a Chair of Cognitive Linguistics and was co-editor of the journal of the UK Cognitive Linguistics Association. But the book seemed too full of errors of fact, mistaken assumptions that properties of the English language are properties of human language generally, and a share of the "relentless self-promotion" which Harris attributes to Lakoff, to make cognitive linguistics a serious candidate to replace generative linguistics (Sampson 2015). Chomsky's original idea, that human languages might all fall at a particular point in the Schützenberger/Chomsky hierarchy, would have been worth trying, if it had ever been fleshed out into a specific hypothesis (even though it would have turned out to be wrong). I am not aware of any alternative hypothesis offered by cognitive linguistics.

While the linguistics wars raged, there were other teams of linguists quietly putting serious effort into constructing actual generative grammars for real languages. Robert Stockwell, Paul Schachter, and Barbara Partee tried to do this job for English, and Maurice Gross led a group which spent more than ten years trying to do the same thing for French (see Stockwell et al. 1973, Gross 1979). Both teams eventually recognized that they had been attempting the impossible. Gradually it came to be generally appreciated that human languages have not got definable grammars, because real-life language behaviour is "creative in the full sense" (cf. Sampson 2017: ch. 3). And if there is no such thing as a generative grammar of an individual language, then the idea of developing a scientific theory which asserts that all human languages have grammars of a specific type will be as if an engineering department proposed to investigate efficient and economical techniques for constructing perpetual motion machines.

That is probably one reason why the heat went out of the "linguistics wars" in the 1980s. The linguistics warriors, whatever their shortcomings as scholars, did sincerely believe in what they were doing. But once linguists came to know in their

hearts, whether or not they allowed themselves to become consciously aware of it, that the generative linguistics programme amounted to chasing a rainbow, it would have been natural to lose interest in quarreling about where the pot of gold is buried.

That doesn't mean that there was any formal declaration of the death of generative linguistics, or of linguistics as a science. Looking at random at a few recent introductory linguistics textbooks and contributions to Lingbuzz, it appears to me that in practice the discipline continues to regard itself as "the scientific study of language", and still sees the basic concepts of generative grammar as broadly uncontroversial, even if the "wars" that Randy Harris documents are no longer being waged. But then, in the university régimes of the late twentieth and 21st centuries, it would have been very difficult to acknowledge the generative enterprise as a mistake.

At this point I must digress, because generative linguistics was a product of the large régime change in universities which occurred in the late twentieth century, and the subject could not have flourished without that change. For Americans, the change took place too long ago and, I believe, too gradually to be a lively topic of present-day awareness. It happens that in Britain, where most of my career was spent, the university régime change was relatively recent and abrupt, brought about in most respects by government acceptance in 1985 of the recommendations of a committee chaired by a businessman, to the effect that academics should be made to see themselves not as scholars of their subject but as employees of enterprises competing, like other businesses, in terms of their balance sheets.

When I had been a fellow of an Oxford college in the early 1970s, academics researched and wrote, if they chose to do so at all, because they thought it worthwhile rather than because they were told to do so, and outside the natural sciences the doctorate of philosophy was still something of a joke degree (it had been created in 1914 in order to attract a share of American graduate students who had been crossing the Atlantic to study in Germany, where doctorates were available – even today the MA formally outranks it). Nowadays teaching graduate students is another income stream for universities, so they have made the PhD a qualification for entry to the profession; and professional academics are required to compete in terms of their publications. (In my undergraduate days we were amused to hear about the American "publish or perish" syndrome, not imagining that we might live to see it introduced in Britain.) All over the Western world there have come to be armies of professional academics who need to produce steady streams of publications in order to hold down and advance in their jobs, and armies of graduate students who need to find thesis topics – both armies swollen by the large expansion of universities in the 1960s as well as by the régime change I have discussed. If one accepts that there are scientific descriptions of languages waiting to be uncovered, then one can carve out areas within that wide field to write up for a PhD or turn into a journal article. The more elaborate and recondite the scientific truth, the larger the domain from which individuals can hack out special subjects to work on. There are not many doctorates to be won by explaining that syntax cannot be treated scientifically.

To those outside the discipline, the resulting mass of research literature might appear unconvincing or pointless, but in modern academic circumstances that does not matter. To advance in their careers, university dons nowadays must engage in competition, but the judges of the competition are other members of the same discipline – who else could do it? The natural sciences often have objective criteria for the value of theories, but in the humanities and "social sciences" on the whole

what matters is only that there exists a body of others who believe in and practise one's discipline. Once a critical mass of research-active academics has accumulated, they become self-sustaining and can afford to ignore the outside world. One can still make a living teaching or researching generative linguistics.

Randy Harris has done the intellectual world a remarkable service by displaying in detail the battles that defined the field of "theoretical linguistics" for many years, not just in terms of the positions and counter-positions taken in the journals but in terms of the clashes of personality behind the published surface of the debates. I was a linguistics graduate student in the USA myself in the late 1960s, and encountered most of Harris's principal *dramatis personae*; I can confirm that yes, this is the way it was – Harris captures the atmosphere of that vanished time very well.

One might argue that Harris is too kind to his subjects. He does not really spell out how little intellectual substance there ever was to the clashes – reading his book, one might suppose that they revolved round issues that could have been treated and resolved through sober scientific research but instead were turned into something like shouting matches. In reality, shouting matches were about all they could be, and the emotional temperature was high, I suspect, precisely because the alternative of sober scientific advance was not available. It was probably necessary for Harris to be kind; someone who felt no sympathy at all for these contending personalities could hardly have summoned up the patience to record the history of their conflicts in such detail.

I do wonder what lies behind the remark Harris makes at one point that now, as opposed to when the wars were being waged, "Linguistics is in a very good place". (He does not elaborate.) For decades I have been contemplating linguistics from the outside, having shifted out of it in terms of departmental affiliation at the end of the 1980s through disenchantment with what it had turned into, so I am inevitably less closely familiar with what is going on in the discipline than if I had continued to teach it till retirement. But my impression is that these days the subject is a patient on artificial life-support. It took off in the 1960s on a wave of excitement about its promise to use language as a window on the workings of the mind, but few believe in that any more. Linguistics departments contribute financially to their institutions by offering degrees to the army of foreign English-language teachers who want to study where English is spoken, and have to be taught something while they are there. Some interesting work has emerged from this "applied linguistics" industry, but it amounts only to rare nuggets in a sea of dross which would scarcely have been produced if it were not financially profitable. There are certainly a number of worthwhile special subjects being pursued in other areas of linguistics, but probably they could equally well have been pursued under other discipline headings if departments of linguistics had never been created.

One can pick little holes here and there in Randy Harris's exposition. Various cited items are omitted from the bibliography. It was never true (p. 92) that the ability of *Syntactic Structures* transformational rules to move constituents implied that "all languages should have completely unrestrained word order".

But is hard to think that *The Linguistics Wars* is destined to have many rivals as a vivid and thorough case study of how large numbers of intelligent people can be persuaded by one or two monstrous egos to squander their talents and passions on a nonsubject.

References

Chomsky, A.N. 1957. *Syntactic Structures*. Mouton (the Hague).

Chomsky, A.N. 1963. "Formal properties of grammars". In Luce, R.D., et al., *Handbook of Mathematical Psychology*, vol. 2. Wiley (New York).

Chomsky, A.N. 1975. *The Logical Structure of Linguistic Theory*. Plenum Press.

Chomsky, A.N. 1976. *Reflections on Language*. Temple Smith.

Chomsky, A.N. 1995. *The Minimalist Program*. MIT Press.

Chomsky, A.N. and M.-P. Schützenberger. 1967. "The algebraic theory of context-free languages". In P. Braffort and D. Hirschberg, eds, *Computer Programming and Formal Systems*. North-Holland (Amsterdam).

Feynman, R.P. 1985. *QED: the strange theory of light and matter*. Penguin.

Gross, M. 1979. "On the failure of generative grammar". *Language* 55.859–85.

Hopcroft, J.E. and J.D. Ullman. 1969. *Formal Languages and their Relation to Automata*. Addison-Wesley.

Johnson, D.E. and P.M. Postal. 1980. *Arc Pair Grammar*. Princeton University Press.

Lakatos, I. 1970. "Falsification and the methodology of scientific research programmes". In I. Lakatos and A. Musgrave, eds, *Criticism and the Growth of Knowledge*. Cambridge University Press.

Lakoff, G. 1991. "Cognitive versus generative linguistics: how commitments influence results". *Language & Communication* 11.53–62.

Peters, P.S. and R.W. Ritchie. 1973. "On the generative power of transformational grammars". *Information Sciences* 6.49–83.

Popper, K.R. 1969. *Conjectures and Refutations: the growth of scientific knowledge*, 3rd edn. Routledge and Kegan Paul.

Sampson, G.R. 1979. "What was transformational grammar?" (a review article on Chomsky 1975). *Lingua* 48.355–78; a version reprinted as ch. 9 of Sampson, *Empirical Linguistics* (Continuum, 2001).

Sampson, G.R. 2005. *The "Language Instinct" Debate*, revised edn. Continuum.

Sampson, G.R. 2015. Review of Vyvyan Evans, *The Crucible of Language*. Online at <ling.auf.net/lingbuzz/002762>; a version printed in Sampson (2017: ch. 6).

Sampson, G.R. 2016. "Two ideas of creativity". In M. Hinton, ed., *Evidence, Experiment and Argument in Linguistics and Philosophy of Language*. Peter Lang (Bern). A version reprinted in Sampson (2017: ch. 2), and online at <ling.auf.net/lingbuzz/002619>.

Sampson, G.R. 2017. *The Linguistics Delusion*. Equinox (Sheffield, Yorks. and Bristol, Conn.).

Schützenberger, M.-P. 1963. "On context-free languages and push-down automata". *Information and Control* 6.246–64. Reprinted in R.D. Luce et al., eds, *Readings in Mathematical Psychology*, vol. 2. Wiley (New York).

Shenker, I. 1972. "Former Chomsky disciples hurl harsh words at the master". *New York Times* 10 Sep 1972, p. 70.

Stockwell, R.P., et al. 1973. *The Major Syntactic Structures of English*. Holt, Rinehart & Winston (New York).

Taylor, J.R. 2012. *The Mental Corpus: how language is represented in the mind*. Oxford University Press.