

Interview with Geoffrey Sampson

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Geoffrey Sampson, Professor Emeritus at the University of Sussex, is a gifted personality with academic interests in linguistics, computing, philosophy, and economics. Fortunately for our readers, Sampson describes himself as a person “whose central intellectual interest is language” (www.grsampson.net), and he agreed to talk to us about various aspects of language and linguistics.

RG

*Prof. Sampson, in 1980 you published the book *Schools of Linguistics*, which had and still has a positive response in the linguistic world. What was the motivation for writing this critical survey of the main linguistic theories of the twentieth century?*

GS

Oh, the main reason why I wrote *Schools* was simply that, not long after I left student days behind and embarked on a career as a professional academic, I looked around at the subject and realized that, by luck rather than good judgement, I happened to have been exposed in some depth to quite a number of alternative approaches – perhaps more than the average linguist. In other words, by chance I was well placed to put a book like that together, and I thought it might fill a “gap in the market”. (And of course it gave me a good excuse to study some other interesting styles of linguistics of which, until then, I had had less experience.)

It was much the same story when I produced the first edition of *Writing Systems*, which is the other one of my books to have attracted sufficient attention to be translated into foreign languages. It always seemed odd to me that (following the lead of Ferdinand de Saussure) linguistics had almost completely ignored the written mode of language, and I realized that the languages I had studied happened to be ones which gave me a wider-than-average exposure to different types of script. The book which perhaps first showed me as a teenager how thrilling linguistic studies can be was John Chadwick’s account of the decipherment of the syllabic Linear B script of pre-classical Greek. For my first degree at Cambridge University I studied Chinese, with special focus on the history of the language (and hence on the evolution of its script). I had put some effort into learning Biblical Hebrew, to get closer to the Old Testament roots of the Christian faith. I had dabbled in Japanese (Japanese script looks to Westerners similar to Chinese, but typologically they are extremely different). So I was well placed to write about this topic, and it needed to be written about. Many Western scholars were so ignorant in those days about the nature of Chinese writing, for instance, that they took quite seriously Sir Jack Goody’s laughable idea that the script is incapable of expressing unorthodox ideas or logical argument. Somebody had to tell the true story, so I thought I would give it a try.

(Actually that original edition of *Writing Systems* did not even fully exploit the range of script types I happened to have encountered. As a first-year graduate student at Yale I had been required, rather reluctantly, to grapple with the Devanagari script of Sanskrit, and later I had

taken Floyd Lounsbury's fascinating course on the decipherment of Maya script, which at that time was only just beginning to be widely recognized as full-scale writing. In my 2015 edition these two cases get due coverage.)

Reverting to *Schools*, the book you asked me about, I suppose there was also a deeper reason why the topic appealed. When I came back to Britain after my time as a graduate student in the USA, my first years were spent as a researcher at Oxford, where at the time there was no linguistics – so that pushed me into studying philosophy, in which Oxford was supremely strong. And I moved from there to the LSE, where the philosophers of science (in what had been Sir Karl Popper's department, though he had retired by then) were the most interesting group I found to interact with. All that got me thinking about what makes a given approach to a discipline worthwhile or unrewarding, so it was a natural step to explore and compare the different approaches that had been tried in linguistics.

RG

Your book Grammar Without Grammaticality (2013, co-authored with Anna Babarczy) has an oxymoron-like title, and the name of your new book The Linguistics Delusion (2017) carries a negative connotation. Is this done on purpose so as to attract the reader, or is the situation in linguistics really full of fallacy and delusion?

GS

It is interesting that *Grammar without Grammaticality* sounds to you like an oxymoron. It confirms to me that our book needed to be written!

The fact is that the idea of languages having grammars has been common currency for many centuries, in fact European education for much of the period from the Middle Ages onward was centred on grammar (particularly Latin grammar) – hence the fact that old-established British secondary schools were often called “grammar schools”. But on the other hand the idea of “grammaticality”, in academic linguists' sense, is very new. It did not exist before the middle of the twentieth century. Before then, people who cared about purity of language usage did sometimes talk about particular usages being “ungrammatical”, meaning that they were disapproved of: in Britain, for instance, split infinitives (*to boldly go ...*) might be called “ungrammatical”. But the rather commonplace idea that certain particular turns of phrase (which in practice are indeed used, otherwise the purists would not identify them as something to be avoided) are “wrong” or undesirable is quite different from the much deeper idea that there exists some definable class (perhaps an infinitely large class) of sentence structures which are “all and only” the sentences that are valid examples of the language. When theoretical linguists talk about “grammaticality”, it is this second idea they have in mind.

The second idea seems to me baseless, and it has never been seriously argued for. Generative linguists in the mid-twentieth century simply asserted that human languages can be defined that way, and that a suitable goal for grammatical description is to compile a system of formal rules which specify “all and only” the word-sequences possible in the language, and the discipline went along with this programme rather unquestioningly. I went along with it myself for a while: at that period I was very young, and young people often do accept that what their elders tell them must be right. What is more, if one accepts the grammaticality idea, it creates a

fertile, interesting field of research, with plenty of scope for developing research projects to “sell” to funding agencies, about constructing generative grammars, or partial grammars, for various languages, and about identifying universal features common to the generative grammars of all languages.

Realistically, though, human language is not like that. We have some grammatical patterns which are extremely well-established and frequent, others rather less so, and others again are quite unusual and novel when they occur. As John Taylor has put it, “Speakers ... are prone to *innovate* with respect to previous usage, using words in ways not already sanctioned by previous experience”. Putting words together in novel ways is a normal part of being a competent language user. We just don’t always stick rigidly to any fixed set of grammatical rules, though often we do use the best-established grammatical patterns – perhaps more often in writing than in speech. Grammatical innovation only works if hearers (or readers) grasp what speakers/writers intend by their wording, but often they do.

In this situation, the task of describing the grammar of a language can only be interpreted as specifying the best-established patterns, down to some threshold of frequency or familiarity which has no theoretical significance but will depend essentially on the level of resources available to the grammarian, or on the purpose for which the description will be used. (If it is to be published as an aid for L₂ learners, for instance, how large a book will the students be willing to buy or use, or how long a book will a publisher agree to publish?) There is no natural stopping-place where one could say “That is job done, you now have in your hand the full grammar of language X.” And hence one can never say “Any sentence pattern not included in this description is ‘ungrammatical’ and will not be encountered in practice.”

The kind of grammatical description just outlined corresponds to what was done in practice by those down the centuries who compiled what linguists nowadays call “pedagogical grammars”, culminating for English in the magisterial *Comprehensive Grammar of the English Language* by Randolph Quirk, Sidney Greenbaum, Geoffrey Leech, and Jan Svartvik. And it is the only approach to grammatical description which makes sense.

If someone doubts this characterization of the “pedagogical grammar” tradition, let him take a few pre-1950s grammatical descriptions of English or other languages, and search in them for examples of structures classified as ungrammatical – what today are called starred sentences. You will find cases such as split infinitives: “some people use structures like *to boldly go*, but the reader is advised to avoid them”. That kind of example, as we have seen, is a special case. If the concept of “grammaticality” were to make sense, one would expect to find grammarians making broader contrasts between “good” examples and “ungrammatical” examples, where the latter would not be things that people do say or write and (arguably) shouldn’t, but rather would be things that in practice do not get said or written. I predict that someone carrying out this exercise will find very few cases like that. “Starred sentences” are a recent linguistic myth.

I said that the “pedagogical grammar” approach to description is the only style which makes sense for a human language. There is another kind of “language” – computer programming languages – which really can be, and are, defined by finite sets of formal rules which specify “all and only” their well-formed “sentences”. The early years of generative linguistics were very heavily influenced by an analogy between human and programming languages. Many of Noam Chomsky’s early publications were in computer science journals.

But this analogy is spurious. Just because systems like Pascal or Java are conventionally called “languages”, that does not make them similar kinds of system to English or Slovak, and they are not.

In my experience, the point of view I am propounding here is accepted as natural and right by educated outsiders to academic linguistics, but it is commonly resisted by professional linguists. That could be for no better reason than protectiveness about their careers – if your professional work relates heavily to research on generative grammars, you will not be happy to hear that no such things exist or ever could exist. If someone hopes to rebut my point of view with respectable intellectual argument, though (rather than mere professional defensiveness), they would need to offer some kind of evidence tending to show that a human language does have fixed grammaticality bounds. What could that evidence be?

If anyone managed to construct an adequate generative grammar for some human language, that would certainly be good evidence. In the 1960s and 1970s, there were serious, large-scale attempts to achieve this for certain major European languages. But they never worked out. (See an article by Maurice Gross in the 1979 volume of *Language* on the fate of a project which aimed to compile a generative grammar for French, for instance.) Work of that kind for English was still continuing at Cambridge University into the early 1990s, but most linguists had recognized quite a bit earlier that compiling an adequate generative grammar is a hopeless quest. (In consequence, some theoretical linguists, e.g. Cedric Boeckx, have suggested that the quest was perhaps hardly ever taken seriously – but that is scarcely fair to the numerous distinguished academics who put intensive effort into the task over a considerable period.)

What other kind of evidence might justify the “grammaticality” concept? The only kind that occurs to me is potential evidence that some limited set of grammatical patterns are used heavily in a language, while other patterns, even if they do occur now and then, occur as one-offs or at sharply lower frequencies than the well-established patterns – so that they might perhaps be set aside as oddities of “performance” or the like: speaker errors, utterances broken off because the speaker is interrupted, and so forth. If we found that situation, it might well be reasonable to say that what is special about the frequent patterns is that they constitute all and only the “grammatical” patterns.

But, as I have been arguing for decades, if one looks at the evidence this is not what we find. Instead we find a continuous spectrum of pattern frequencies, from a few very frequent patterns to a huge range of one-offs *and everything in between*. There is no point where it is plausible to divide the spectrum and say “on one side we have well-formed sentences, on the other oddities of performance”.

For most of my career I argued this by reference to data drawn from English. It might be said that English does not make the ideal test of the “grammaticality” concept, because it is a rather messy and anarchic language. We have little inflexional morphology, and English-speaking nations do not have “National Academies” empowered to regulate English usage. So in one chapter of my new book, *The Linguistics Delusion*, I use a data-set from German, which is a much more “officially regulated” language than English. If grammaticality were a reality for any language it would surely be a reality for German. (And this data-set had other advantages: it is several times the size of the English data-set I used earlier, and it was compiled by researchers unconnected with me, so there can be no suspicion of manipulating the data to achieve the

desired result.) But the outcome of my analysis was the same: a smooth spectrum of constructions at different frequencies, down to numerous constructions that occurred once only in this relatively large data-set.

I wonder how many linguists, even if they are attached to the concept of grammaticality, would believe in their hearts that expanding the data-set further, or choosing some other language, would give radically different results? I feel sure the results would be essentially the same. And if that is right, what other kind of evidence could believers in grammaticality point to? Perhaps there might be such evidence, but the ball is in their court: if the true believers want the rest of us to take seriously this only-sixty-years-old concept, they would need to start explaining why we should take it seriously. They never do. Grammar is a real thing, but grammaticality is a delusion.

RG

In your chapter on Noam Chomsky and generative grammar you characterize the understanding of linguistic universals by Chomsky and his followers as a “rush to universals” (p. 149). You have done a lot for and in corpus linguistics. Looking at the state of the art in linguistics, one may come to the conclusion that everything is either corpus-based or corpus-driven. Isn’t it something like a “rush to corpus”?

GS

There is something suspect, I find, about the phrase “corpus linguistics” (though I am sure I have used it in some of my own writings).

Consider an analogy. Before Hans Lippershey invented the telescope at the beginning of the seventeenth century, astronomers tried to theorize about the nature of heavenly bodies and their motions on the basis of unaided eyesight, supplemented by various principles that they saw as intuitively obvious, though in due course these often turned out to be wrong. (For instance, people believed that planetary orbits must be circles, because that is the only shape perfect enough for the heavenly realm – in reality, planetary orbits are elliptical.)

When the first telescopes were constructed, they enabled astronomers to see significant things that could not be seen without them. But telescopes were expensive instruments and hard to get hold of, and only a few astronomers had them. For a while it might have been reasonable to describe those few as practising “telescope astronomy”, while the others continued as best they could with traditional methods. But if telescopes revealed data that were relevant to some theory, there ceased to be any value in developing that theory without taking the telescope data into account. Science seeks to build theories which are answerable to *all* available data. If someone without a telescope developed an astronomical theory which worked fine for naked-eye data but was incompatible with observations that others had used telescopes to make, and if he tried to defend his position by saying “I’m not a telescope astronomer”, his theory could not be taken seriously. The phrase “telescope astronomy”, in the sense I gave it above, would have ceased to be meaningful, because everyone could see that when telescope data were potentially relevant to a given area of theory, people hoping to contribute to that area must either use it, or find something different to do with their lives.

There are certainly areas of linguistics to which corpora have no relevance. Much of

phonology would have little use even for speech corpora, I imagine. But there are other areas to which corpus data are highly relevant. Grammatical description involves looking at the incidence of structures which are so multifarious that just waiting until you happen to hear or read an example does not get you all that far – if you encounter no cases, that might mean that people do not use that structure, or it might easily mean that they do, but you have just been unlucky. The ability to search rapidly a large machine-readable sample of the language under study allows one to form a considerably clearer and more accurate picture of usage than is possible otherwise.

The first computerized corpora were developed in the 1960s, and from then to perhaps some time in the 1980s there was a period comparable to the time immediately after the invention of the telescope. It was reasonable to think of “corpus linguistics” as one approach to linguistics, alongside more traditional approaches. Not all academic researchers yet had easy access to computers, and even if in principle they had such access, they would have needed to find out about corpora that might be useful in their research, get hold of copies and study their structure, and learn how to extract information from them using some programming language that would certainly have been more cumbersome to use than Perl, the standard corpus-research language nowadays, which became available only in 1987. All these things took time and effort; before e-mail and the internet they were difficult unless one was working alongside a few like-minded colleagues who could help with advice; and some of them were expensive.

I was very lucky. I began to learn computer programming at Yale in the 1960s (at one point in those days I was a house-guest of Nelson Francis at Brown University, the creator together with Henry Kučera of the first computerized corpus of English); and from 1974 on I worked for many years at Lancaster alongside Geoffrey Leech, the prime mover behind the British counterpart to Francis and Kučera’s Brown Corpus of American English. So you could say I had a head start.

But in the 1980s the equation began to change. Anyone in a university who wanted to work with a computer could easily do so – a few years later, most academics had computers quite adequate for corpus research in their own homes. Many corpora became freely available over the internet. Perl is universally available, and it makes the relevant kind of data-manipulation so easy that an academic who could not use it after a few hours’ study would surely be in the wrong job.

Furthermore, it became obvious that corpus data are indeed highly relevant for linguistic theorizing. Many generative linguists had supposed that it was not necessary to examine real-life usage in that way, because a native speaker was alleged to be able to look into his mind and know by introspection whether some hypothetical usage is or is not good in his language. But in case after case it turned out that this sort of grammatical intuition is fully as unreliable as the assumption of circular planetary orbits. In *Grammar Without Grammaticality* Anna Babarczy and I discuss a case about what kinds of verb take which kinds of complement in English. In 1974, “Haj” Ross stated as a fact that if a verb incorporates a prefix it cannot take a non-nominal complement. I do not know why Haj believed that; perhaps some particular hypothetical examples sounded unnatural to him, and he inferred that the language has a general rule about it. After 1974, a whole series of linguists over many years developed theories to explain this datum. The trouble is, as Laurie Bauer pointed out in 1990, Haj’s intuition was plain wrong. These days

you do not even need a formal linguistic corpus to refute it. A few seconds with Google will give you as many examples as you like of the allegedly “ungrammatical” structure – here is just one:

You will want to overindulge on this one, but ...

The verb *overindulge* contains the prefix *over-*, and the complement *on this one* is a prepositional phrase. Is anyone going to argue that this (and the thousands of further examples that Google will show you) are “performance errors” or otherwise non-English?

So by the turn of the century one might have expected that the phrase “corpus linguistics” would have fallen out of use. Everyone would have come to expect that if a linguist propounds a theory which has observable implications for the facts of usage, then he and others will treat the available sources of usage data as crucial for acceptance or rejection of the theory. Depending on the specifics of the case, individual experience might be adequate for that, or a Google search might be adequate. But if the theoretical implication is subtler, say “young adults use fewer embedded clauses in speech than older people”, then you are probably not going to be able to devise a way to test that via Google. You need to write a Perl program to extract relevant data from a linguist’s corpus containing structure and annotations permitting the assertion to be tested. There would not be “corpus linguists” any longer; there would just be linguists, who naturally use corpora when these happen to be the tools relevant for testing their hypotheses.

Regrettably, this is not exactly how things have worked out. More than once I have had the surprising experience, in correspondence with other linguists whom I respect as colleagues, of suggesting to them that some particular idea of theirs would not survive testing against corpus data, and receiving the dismissive reply “Oh, we don’t use corpora here, we aren’t equipped for that type of research”, as if it really did not matter whether or not the idea in question was consistent with facts that are available to those who care to examine them.

This gives me a bad feeling. True, the two individuals I am thinking of are both, I believe, older than me, so perhaps one can excuse them as academics who were equal to the research techniques of their own day but have not caught up with newer possibilities. However, I have the impression that university linguistics departments nowadays contain younger people who are developing ideas that ought to be tested against empirical data, often the kind of data available in corpora, but who have no intention of doing so. You say there has been a rush to corpora, but I am not sure that everyone who ought to has joined the rush. If I am correct about that, I see no explanation for it other than laziness. Such people presumably do not want to take the trouble to master the technicalities involved, simple though these are nowadays. But that attitude would just mean opting out of any serious attempt to advance the total of human knowledge. It would reduce linguistics to a pretty pastime that might yield publications looking impressive on someone’s CV, but it would really be a waste of public money.

RG

When talking to linguists from various fields of linguistics, I sometimes have an impression that linguistic universals and typology are more attractive than a focus on differences among languages, as if to accept homogeneity were easier than to accept diversity. What do you

think? Why is it so?

GS

It has not always been that way, you realize. When I first studied linguistics, in the early 1960s, the subject was essentially a set of techniques for describing different languages in a clear, objective way, independent of the particular philological traditions that have grown up round the individual languages. Not only did the discipline not focus on universal properties common to all languages, it explicitly denied that there are any. Martin Joos wrote in 1957 that “languages can differ from each other without limit and in unpredictable ways”, and for the linguists of that period this was an important principle. The subject had developed chiefly in the USA, largely in connexion with anthropological studies of American Indian cultures; I am not familiar with any of their languages but they are evidently very unlike the ones I do know. If you asked a linguist of that time what message of value the subject might have for society generally, a likely answer was that it showed how alien cultures have their own intellectual worlds which are no less subtle and internally consistent than the one we Europeans inhabit, though in many respects they are quite different and scarcely or not at all translatable into our terms. The languages of the world do not divide into European languages and “primitive languages”: languages are just different. These ideas are central to the writings of Franz Boas and Edward Sapir.

The trouble with them was, from the point of view of people who wanted to elevate linguistics into a “science”, that any science is essentially about laws which apply across the board to every case of the phenomenon in question. If linguistics is to be a genuine science, there *have* to be language universals, and identifying them becomes its central goal.

This reformulation of the nature of the subject was aided by a change in the kinds of people who studied it. As I said, in the first half of the twentieth century a high proportion of the few people who studied linguistics did so in the context of anthropological study of cultures which were very exotic by European standards. As a student of (mainly Classical) Chinese I found linguistics gave me a useful handle on alien properties of that language. However, the time when I encountered linguistics was a turning-point, when it was becoming usual for the subject to be taken by large numbers of students of modern Western European languages, or even of their own language – in Britain, people taking BA degrees in English. Academics need student numbers to justify their salaries, and that is where the numbers were to be found.

But this inevitably made the message that languages are all fundamentally similar seem much more plausible. Modern Western European languages really are quite similar. They almost all belong to a single family, and because of the immense intellectual prestige of Latin during most of the Christian Era the modern languages have to some extent been remoulded to make them intertranslatable with Latin and hence, as people saw it, fit vehicles for serious thought.

Thus when these undergraduates were told that all languages are similar, they found that easy to believe. And if they went on to take academic posts they realized that furthering the welfare of their discipline as a science required them to join the hunt for universals; so they did.

To me this move to make linguistics a standard part of European-language degrees was something of a disaster. It has not had much general educational value: these students are scarcely in a position to grapple with the interesting intellectual issues that arise in the discipline,

they mainly just learn to parrot “what it says in our textbook”. If a British student of French happens to encounter a discussion of some remarkable semantic structure in, say, Mohawk, it seems irrelevant to the bulk of his studies and leaves little impression. And now that a high proportion of linguistics teachers have emerged from this kind of background, it shows in the books they write. Jerry Fodor in *The Language of Thought* spent hundreds of pages explaining how all human languages are fundamentally equivalent in terms of word semantics, but he only ever once referred to one tiny example of any language other than English: he tried to quote the French for “dog”, though he even got that wrong.

If there really were an interesting range of language universals to be discovered, it would just be a question of persuading members of the discipline to search for them in a more responsible manner. But Joos was right: languages can differ in almost any respect. Generative linguists have often made large claims about the numbers of universal features which have been identified in recent decades, but if one tries to cut through the rhetoric and pin them down to specific examples, it is amazingly difficult to locate them. Several people, myself included, have attempted this exercise, and found only a few cases, all of which dissolve on close inspection. The idea of linguistics as a “science” alongside subjects like physics is an error. The study of human languages can never be that — because languages are moulded by thought, and our thought is a creative activity which tends to escape any bounds drawn round it.

The years when I was a researcher at Oxford was a time when the language teachers there were thinking of introducing linguistics to the syllabus. My role gave me no say in that, but I remember commenting to a senior colleague that I thought it would be a mistake: linguistics is not really a subject for undergraduates, or at least not for students of familiar European languages. (The colleague I was speaking to agreed with me, but Oxford went ahead.) Later I myself spent years teaching the subject to undergraduates. I needed a job, I had not yet seen through the pretensions of generative theory (and in those days linguistics had not yet surrendered as completely to the scientific delusion as it came to do). I hope my students got something positive from my teaching, but I came to have an uneasy conscience about it, and halfway through my career I was glad to be offered the opportunity to switch into computer science, where there is no doubt that the material taught will be of benefit to the students. I never taught anyone linguistics after the 1980s. If I had a magic wand, I would change the subject back to what it was in the 1950s: a minority pursuit practised mainly by people who need it to help make sense of the mysterious structures found in exotic languages, and researched in a responsible style which emphasizes exact empirical scholarship rather than winning publicity through grandiose claims.

RG

In one interview on corpus linguistics you mention a correlation between the complexity of an individual's speech patterns and the speaker's age. You indicate that this is probably due to changes in British society (the spread of television and internet use). Can the issue be also interpreted as the correlation between language and society? You know, new technologies, new media of communication influence the way we communicate: simple language of internet communication full of abbreviations and acronyms, text messages by mobile phones, etc.

GS

I am not sure what interview you are referring to here, but of course I recognize the finding you mention. Only, it seems to me that you misrepresent my explanation for it. (Or at least, if this is what I said in the interview in question, I evidently expressed myself very badly!)

I had looked at grammatical structures in a subset of the section of the British National Corpus which represents the everyday, casual speech of a cross-section of the British population at a specific point in time, the early 1990s. (The BNC term for this section is “demographically sampled speech”.) For each individual speaker in my subset I computed the average complexity of his or her wording – complexity in the standard schoolroom sense, i.e. how far main clauses have subordinate clauses embedded within them, subordinate clauses themselves have lower-level subordinate clauses, and so forth. Some individuals used much more clause subordination than others, which will surprise no-one.

Then I tried correlating average complexity levels with speakers’ ages. (I looked at other demographic variables too, but it was age that yielded results that, to me, were both surprising and socially-significant.)

Anyone would predict that small children tend to use simpler grammar than older people – and in my data they did. What I had not expected was that average complexity kept on growing with speaker age, not just between childhood and adulthood but on throughout adult life. For instance, people over sixty used more complex structures (on average) than people in their forties and fifties.

Many linguists have been influenced by Eric Lenneberg’s idea that there is a “critical period” in language acquisition, a point perhaps about the age of puberty when our childhood capacity to learn language “switches off”, leaving us in something like a linguistic steady state throughout adulthood. So I looked at the subset of my speakers who in terms of age could be presumed to have reached the steady state, and I checked whether the age/grammatical complexity correlation among just those speakers was statistically significant. It was. The correlation seems to be a real phenomenon, not just a chance glitch.

(I should say, though, that the significance level achieved was not high: $p < 0.05$. I would very much like to see the same analytic technique applied to a much larger data-set, such as the complete BNC demographically-sampled speech section, to check whether the effect occurs at a higher level of significance.)

For the moment, suppose that the correlation I found is a reality. Because the BNC data all represent a single point in time, there are logically two alternative interpretations. My own assumption was not what you say it was. I assumed that the finding reveals a process that we all go through as we age: with extra years of life our speech tends to grow more complex, irrespective of whether we were born in 1980, 1950, or 1920. The alternative is that different historical periods induce different styles of speech in those who acquire their mother tongue in the respective periods: people born in 1980 grow up speaking more simply than those born in 1920, and the speech of the 1980-born will remain simpler however long those individuals live. Call these the maturational and the generational interpretations, respectively.

When I discovered the correlation, I instinctively supposed that it must be explainable in maturational terms; the generational interpretation did not cross my mind. But, to my surprise,

when I began discussing the data with other linguists, many or most of them suggested that the generational interpretation was more plausible; and I certainly agreed that both are equally consistent with the data. (The alternatives are not necessarily exclusive, of course.)

Your question does not refer to this contrast between alternative interpretations: you are contrasting two varieties of the generational interpretation, depending on which particular historical changes might be responsible for changing speech patterns – the spread of television and the internet, or the abbreviated language of e-mails and text messages. If the generational interpretation is the correct one, perhaps you are right to say that the second set of factors are more likely to be responsible. (Though how far were people using e-mail and text messaging in the early 1990s? My memory is that texting only came in some years later – I could be wrong.) But I am not sure why you exclude the maturational explanation from consideration.

The only way to choose between the alternatives would be to compare the BNC speech data with similar data collected at a different period. It is probably too soon to do that yet, and there might not be much point in doing it at all unless more 1990s data can be used to yield a higher significance level for the correlation.

Both these lines of research, though, seem to me potentially much more significant, for society at large, than most linguistic research. Speech structure is an index of thought structure: if people's systems of thought grow more intricate as they move through middle towards old age, what does that imply for the roles society should be allotting to them, or the ways in which society should be supporting them, at different life-stages? We ought to want to know what the facts are here. Conversely, if changes in society are causing younger generations to think in permanently simpler ways, does that matter? Will it mean, for instance, that in future it will be difficult to find individuals suitable for appointment as judges? – that being one profession which regularly requires its members to grapple with unusually tangled systems of ideas. And are there things we ought to be doing to rectify the situation? These are issues which really matter to society. Some of them relate to topics which are already part of public discussion, but by turning the concept of speech complexity into a precisely quantifiable measure linguistics has unique contributions to offer. (By contrast, many issues which linguistic researchers spend their time on have little interest to anyone outside the discipline.)

I have now retired, but I would very much hope that some group or groups of linguists currently in harness might pick up these issues and take them further – it is really a task for teamwork rather than for a lone individual.

RG

Can you explain to our readers the idea that linguists should learn from economic thought?

GS

One general way in which linguistics resembles economics, and hence might be able to learn from the older-established discipline, has been pointed out by the social theorist Friedrich Hayek (joint winner of the 1974 economics Nobel): both subjects, unlike any other area of social studies, have been formalized to the point where they might be called mathematical. (In both cases one can be sceptical about the value of this mathematization – but it is a correct description of the respective disciplines as they currently are.)

But there is a much more specific point, which I discuss in one chapter of *The Linguistics Delusion*. The idea behind “linguistics as a science” and the search for language universals is that human cognitive life is much more rigidly constrained than people have traditionally supposed. A speaker of a given language produces one or other of a fixed, infinitely numerous but definable, range of sentences: generative linguists do not see grammatical innovation as a routine part of language behaviour. Language universals define a fixed range of “possible human languages”, which are the only languages that humans are genetically capable of acquiring – one can easily describe hypothetical languages of other types, but if they existed no-one could learn to speak them. (In some of his writings, Noam Chomsky has suggested that the genetic constraints might be so narrow that there are only finitely many different possible human languages.)

And for Chomsky and other philosophically-minded generative linguists, language offers an unusually clear (because formalized) demonstration of something which they believe is true of cognition more generally. In his book *Reflections on Language* Chomsky argued that the genetic constraints on the human mind must mean that only a fixed range of artistic techniques is available to us. Many people find some modern art chaotic and valueless, and for Chomsky that shows that mankind has now exhausted the valid artistic possibilities and is reduced to producing rubbish. Likewise he says that the range of possible hypotheses available to human scientists must be limited by genetics. There may be subjects for which accurate theories exist in the mind of God, as it were, but those theories fall outside the range genetically available to mankind. If so, then we can go on trying and failing to understand these topics as long as may be, but we shall never succeed.

This was a novel idea when first put forward. Commonly, people have seen our mental activities as creative and free of constraints comparable to those which limit our physical growth and activities. But if we accept that our physical anatomy is controlled by our genes, then, Chomsky asks, is it not irrational to believe that this other crucial aspect of human life is not so controlled?

Whether or not that is a good argument is a question I shall not go into here. It has at least been influential enough that much of current theoretical linguistics assumes (often without spelling it out explicitly) that the argument is right. The concept of linguistics as a science more or less implies that our language behaviour must be less freely creative than people in the past have taken it to be. My reason for introducing economics into the discussion is in order to draw attention to the fact that, while current theories in linguistics assume what we might call a “bounded mind”, one of the most influential currents in present-day economics on the other hand assumes an “unbounded mind”.

A long-standing puzzle for economists has been the phenomenon of economic growth. The classical economic ideas of the nineteenth and early twentieth centuries implied that societies ought not to become wealthier over time. Either they should remain in a steady economic state, or perhaps even sink into increasing misery. Yet the overall experience of the Western world in the modern period has been massive economic growth – of course with hiccoughs now and again, but those have been only brief minor interruptions to an underlying upward trend.

The solution to this puzzle which is generally accepted nowadays by economists is due to

Paul Romer, and it is a theory which incorporates idea-generation into the economic equations. Your readers will not want to go into the mathematical details, but in brief the theory implies that the historical fact of growth can be explained provided (and only provided) we accept that human beings have a capacity to generate novel ideas in an unbounded fashion: not just new examples drawn from a predefined class, but endlessly new *kinds* of idea. Romer is quite explicit about the fact that this is a postulate, not a logical truism, but without that postulate the facts of economic growth are inexplicable.

So on the one hand we have linguists saying “We assume that human cognition is genetically constrained, and we are basing our linguistic theories on that assumption.” Many have felt that it is at least a reasonable assumption, and that they have no solid evidence against it; it may be a novel (and not particularly congenial) assumption, but perhaps we must go along with it. But it turns out that on the other hand we have economists saying “We assume that human cognition is not genetically constrained, and we are basing economic theories on that assumption – we wouldn’t know how to do without it.” Thus it seems that we do not have to go along with the linguists’ assumption merely because it is the only one in the field, or because it might be some kind of logical truism – it is not. Linguistics and economics are contradicting one another, so they cannot both be right.

My own conviction is that it is the economists who are right on this: human intellectual creativity really does allow us to escape from any bounds derived from past experience or from genetics. I could develop that argument at length, but this is not the place to do so. The important point here is that current thought in economics gives us a good reason to keep an open mind about a fundamental issue which linguistic theorists are inviting us to see as a settled matter where only one view is reasonable.

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Does linguistics need something like “popularization”?

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No, popularization should not be a priority for linguistics at present. We live these days in a celebrity culture, and recent popular books about linguistics have too often consisted of attempts by their authors to make a splash with grandiose, surprising statements about language which are asserted recklessly, with little concern for how strong the evidence is (sometimes, for whether there is any evidence at all).

What linguistics needs is to draw in its horns, and adopt an ethos in which it becomes unacceptable for a linguist to make general statements unless he treats them as answerable to hard empirical data, and unless he accepts a responsibility to search those data for possible counter-evidence to his ideas. Of course, many individual linguists are functioning that way today. But too many do not – and popular treatments pay more attention to the grandstanders than to the humble scholars working in a responsible manner. A reformed linguistics will perhaps be a less exciting discipline, and it will certainly be a smaller one, but it will be a discipline with a good chance of leading to greater understanding of this crucial aspect of human life.

It will be soon enough to think about popularizing linguistics when it has managed to

revert to being a serious, empirical discipline.

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Thank you very much for your time.

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