

Interview with Richard Hudson

by Joseph Hilferty

Richard Hudson is Professor of Linguistics at University College London. He did his doctoral thesis on the grammar of Beja, a Semitic language spoken in north-eastern Africa. He has done extensive work in the area of syntax and is the author of the classic textbook, *Sociolinguistics* (Cambridge University Press). He is currently hard at work on his own theory of language structure, word grammar.

On Linguistics

Q: Ever since the mid-1960s, the field of theoretical linguistics has slowly fragmented into many different schools while at the same time being dominated by the figure of Noam Chomsky. How would you assess the current state of linguistics? Is it in a healthy state?

A: It was already pretty fragmented in the early 1960s, when I was a research student in London. I was faced there with a choice for syntactic theories between Halliday and Chomsky, and within a few years I was learning about Lamb's Stratificational Grammar, Hjelmslev's Glossematics, Pike's Tagmemics and a few others. I did my PhD at the School of Oriental and African Studies, so I did quite a bit of phonology, and there I was very much aware of the difference between Firth's prosodic theory and phoneme theory—or rather, between it and the various phoneme theories, including the Joneme of Daniel Jones, in UCL (just round the corner). Morphology had Item-and-Arrangement, Item-and-Process and Word-and-Paradigm approaches. And don't forget that historical linguistics was a much bigger part of linguistics than it is now, so we heard quite a bit of the big debate about whether sound changes were blindly regular or not. On the other hand, linguistics was a much smaller and newer subject then, with far fewer practitioners, hardly any degree programmes and a tiny literature (at least compared with today). So even though there was plenty of fragmentation, each of the fragments was much smaller and easier to grasp than in today's scene.

What about today? Not that different from the 1960s, but bigger in every respect—more people, more alternative theories, more conferences, more journals, more books, more branches of linguistics (like neurolinguistics and forensic linguistics). And lots more facts.

It's still very much dominated by American linguists—not just Chomsky, but most of Chomsky's opponents too. In fact maybe that's even more true now than it was in the 1960s, though recently Europe has started to play a much more important part through institutions such as the two Max Planck Institutes and we seem to have far more than our share of linguistics publishers. And we've got David Crystal and his superb encyclopedias. But research is certainly driven by American academe.

Is it healthy? Yes and no. I think diversity and fragmentation are what we need at this point in time when so many basic questions remain unanswered. If everyone was marching down the same road I'd say it was very unhealthy—with all our eggs in one basket, we'd all look pretty silly if that road turned out to be a dead end. It seems pretty obvious to me at least that alternatives in research should be considered in parallel rather than in series. Another healthy thing is that we're all much more specialised than we used to be. That's healthy because the schools tend to be specialised too—one range of schools for syntax, another for morphology, another for phonology and so on. That's good because the dividing lines in one part of language tend not to be relevant in other parts. Schools aren't a bad thing in themselves—they exist precisely because they bring together like-minded researchers. They're a social institution, and like any other social group a school helps its members to achieve things they couldn't otherwise achieve. Without schools we wouldn't have half as much research output as we have at the moment. And these schools are social rather than institutionalised, so (for example) some of the best journals are open to work from all schools—think of *NLLT*, which carries articles by mavericks like me as well as solidly mainstream ones.

The unhealthy part, of course, is that fragmentation is fragmentation. For one thing, although schools increase the flow of information among their members, they reduce the flow across school boundaries. For another, most linguists nowadays are so specialised that they lose sight of the larger picture. This matters because we all need to have at least some idea of how all the bits of language work before we can make good progress in one area. For example, if I'm working in syntax it's important for me to know roughly how my semantics and morphology will work as well, otherwise I have no way of deciding which phenomena belong to syntax and which to these other areas. But the same is true in any subject, and good linguists are capable of thinking laterally. So on balance I don't think the subject is in bad shape.

Q: It's sometimes joked that, if you put five linguists together, you get six different theories. Are linguists able to agree on anything?

A: Yes. As you know, I produced a list of 83 points on which linguists did seem to agree back in 1980, and I recently collaborated with April McMahon in expanding the list with a few more items. Obviously how you assess the situation depends on your perspective and where you look. I built my collection of agreed points for the sake of people in education—school teachers and those who train them—who (at that time at least) were struggling with our disagreements. So what I looked for were points that distinguished all linguists from non-linguists. For a linguist, the list is bound to be disappointing precisely because we take it all for granted—e.g. that there are no primitive languages, that all languages can be analysed, that there's no such thing as the oldest language. But from an educational perspective, these agreed points are really important precisely because many teachers, at least at that time, did not accept them. The same is true for our first-year students—most of the things we tell them are pretty much beyond dispute, but new or even contentious to them.

Changing perspective to you and me, as research linguists, the scene within linguistics may well look much more gloomy, especially if we look at high-level theories and analytical categories. Six theories for every five linguists sounds about right. But even here it's not all doom and gloom. For one thing linguists in general agree in rejecting post-structuralist relativism as far as our research is concerned, so we all think the issue is truth, and not merely how our society constructs reality for us. That's encouraging, and I think the reason is that we have plenty of really good data to get our research teeth into. Any linguist knows a long list of attested phenomena with names like extraction, clitics, cross-serial dependency, vowel harmony and so on, and nobody seriously disputes the facts although these "facts" are actually quite high-level and abstract generalisations. Many of these facts are relatively recent discoveries, but we inherited a vast collection of similar facts about language from earlier generations, some of them going back thousands of years, so our database of "things to be explained by any self-respecting theory of language" is pretty impressive. The disagreement comes at a higher level still, where we're trying to integrate the phenomena into more general frameworks.

Q: What do you think the major achievements of linguistics have been? What do you foresee in the future?

A: Maybe those encyclopedias that came out in the 1990s—David Crystal's two single-volume masterpieces, Bill Bright's *International Encyclopedia of Linguistics* in four volumes, and Ron Asher's *Encyclopedia of Language and*

Linguistics in ten. Every page records some achievement, whether it's an analysis of the phonology of Estonian or a general theory of phonology or language change, but it's the phonology of Estonian (and so on for thousands of other languages) that we should be most proud of. We should really learn to appreciate the more hum-drum descriptive work more, because this is really what most linguists do best—and where I personally get most fun and satisfaction. Maybe we pay too much attention to the disputes you asked about earlier.

And the future? More of the same, I hope, but I'd like to see serious progress in transmitting the ideas and findings of linguistics to the general public. That's certainly possible. Whether we're going to see major breakthroughs in higher-level theorizing I don't know, but I don't see why not.

Q: You might be thought of as a theoretical linguist with a descriptivist background. Should descriptive linguists be afraid of theory? And should theoretical linguists be afraid of description?

A: I suppose that would be a fair description of me, though I'm not sure how I'd balance my theoretical and descriptive parts. I just hate being muddled, so I welcome theory as a way of avoiding muddle. I'm still muddled about a lot of things, but less so than I would be without theory. I think most people are like that, though maybe some people don't mind muddle so much. So I certainly don't think descriptive linguists should be afraid of theory—they should see it as their friend, their support system, their essential tool. It would be a great pity if a linguist produced a worse description because of some imposed theory. Nor should theoretical linguists be afraid of description—that would be like a theoretical psychologist being afraid of experiments. I suppose anxiety is appropriate because it's the experiments or descriptive facts that will ultimately destroy the theory, but equally if it's a good theory they'll support it. I can see that there's a lot of tension between theory and description, and potentially they can conflict—my description may be theoretically muddled, and my theory may be descriptively wrong. And they can conflict emotionally as well—if all I want to do is to sort out the English auxiliary verbs, I may begrudge time spent on theory, and if I'm working on the theory of clitics I really don't want to learn all about the details of Serbo-Croat clitics. But ultimately description and theory are like two sides of a coin and you can't have one without the other, so I think we need to go beyond the fear stage.

Q: What are your feelings about the controversies surrounding the nature-nurture debate or modularity? Is there any resolving these issues?

A: There's obviously a lot of pretty unenlightening polarisation in both those debates, and I suspect to at least some extent scholars adopt positions which

suit them emotionally, because the facts and arguments certainly underdetermine the theories. Some people would like to think we learn a lot from experience, and others would rather think a lot is in our genes. I guess it's ultimately a matter of what you think humans are like, and how important you think cultural as opposed to biological evolution is. Personally I'd put my money on cultural evolution; and maybe that's because of my long involvement with sociolinguistics (and before that, my years with Michael Halliday, who's very much more interested in sociology than in biology). Learning from experience just seems so overwhelmingly important, as do the enormous differences between cultures. So regardless of academic arguments, I'm more inclined to believe that language is the result of nurture.

For rather similar reasons I'm reluctant to believe in modularity, simply because I see so many interconnections everywhere I look in language (and outside). Networks seem a much more plausible model of language than interconnected boxes. But I imagine that modularity has a strong emotional appeal because those boxes make people feel safe. If you're going on holiday you put all your goods in a couple of suitcases, because you can then deal with the suitcases instead of the individual items, and the items are all guaranteed safe so long as your cases are safe. Likewise for language modules—you can think about the entire boxes without worrying about their contents, and you can think about the contents of one box without worrying about how it's related to any other box. I actually think this is an illusion—you're kidding yourself if you think you can ignore other boxes and live, like a goldfish, just in one bowl.

My own view on both nature/nurture and modularity is that the focus of the debate should be on relations, not boxes. If knowledge really is a network of relations, then we should be asking where these relations come from (nature or nurture?) and how they differ (modularity); and in both cases the answer is the same. Relations are typed—i.e. classified—and relation-types (a) can be learned, just like non-relational concepts, and (b) divide knowledge into “modules”—e.g. all the concepts that are on the end of a “meaning” relation have a great deal in common, as do those which are identified by the “friend” relation. If we think of modularity in neuropsychological terms, we may expect brain structure to correspond roughly to network structure, so concepts that share a lot of relations will tend to be near each other. But this won't amount to a vindication of modularity; it will just show that the network has sub-networks with relatively dense internal links and relatively sparse external links.

Is there any resolving these issues? (Nice example of a gerund there!) If I'm right, the issues are ultimately emotional rather than intellectual: which view makes me feel most comfortable? Not many people are inclined to change their minds in the face of evidence. But the great thing about the nurture position is that even

those emotions may be learned, rather than innate, so future generations may have more scientifically “accurate” emotions! Meanwhile I think we have to proceed as though it was all a real intellectual debate, and each side should develop its slogans into specific and concrete proposals. The big debate about how we store and process irregular verbs is a splendid example of this, and shows how far we still have to go before we have proper theories rather than mere slogans.

Q: What can syntacticians learn from sociolinguists?

A: Humility is one answer I’m tempted to give. Sociolinguists don’t set out to produce Grand Theories, so there are no schools of sociolinguistics. They’re also very self-critical on matters of method and data, and are for ever wishing that their sociology was better. There are theories, but most sociolinguists are rather down-to-earth people with rather practical concerns and not much time for theory. At this stage in its development the subject probably has the right priorities—mainly collecting and cataloguing fairly low-level data.

Another answer, though, is a bit more technical. What sociolinguists do is to study the bits of language that carry “social meaning”—for example, about the social information carried by greetings such as *Good morning* or *Hi!*. What they’ve found is that social meaning is spread right through the language system, and not just in rather peripheral bits like the greetings. We even find it in syntax—hence your question, I suppose. Constructions often carry social meaning—for example, *Don’t you come home late!* is different from *Don’t come home late!* both in syntax (presence/absence of the subject *you*) and in social meaning (threatening/neutral). Similarly, there’s an important social difference between *he is* and *he’s*, between the house in which I live and the house I live in, and so on and on. The question for syntacticians is what they propose to do about this social meaning. It’s easy to put it in a footnote or to say it in prose, but what about formal models and analyses? Everyone agrees that syntactic structures map to semantic structures (of some kind), so why shouldn’t they also map to social structures (of some kind)? If so, how?

This is a very good example of the dangers of modularity and specialisation. If you think syntax is a module which is insulated from the rest of knowledge then you can ignore this question because by definition there can’t be any relation between a specific syntactic pattern and a bit of sociological knowledge. But if you take that position, you’re creating a problem for later generations: how to integrate your sociology-free syntax with the sociology—in other words, how to break open your module and redo the analysis without modules. They may even decide that your syntax isn’t good enough for them because you’ve ignored a distinction that the sociology requires. A nice example of this is that there are two kinds of sentence

that are syntactically very similar but sociologically very different. One is illustrated by *There goes John* and the other by *There lies my father*. Both are examples of “locative shift”, with a delayed subject and a front-shifted locative, but socially they are quite different—the first is very casual and commonplace, while the second is rather archaic and literary. The social difference actually correlates strongly with lexical and semantic differences, so the best analysis is also the one needed for the sociology. So the moral for a syntactician is that social meaning is just as important as any other fact about a construction.

On Educational Linguistics

Q: You’re a major advocate of educational linguistics. How did you become interested in the topic?

A: Through spending three years working for Michael Halliday. When I got my PhD my first job was as a research assistant with him on a text-analysis project, and when that finished I switched to another project that took most of his time, a vast project on Linguistics and the Teaching of English. (Vast in terms of length and resources—it lasted for six years and employed a dozen or so members of staff.) That was from 1967 to 1970, in my late twenties. I hadn’t had much interest in education before that, but my job was to teach linguistics—specifically, English grammar—to school teachers who were seconded for a year to take a course that we taught jointly with the Institute of Education. I doubt if anyone could have emerged from that without some interest in educational linguistics! It was a very stimulating environment, and I think I’m still working through a lot of the ideas that I picked up from Michael Halliday at that time. I think my interest was deepened a bit by my experience of sociolinguistics (which I started teaching at Halliday’s prompting). In 1980 I was one of the founding members of the Committee for Linguistics in Education, and I’ve had contact off and on with that committee ever since. The more I hear about language education, the more relevant linguistics seems.

Q: What do you think that school-aged children can get out of linguistic analyses?

A: A lot, but the main thing, I suspect, is the ability to pay close attention to linguistic forms—to look at the words as words, rather than (as someone put it) looking straight through the words at their meanings. As you know, attention is a prerequisite for learning, so if you don’t pay attention to the words you read

(or hear), you won't notice when they're in unfamiliar patterns, so you won't learn these patterns. I'm strongly in favour of grammatical exercises at school, especially if they involve some kind of diagramming system; they don't have to be boring or pointless, as they used to be in the Bad Old Days.

Q: In the United States it's not unusual for students to do their first phrase marker in college. In other countries, such as Spain, students start doing tree diagrams in the sixth grade. Do we need a middle road here?

A: No, I think Spain starts too late—in Russia they do the equivalent in the second grade! As you know I'm not a fan of phrase markers as such, so I'd prefer some other kind of diagramming system, but I don't see why they shouldn't start really early. Those relations are the most important part of grammatical analysis—the word classes are relatively easy, but relatively uninformative as well. If they can diagram then they can explore ambiguities in examples like *I saw the man with the telescope*, and start to learn about sentence meaning. I'm astonished how few of our undergraduates, who are mostly rather clever young people, are quite unable to “see” how the words in a sentence combine to make the whole sentence's meaning—e.g. to see that in *John asked Mary to go*, it's Mary who is going. This is the kind of thing that sentence analysis makes you think about, and once you can think like that about sentence structure, I'm convinced you're on the way to being a much more literate person—not only a better writer but also a better reader.

On Word Grammar

Q: You've been working on the theory of word grammar for some 20 years. What are some of the main characteristics of this project?

A: I tend to talk about three main “pillars” of Word Grammar: dependency structure, default inheritance and networks. But I suppose the most general of these ideas is that language is a network. That in turn leads to the general claim that this network is just part—or maybe a number of parts—of the even larger network of cognitive structure. This is why I think WG is a branch of Cognitive Linguistics, whose main idea is that language is an area of knowledge and shares the general characteristics of general knowledge—a very non-modular view.

Q: Word grammar uses default inheritance as its main representational mechanism. What are the strengths and weaknesses of this approach.

A: The main strength for me is that it builds on the very strong intuition that I share with a lot of people and that sees default inheritance everywhere else in cognition. It seems obvious that we apply it in everyday thinking about cats and birds and things in general; on the one hand we can inherit information from a super-category, so we can infer that if an object in the distance is a bird, it has skin; but on the other hand we can also ignore such inheritable information if we actually know better. This is exactly right for so many facts of language, where rules (inheritable defaults) very often have exceptions that override them. It seems to me that the idea of “default” captures all the insights that other theories express in terms of markedness, transformations, ordered constraints and other ways of handling competition between alternatives. The main weakness is that logicians hate default inheritance because it’s non-monotonic—you can draw inferences which turn out to be false. It’s said to be very hard to implement in computer systems because it involves a vast amount of checking and double-checking to make sure that the inference you’ve just drawn is not overridden. Personally I don’t see why this has to be so if information is organised in terms of inheritance hierarchies—if there is an overriding exceptional fact, it will be located in a very precisely definable area of this network, so you don’t have to look very far for it. Also (but this is a big extension for most computational linguists!) you can reduce the search even further by bringing in spreading activation, because any potential overrider will always be heavily active, so you just need to check the currently active nodes. But the opposition of logicians and computer people is a serious obstacle to any theory that uses default inheritance. I once gave a talk about default inheritance to an audience of logicians who only produced a single question at the end: why on earth was I bringing cognition into logic?

Q: Word grammar has no constituent structure, only word strings and dependency structure. What exactly made you give up phrase-structure grammar?

A: That’s easy. The first theory of syntax that I became familiar with was Halliday’s—that’s what I used in my PhD thesis, and my first book (*English Complex Sentences: An introduction to Systemic Grammar*, 1970) was about it. In fact, it was the first semi-formal account of Systemic Grammar (which is now called Systemic Functional Grammar). This is a version of phrase-structure grammar, based on the American post-Bloomfieldian grammars of the time—I think Halliday was particularly impressed by Archibald Hill’s grammar book. So while Chomsky was turning Harris’s version of Immediate Constituent analysis into early phrase-structure grammar, Halliday was turning Hill’s into early Systemic Grammar. Equally interestingly, they both found phrase structure on its own glaringly inadequate, so they both had to enrich it—Chomsky enriched it with transformations,

while Halliday added very rich feature structures (organised in systems - hence the theory's name). Anyway, that's the intellectual world I was in as a research student and research assistant. I was heavily influenced by both Halliday and Chomsky, though I could never accept transformations emotionally—I don't really know why. I think I just wasn't impressed by the arguments because I knew there were alternatives which weren't even on the menu.

Anyway I read Chomsky's *Aspects of the Theory of Syntax* quite carefully, and was impressed by all the discussion of subcategorisation which allowed a verb to select for a particular set of complements—for example, that *like* takes a *to*-infinitive but *dislike* doesn't (*I like/*dislike to go to bed early*). It took a while for the implications to sink in, in fact I suppose it must have been the best part of ten years! Not a flash of inspiration or anything like that. I asked myself how these important facts could be handled in Systemic Grammar, and liked one part of the answer: Systemic Grammar made it easy to identify the object because grammatical functions were identified explicitly, rather than implied by the phrase structure. That was a good start, but the rest of the answer was disappointing. The systemic answer was that a verb is linked to its complements via the mother clause's classification: this classification was so rich that it reflected every detail of subcategorisation. I was quite happy with this way of doing things for a long time—in fact my 1970 book was full of such things, and I felt rather proud of the machinery that allowed the results to fall out. Very generative, I thought.

But eventually I got disillusioned because all this machinery wasn't really satisfying; and those subcategorisation relations were especially unsatisfying. In fact, they seemed just wrong. The relation between a verb and its complement is just that—a part-part relation. There's no evidence that, say, *like* appears in one clause-type and *dislike* in another. It's like saying that if I like you that's because we belong to a "liking" dyad, in which one person likes the other, whereas we're in a "disliking" dyad if I dislike you. That was the early 1970s, and by chance I heard about dependency grammar through the work that John Anderson was doing in Edinburgh—in fact, to Michael Halliday's credit I think it was through him that I heard about John Anderson's work. Anyway, a direct dependency between the verb and its complements sounded like the solution to the problem, but I was still keen on the other bits of Systemic Grammar so I decided to marry the two in a theory I called Daughter Dependency Grammar—"Daughter" because it still had the mother-daughter relation of phrase structure, but "Dependency" because of the daughter-daughter dependencies that it had as well. That was my second book (*Arguments for a Non-transformational Grammar*, 1976). I was very pleased because Chomsky reviewed the manuscript positively, and Paul Schachter wrote an enthusiastic review in *Language*. But it still didn't seem right, as I had to accept

one summer when I decided to write a little paper for students explaining why both phrase structure and dependency were needed. I wrote the dependency bit first, and that was easy; but when I turned to the phrase structure bit I really couldn't think of any good arguments at all. So I concluded that there probably weren't any, so I moved to pure dependency grammar—hence Word Grammar.

I suppose the story has two morals. One is that teaching is good for research in the sense that it forces you to review basic questions and see them from the point of view of a potentially sceptical student. And the other is that developing a theory takes time and is a bit like language acquisition—as Jim McCawley said a long time ago, the child moves gradually from stage to stage, each time making minimal changes to the existing system rather than going back to the beginning and rebuilding from scratch. You tinker with a bit here and a bit there and gradually the whole thing changes into something quite different from what you started with; but each such change requires not only hard thought but a bit of an emotional upheaval. If you're fond of an idea you're also fond of all its parts, so you fight for them until the evidence becomes irresistible. A small change only causes a small pain, which is outweighed by the satisfaction of solving a problem, so gradual evolution of a theory is relatively bearable. But there's no doubt that Word Grammar in 2003 is a totally different view of language structure from Systemic Grammar in 1964 (when I wrote my thesis), although there's a continuous chain of intermediate theories between them.

Q: You've been highly critical of abstract syntax in the past, yet I've heard that word grammar is making some moves to allowing restricted use of empty categories. Can you explain this change in your thinking?

A: This is one of the small changes that I've just been talking about. I'm still very much opposed to Chomsky's abstract grammar, and my opposition is basically emotional—I just don't like that way of doing grammar, and never have. I'd like to say it was an intellectual objection, but I don't think I can put it into words so it must be emotional. (Maybe that doesn't follow, and maybe emotion and intellect aren't really that separate anyway.) I think one of my objections is the way abstract syntax is presented—look how clever we are, just like physicists with their invisible sub-atomic particles—as much as with the content of what they're saying. My view is that analyses should stick as close as possible to the concrete data that we're really sure of, and should get more abstract only when we really have exhausted all the other possibilities. Chomskyans seem to take the reverse view: prefer abstract analyses over concrete ones if they give any benefit. My view is very much in line with Halliday's Systemic Grammar, which is a very concrete system—so far as I know, empty categories have no place in it at all, and never

have. Who knows whether I prefer Halliday's analyses because they're concrete or the other way round.

Actually any grammatical analysis is very abstract, so we don't really have a choice between abstract and concrete analyses; it's really a question of where we put the abstractness: in the categories or in the relations, i.e. in the nodes or in the links. Chomsky puts abstractness in the nodes by allowing empty nodes, whereas I put it in the relations. Dependency relations are much more abstract than phrase structure because they're less closely tied to the concrete observable linear order; and obviously an unobservable empty node is more abstract than one which is observable. (Loose talk—I'm really talking about the abstractness of the thing represented by the node rather than the node itself.)

I always resisted the idea of unobservable nodes simply because I could usually see an easy way of doing without them. For example, I wasn't impressed by the arguments for empty subjects like PRO or pro because all the things they were supposed to explain could be explained very satisfactorily by assuming an element in the semantics—especially if we could identify that semantic element as the “subject argument”, the semantic argument that normally maps to the subject. I was often surprised how Chomsky and his followers could miss these rather obvious alternatives.

Then I started thinking about case-agreement in predicatives—that's something I first heard about a long time ago in an article by Avery Andrews about Ancient Greek (I never studied Greek, so it was all new to me). In Greek, predicative adjectives agree in case with their clause's subject, so if the latter doubles up as object of a higher verb, the object case shows up on the lower predicative. I'm thinking of sentences like *I consider John to be clever*, where the word for *clever* has the same accusative (or whatever) case as the word for *John*. I knew exactly how to handle such sentences in WG: the word for *John* depends on both verbs, and the accusative that it needs by virtue of being the object of *consider* overrides whatever default case it gets from *to be*. I thought the facts about predicative case agreement would provide some nice ammunition in favour of this analysis. Unfortunately it turned out to be otherwise—both Greek and Icelandic allow another pattern, in which the predicative does not in fact have the same case as the shared “object-subject” noun (e.g. *John*), but has the default case for the subject of an infinitive. That's really persuasive evidence that the infinitive does in fact have a subject—but its subject must be an empty node, it can't be the shared noun precisely because it has a different case. So the facts have dragged me, kicking and screaming hard, to accept some empty nodes. And once you accept some you start finding them all over the place, and the problem now is where to draw the line. I don't know the answer to that but I'm hopeful that someone will think of one.



For more information on Dick Hudson's work, click on the following link:
<http://www.phon.ucl.ac.uk/home/dick/home.htm>