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Interview with Elizabeth Closs Traugott

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The following interview was conducted via e-mail by Scott A. Schwenter during July 2007. The text of the interview has been edited somewhat from the original email exchanges.

SAS: *For a student who would like an introduction to your research over your 40 plus-year career, what works would you recommend as the most essential ones to read?*

ECT: Well, if you want to go really far back, avoiding books to start with, I suppose my article in *Language* (Closs 1965) on the development of members of the syntactic category auxiliary (or more properly what Chomsky had called Aux) would be the place to start. It is actually seriously flawed, because I did not question whether there was in fact such a category in Old English (nor did anyone else at the time, at least in print). But it illustrates what I suppose has driven my research throughout my career: the attempt to combine traditional work (in this case work on the history of English) with new theoretical insights (in this case Chomskyan syntax of the *Syntactic Structures* kind).

While I spent many years thinking about theoretical issues in language change, especially possible relationships between language acquisition, creolization, and language change, I think the next most important work a student would need to read is my 1982 paper in which I began to pay serious attention to pragmatic and ultimately semantic change as a factor in syntactic change. Also, I was troubled by Givón's (1979, 209) famous hypothesis that "discourse > syntax > morphology > phonemics > zero." Although the hypothesis fit well with then-current notions about acquisition, pidginization, and language change, I never understood how it was possible to have discourse without syntax, and so I was concerned to show that syntax changed *via* discourse (or more properly discourse practices). This paper got me started on two intersecting tracks: grammaticalization and semantic change. Grammaticalization is probably best represented in my book on the subject with Paul Hopper (2003). This is a considerably revised second edition of a 1993 version, and if one really wanted to find out how my thinking developed, one could read both versions, but I think that would be too much by far!

With respect to semantic change, over the years I revised my views about expressiveness, first put forward in this 1982 paper, and reconceptualized it as subjectification, what I now think of as the development of meanings that express speaker attitude or viewpoint. My example in the 1982 paper was the development of *while*, which, in the construction *þa hwile þe* 'the time that' came to have the concessive polysemy 'although.' 'Although' is a subjectively construed relationship. Once I had seen this change, I found it all over the place, in changes from the modality of obligation to that of conclusion (the difference between *You must go* and *You must be happy about that*), from verbs that refer to speech events or cognitive states to verbs that can be used performatively (the difference between the report of a promise or recognition and use in expressions like *I promise to go*, *I recognize the senator from Michigan*), from expressions of purpose to expressions of exhaustive focus (the difference between *All I did was to honor her* 'everything I did was for the purpose of honoring her' and *All I did was (to) order the wine* 'I only ordered the wine' [the so-called "pseudo-cleft" or "specificational focus" construction]). A fairly recent update can be found in my 2003a paper on subjectification and intersubjectification (the development of polysemies that pay attention to the addressee in the communicative dyad), and of the importance of distinguishing them from subjectivity and intersubjectivity, which are always ambient in any communicative situation.

The extent to which semantic change needs, in my view, to be thought of as the codification of what at an earlier stage were pragmatic implicatures, is outlined in my 1999a paper.

Finally, I should mention my paper with you (Schwenter and Traugott 2000), since that got me thinking about the way in which "dialogic" contexts may lead to adversative meanings. For example, in making a contrast, a speaker takes on different perspectives. In our paper we studied the way in which *in fact* came to take on epistemic modal meanings in contrastive contexts. A nice example from my current work on the pseudo-clefts is,

(1) By all which your Honours may perceive, how he hath falsly traduced the Commissioners of the Navie, . . . and ***all he drives at, is by his unjust aspersions to bring the Parliament and them at ods***, that so he might accomplish his own ends (1646 msch [ICAME: Lampeter]).

From the perspective of the person reported on, he presumably thought that everything he did was for the purpose of causing conflict; from the perspective of the speaker, however, what he did is evaluated negatively, as signaled by *falsly*, *unjust*. Extensive use of the "All Pronoun did was X" construction in dual perspective uses of this kind presumably led to its conventionalization as an autonomous construction that nowadays does not need context to be understood as specificational focus.

A tertiary research line has been in linguistics and literature. Apart from the 1980 book on the subject, a more recent paper of interest might be the one listed in the references as 1999b. But this does not break new ground of any sort. The research I have done in this area has certainly helped me understand the textual records that I now use as my database.

SAS: *What are some of the other works that you consider essential readings for students interested in semantic change?*

ECT: I'll suggest a few that represent different theoretical approaches. Without question, the first chapter of Sweetser's (1990) book, in which she outlines a theory of metaphorical mapping from vision/hearing/tactile acts to intellect (e.g., *see* meaning 'understand'), and what she calls the socio-physical world to the world of reason and to that of speech acting (her synchronic examples include expressions like *You may go, I permit you* (socio-physical permission), *Kim may be tired* (conclusion/"epistemic" modal, expressing one's reasoning/belief based on evidence), and *Kim may be a nice guy but I don't trust him* ("speech-act" modal quoting someone else's view). Blank (1999) seeks to explain meaning changes against a cognitive background in terms of salient perceptions, salience, similarities, etc. These are all concerned with metaphor. Horn (1984) develops a pragmatic theory based on neo-Gricean principles of Quantity and Quality. Brinton's (2007) paper on the development of *I mean* is an important article on the methods of using electronic corpora to account for pragmatic-semantic change. An intriguing topic is outlined in Adamson's (2000) paper: she shows how certain adjectives become more subjective (and abstract) as they are moved leftward in the determiner phrase and conventionalized there, e.g. *lovely* in *a lovely little problem* (in this instance, *a little lovely problem* is most unlikely).

SAS: *How did you first become interested in the study of semantic change?*

ECT: It started with work associated with the Stanford Universals of Language project, directed by Charles Ferguson and Joseph Greenberg. I wrote a paper (1978) on cross-linguistic evidence for the space > time shift, investigating spaces used for tense vs. aspect. But it was actually the observation about *while* that I wrote about in the 1982 paper that hooked me.

SAS: *You are probably most well-known around the world for your seminal work on grammaticalization, as exemplified most obviously by your Cambridge University Press textbook with Paul Hopper. How and when did you start working on this topic?*

ECT: As I mentioned, my preference in doing research is to explore new ideas, my own or others'. Grammaticalization is a subpart of syntactic change, which had been my original interest. A subset because, for example, in those days I did not envisage it as involving word order (though I am beginning to revise my views on that, at least where word order changes involving

information structure are concerned). Grammaticalization began to be talked about in the U.S. in the late 1970s and so I set to work to try to understand it. Since in the early days much of the work in grammaticalization was on the development of auxiliaries, and I had started my career writing on them in English, it was natural for me to explore it. The hypothesis of unidirectionality that lexical items or constructions become grammatical ones, not vice versa, which was articulated by Meillet in 1912, is a strong hypothesis, and testable, so I wanted to see how far I could go with it. I have enjoyed thinking about the challenges to unidirectionality that have been posed, but have no interest in the alternative “random walk” hypothesis, since it is untestable. The evidence clearly suggests that unidirectionality is a strong (though not necessary) tendency. So the next question is why, and this is where work on discourse factors and negotiation of meaning come in.

SAS: *Relatedly, how do you position your work on grammaticalization with respect to the more general upsurge of interest in the topic in the early 1980s?*

ECT: In the early 1980s the main contributors to work on grammaticalization were working on cross-linguistic typology at the University of Cologne, Germany: Christian Lehmann, whose monograph *Thoughts on Grammaticalization* ([1982] 1995) was at that time available only as a working paper and was hard to get, and Bernd Heine (see Heine and Reh 1984). Both worked on cross-linguistic typology and focused on morphosyntactic issues, Lehmann in European languages with long histories, Heine with African languages, the histories of which were largely unknown. In the U.S. Givón and Greenberg were also working on morphosyntactic change. I think my main contribution was thinking about the pragmatics and semantics involved in grammaticalization, and conceptualizing it as arising in the flow of speech, and in that sense metonymic to it. Later in the 1980s, others explored the semantics involved in grammaticalization in terms of metaphors (Bybee and Pagliuca 1985; Sweetser 1990; Heine, Claudi, and Hünemeyer 1991).

SAS: *A constant in your research agenda from the outset has been the study of language change, whether syntactic, semantic, or otherwise. What originally drew you to this area? Why do you think the study of language change is valuable to linguistics?*

ECT: I guess I sort of fell into historical work. When I was in high school (in the UK), I applied to Oxford and apparently did well on the test they then gave on grammar. They asked whether I'd like to do the course on the history of English. I tried it and I liked it. The question of why the study of language change is valuable to linguistics is a very different one. I think it is valuable in many ways, perhaps the most obvious of which is that it puts a perspective on variation. Assuming variation in language is the result of change, and also a reason for it, knowing what structures were available in the past and

the direction in which they are likely to be taken can help the researcher assess what is innovative or not. As an example, a group of us were studying the use of *all* as an adverb of intensity, meaning something like *very*, as in, *She's all sad*. This had been said to be an innovation, and my colleagues thought it was. They thought I was quite weird when I said I was sure it wasn't. A little research showed that it goes all the way back to Old English, and has been used in more and more contexts over the last thousand years, so that now we get expressions like *She's feeling all Joan Crawford*, which fits in with the expansion of other degree modifiers to nominal environments (e.g., *rather fun*, *pretty cowboy*, on analogy, I think, with *a bit/lot/shred of* in their intensifier sense). So why do people think intensifier *all* is new? As my colleague Arnold Zwicky has suggested, people, including linguists, may have a "recency illusion": what has been recently noticed is assumed to have developed recently, or an "adolescent illusion": what is associated with adolescents is assumed to be recent. In the case of *all* there is a new phenomenon, use as a quotative (*She was all "What!"*), which did seem to be associated with adolescents in the 1990s (Buchstaller and Traugott 2006). Since 2005 it appears primarily together with *like* (*She was all like "Stop it!"*). Which brings me to another value that historical linguistics can bring to variation studies: many people seem to assume that once a phenomenon has come into being, it will spread and "go to completion." Far from it. The historical data are full of things that started and then disappeared, including *a deal of* meaning 'a piece of,' or that started and then became caught up in other constructions. The study of language change, in other words, shows us constraints on how things can vary over space and time. I think it's also important to recognize in all domains of research (and life) that the present is the outcome of the past, and that there are lessons to be learned from the past, whether political, moral, or linguistic. The twentieth-century concern with synchrony and the present was an intellectually important time, but it did not help us to understand ourselves very well, or to value things of the past, including our environment. I am glad to be part of what I see as a gradual shift back toward respecting and valuing research on change, whether of language or the climate.

SAS: *How did the introduction of searchable electronic corpora change the way you conduct research?*

ECT: I'm glad you asked. It radically changed the work I did. Instead of continuing to do cross-linguistic typological work looking for possible evidence of the development of, for example, temporals from spatial (like *after* from 'behind,' as in *aft* of a boat), or of conditional markers from temporals (like the *while* example) and other sources, I started to seek textual evidence for the kinds of pragmatic contexts in which such changes could have arisen. This meant my work shifted from using grammars as sources of data to the

Helsinki Corpus (a diachronic corpus of the history of English now available along with many others, both diachronic and synchronic, from ICAME), and subsequently any corpora that I could get my hands on. This work showed me that potential new meanings were often available redundantly in the text, and probably enabled the change.

SAS: *In your work with computerized corpora, how do you determine when/whether a change has occurred in the language? Are there clear steps of inquiry that you follow when examining the data?*

ECT: The first essential rule is to keep track of dates; if a specific date is not known, approximate ones are fine, but without a date, one can assess nothing, since any hypothesis about change requires one to know what came before what. Then if I find just one example of a possible new meaning or morphosyntactic structure, I treat that as a possible innovation, but look for at least two or three more examples in other texts before being willing to posit a change. This is because in my view, innovation does not constitute change. The innovation must spread to others to count as a change. Often one finds a sudden (but typically small) increase in the number of examples during a certain decade. For example, there is a famous fifteenth-century example of a possible early use of *be going to* as a future, but I would not count that as evidence for a change, because it is ambiguous, and there are no other examples for about another century. When they do appear, there are several. In my work on the ALL-pseudo-cleft I have found no convincing examples of the construction before 1600, but around that time several begin to appear. Of course, because of the conservative nature of writing, changes may not be represented in our texts until some time after they appeared in conversation, so we can only conclude that the change happened by the period for which we have examples.

SAS: *To what extent do you feel the diachronic generalizations relevant to the study of subjectification and/or grammaticalization can be found (or replicated) in the investigation of synchronic phenomena?*

ECT: In my view (though this is certainly not shared by everyone), the stages of development that one sees over time have theoretical validity as stages only to the extent that dates can be attached to them. It is true that our textual data record is not complete (but then our synchronic one is also not complete!), but for many languages it is quite extensive. We are very fortunate in having a lot of data for English (much of it in electronic form now), for many European languages, and, as an invaluable check on generalizations, for Chinese and Japanese. (Unfortunately, however, access to corpora for the histories of languages other than English is quite limited, though it is coming to be less so.) While it is often possible to see the outcome of changes in synchronic variation or polysemy (e.g., *must* is used in both its original obligation and in its newer conclusion sense, *a bit of* both in its

original partitive sense and in its more recent intensifier sense), there is no direct relationship between the historical changes they underwent and current frequency of use, idiomaticity, etc. It is fine to organize one's synchronic data along "clines" or "paths" that match the history, as anticipated from generalizations made in research on grammaticalization or semantic change (e.g., more subjective polysemies will have developed later than less subjective ones), but in my view that is simply an issue of organization and has no theoretical status. Vice versa, if all one has is synchronic variation, one can hypothesize the historical development, but cannot claim that it is factual. This is because systemic changes may occur in a language, as happened when case got lost in English and the *-s* genitive became a clitic. What I do think is valuable is to take known generalizations as hypotheses about possible changes, and, if one has historical data, use them as guides for research, provided, of course, that one is willing to find counterexamples, or to modify the generalizations with new data. That is just good research practice.

SAS: *Even though you retired from Stanford several years ago, you are still one of the most active scholars in linguistics today. In what direction(s) do you see your current research heading at this point and over the next few years?*

ECT: It was only three years ago. At my age that doesn't seem like "several"! I have been engaged in two exciting strands of work during those three years. One is related to the fact that my daughter lives in York, England. Having taught at the University of York in 1966-67, I have always had fairly close ties to the linguistics department there and was privileged to be able to do some work with Susan Pintzuk on the Old English parsed corpus. Pintzuk was a major contributor to the syntactic parsing of the corpus; we took on the challenge of developing code for retrieval of information structure, as part of a larger project that she is engaged in on word order in Old English. As a pilot study we investigated object topicalization in Old English, and I subsequently investigated left-dislocations. This got me thinking a lot about information structure and also got me into frequency studies for the first time. The other strand of work, which I have been invited to give many talks about around the world, most recently in China and Brazil, and in the fall in Japan and Germany, is grammaticalization and construction grammar. For many years I have been saying, as have many others, that not only lexical items but also constructions are input to grammaticalization. I even wrote a paper on grammaticalization and constructions that was published in 2003 (listed as 2003b). But I felt I did not really understand what "construction" meant, so was eager to learn about constructions as developed in construction grammar. Of course, there are many versions of construction grammar, but I chose Croft's (2001) version, Radical Construction Grammar, mainly because it was in part designed to account for grammaticalization.

The challenge is to take a primarily diachronic theory (grammaticalization) and merge it with a primarily synchronic one (construction grammar), but you know I enjoy that sort of challenge. I think many useful perspectives can arise out of the merger. For one, grammaticalization studies have always involved form-meaning pairings, even though the formulae are monostratal (e.g., main verb > auxiliary > clitic > inflection); merger with construction grammar forces one to look directly at form-meaning pairings (I often say it's like putting on 3-D glasses). For another, construction grammar's focus on alignments and networks help us understand better the hypothesis that grammaticalization involves gradualness (small step-by-step changes) and micro-reanalysis. As is coming to be increasingly widely understood, these micro-reanalyses are driven by analogy, indeed they *are* analogical extensions (a point also made from the very different perspective of Optimality Theory by my colleague Paul Kiparsky).

Since 2005 there have been a number of conferences devoted to understanding how grammaticalization and construction grammar can inform each other, and it has been a pleasure to contribute to this new direction in research.

SAS: *Textbooks in historical linguistics have a tendency to devote very little space to issues in semantics and pragmatics, and even when they do the kind of data and approaches to such data presented are often very different from your own. Why is this?*

ECT: I suppose it's because textbooks tend to look back rather than forward. They cover what is agreed upon. Semantics was not much studied during the first eight decades of the twentieth century, and so textbook writers mainly looked to Ullmann (especially 1964) for examples. These examples tended to be drawn from nouns, which, being referential, are especially subject to external factors, such as changes in the concept of *car*, or *taboo*, and to be listed under contradictory headings, such as pejoration and amelioration, narrowing and broadening. Therefore, although the examples and changes identified are all legitimate, it was natural that people thought about semantic change as somewhat haphazard. But things have started to change, and Campbell's (2004) *Historical Linguistics: An Introduction* mentions recent work on semantic change.

SAS: *Given that academic positions in historical linguistics do not seem to be very plentiful these days (if they ever were), what kind of advice would you have for graduate students who are interested in historical linguistics?*

ECT: Historical work depends crucially on good synchronic analysis, so it is essential to know and do good synchronic linguistics. Historical linguistics provides a corrective to many assumptions about variation, evolution, the role of writing, etc. For example, in a recent article on non-prototypical –s-genitives in English Rosenbach (2006) draws on evidence from Old English

to support her argument that a semantic rather than a syntactic analysis of possessives with indefinite rather than definite readings is needed (e.g., *a physical therapist's assistant*): essentially, the historical part of the argument is that since Old English did not have articles, syntactic analyses that depend on them are inadequate. So even if there are no or few historical jobs, nevertheless, historical material can be brought into virtually any course or line of research. And the good news is that there is increasing interest in variation and change, and in various aspects of change, so there are growing opportunities to teach at least a course or two on historical linguistics. Language departments are often interested in a course on the history of the language. So students should not assume that because there is no historical job per se, there will be no opportunity to work on and teach historical subjects if they want to. And they should encourage the departments in which they get jobs to put on historical courses.

SAS: *What do you see as the most exciting developments in the field (both narrowly and broadly construed) these days?*

ECT: I think there is a lot of energy in linguistics these days, as more and more attention is being paid to interfaces, for example between pragmatics and syntax (here information structure is important; also multiple perspectives/dialogicity), or between pragmatics and phonetics. While we are very far from understanding the neurological underpinnings of language, I suspect that over the next few decades we will learn much. I have often said that linguistics is like an octopus, with tentacles reaching out in all kinds of directions, from traditional ones like literature, philosophy, and education to computer sciences, artificial intelligence, and now neurosciences and genetics. It's an exciting time to be a linguist. We need to learn better how to tap the public's great interest in language. Earlier this month I was most impressed in São Paulo, Brazil by the museum of Portuguese language there. It combines information on the history of Brazilian Portuguese from Indo-European to the present with interactive displays on African and other languages that form part of the heritage of the language, to readings of poems with projections on the auditorium walls and ceiling of key words. It is so popular, we had to stand in line for 20 minutes to get in! It would be good if we could do something comparable in the U.S.

SAS: *Following up on that last answer, what areas of investigation in historical linguistics, specifically, do you deem under-explored, and promising avenues for future research (either your own or that of others)?*

ECT: The most important thing is to do more work on languages other than English. As I said, semantics was not studied much until recently. Even less was done till the later part of the last century on information structure, interactional meaning-building, and similar pragmatic topics. There has naturally been a time lag in historical work, but a considerable amount of

research is now being done on English, French, and other languages, including Japanese. I am thinking for example of the work of Ans van Kemenade (2006) on English, and Sophie Prévost (2006) on French. Just this year a special issue of the *Journal of Historical Pragmatics* (2007, vol. 8.2, edited by Noriko Onodera and Ryoko Suzuki) is devoted to historical Japanese pragmatics. Susan Pintzuk and I have investigated left dislocation and object topicalization in Old English. Coding the syntactically parsed *York-Toronto-Helsinki Parsed Corpus of Old English Prose* (Taylor et al. 2003) corpus for information structure properties has been quite a challenge (Traugott and Pintzuk forthcoming). It's hard enough to code pragmatic data for a living written language that has punctuation conventions for sentences. It's even harder to work with texts written with different conventions (in Old English it was used to mark prosodic, not syntactic factors, and most definitely not what we would think of as a "sentence"). Most corpora depend on editions, which usually add punctuation along modern lines. As we did our work, we actually came to question the analysis of a couple of the complex sentences in the parsed corpus. I think there is a lot of worthwhile work to be done along these lines, particularly with focus on the interface between information structure, interactional negotiation (pragmatics), and word order changes (syntax).

SAS: *You have long been a central member of the Linguistic Society of America (LSA), serving both as President (1987) and for a number of years as Secretary-Treasurer (1994-1998). What kind of impact have these positions had on your professional development? What do you see as the role of the LSA and similar organizations in the field?*

ECT: It was a privilege to serve in these capacities. I think the main impact on my own work was that it made me more aware of how extensive the range of linguistic work is, and what potentials there are for research across fields. Interdisciplinarity is a major issue these days; linguistics has always been interdisciplinary—historically it has been connected with literary studies, anthropology, and philosophy, more recently computer science and neuroscience, to mention only a small portion of the connections. Being president of the LSA brought this home vividly as institutional realignments or themes of Summer Institutes came under discussion, and as categories for submission of papers for meetings were discussed. Most other organizations are specific to particular topics, e.g. endangered languages, historical linguistics, creolistics, and cognitive linguistics. The role of the LSA is to bring the sub-disciplines together and to help foster new research across the sub-areas. I think the Summer Institutes, and the various invited plenary sessions at annual meetings, are going a long way in that direction, but of course there is always far more that can be done.

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