AN INTERVIEW WITH JOHN SWALES

Sally Burgess
Universidad de La Laguna

In November 2000 I had the good fortune to attend the conference on *Research and Practice in Professional Discourse* at City University, Hong Kong. Of the many such events I have attended over the years, this stands out as among the most fruitful, for me personally and, I hope, for the *Revista Canaria de Estudios Ingleses.* I was able to solicit contributions for this issue of the *RCEI* from Paul Thompson, Brian Paltridge and John Flowerdew, all of whom I had met before. I also had the chance to meet Chris Candlin and ask if he would agree to republish his and Jennifer Thurstun's paper. Ken Hyland spoke immediately before me and I was thus able to meet him and to invite him to contribute. If this were not enough, the key-note speakers at the conference were Charles Bazerman, Yuegou Gu, Jim Martin, Srikant Sarangi, Ruth Wodak and John Swales.

I read *Genre Analysis* shortly after it was published in 1990 and had one of those epiphanies in which I suddenly knew that my thesis topic —until then loosely termed 'a comparative study of texture in Spanish and English'— would in fact be a genre analytic study of academic discourse produced by English-speaking background and Spanish scholars. I know, of course, that those who had similar reactions to *Genre Analysis*, and indeed to John Swales' earlier work, are legion. It was partly my awareness of this that constrained me when, over the years, various friends and colleagues suggested I contact the man himself. My reaction was always "But what would I possibly have to say to him?" *Genre Analysis* left me with very few dilemmas urgent enough to prompt me to bother one so eminent whose advice was inevitably sought by many with more pressing needs than mine.

Nevertheless, I did have a list of questions that had pricked my curiosity on first and subsequent readings and which had often made me wish that I might indeed have the opportunity of putting my questions to John Swales face to face. And there he was on the conference programme and there I was with a ticket to Hong Kong and the guest-editorship of the *Revista Canaria de Estudios Ingleses* looming on the horizon. Françoise Salager Meyer, another contributor and long-standing mentor of those of us in the *Departamento de Filología Inglesa y Alemana* with an interest in academic discourse, was kind enough to send John Swales an email message telling him to look out for me at the conference. She thus eased the task of approaching him with my petition considerably, though all those who have had the pleasure of meeting him will testify to the fact that he is extremely approachable. Approached he was and my petition accepted. The jewel in my conference crown was thus that John Swales agreed to be interviewed.

As there was little time at the conference itself, we decided that I should get in touch when I got back to Tenerife. I had hoped that we might be able to meet up during the AESLA conference in April or failing that that I might even persuade John Swales to visit Tenerife, but it became clear that we would not actually be in the same place at the same time at any stage. An email interview was the only option.

It was then that I realised that the academic journal interview was a genre which remained, for me at least, one in which the conventions were anything but explicit. I therefore set about doing a little rather *ad hoc* genre analysis of my own. I was struck by the fact that the best interviews were clearly the result of a genuine dialogue with each subsequent question arising out of the previous response. Even if the interviewer had perhaps compiled a list of questions beforehand this was never obvious. Some of the interviews I looked at, however, were rather too clearly the result of a list of questions having been dispatched to the interviewee, who had dutifully provided answers. This somehow gave the impression that the person conducting the interview was bored or even slightly irritated by the responses and the effect was a little like a police record of interview or perhaps an examination question paper and answer script. I was very anxious that my interview with John Swales should not read like this. I asked if I might send him one question at a time. He agreed to this and what you have here is the result.

The only disadvantage of this one-question-at-a-time approach was that the interview took a long time to complete, though this was almost entirely due to my tardiness in sending the next question and never to any omission on the part of my interviewee. I sent my first question in April and received the last response in early December. This must, at times, have seemed more than a little tiresome. Instead of taking a mere afternoon away from far more engaging projects and interests, I had compelled John Swales to maintain a dialogue with me over a period of almost eight months.

For me, as readers will readily appreciate, conducting the interview was a thoroughly delightful task; reading the responses and trying to choose between the many questions that they would prompt seemed both a privilege and a luxury. I hope that others will judge that I have made good use of this privilege and that at least some of my questions are those they too would have wished to ask. I need make no plea for sympathy for the answers; I know they provide enriching and stimulating reading for all those who have an interest in genre studies of EAP.

I think the thing that stands out for me in the interview is how generous it is. I mean this in terms of the time and care given to each response but also in the way that one who is such a major figure in the field consistently cites, quotes and draws our attention to the work of those who are less well-known. This desire to celebrate the contribution that others make is something for which John Swales is renowned. I thank him for giving me and those who read this interview so much that is truly novel and for providing such insightful reflections on the past, present and future of the study of academic discourse.

SB: The last two linguistics issues of the *RCEI* have been devoted to Systemic Functional Linguistics (issue 40 *Systemic Functional Linguistics across Genres: Present and Future Perspectives*) and research carried out in the framework of Simon Dik's Functional Grammar (issue 42 *Challenges and Developments in Functional Grammar*) respectively. So, in a bid to achieve some kind of coherence, I though we might begin by discussing your attitude to functional theories.

Meriel Bloor says that your work on genre analysis should be understood as "a contribution to the field of systemic functional linguistics" (hence SFL). Do you agree with her?

JS: In the 1960's I was trained in syntax at Leeds University by Michael Gregory, who at that time was teaching a version of category-and-scale grammar. This was a valuable grounding, fast-paced, and with excellent exemplification. However, our attempts in discussion to come up with problems were somewhat brushed aside. I remember arguing for instance that the grammar at that time had to provide entirely different analyses of "You need to go AND see the doctor" and "you need to go TO see the doctor", and that this didn't seem right.

In subsequent years, as I became more involved in ESP materials etc., I looked upon SFL as a resource, rather than an inherited body of knowledge. I took what I needed, and also was much impressed by the interesting things about the English language that were being said by Bolinger, Sandy Thompson, Quirk, various Scandinavians and the like. I was like a guy looking at a hugely complicated classic French recipe and deciding "well, I could do a version of this that was much simpler" although doubtless lacking some of the nuances of the original.

I was also never very happy with the field/tenor/mode tripartite contextual analysis scheme since, especially as an applied discourse analyst, it didn't seem to allow sufficient prominence to "communicative purpose."

Over the last decade, I had sort of been hoping that SFL would become a less closed body of dedicated adherents, and begin to realize the values of discoveries in alternative approaches. If anything systemicists have become even less open to quote work from outside the "charmed circle." Indeed, I was very struck when Jim Martin was questioned about this in China last fall and he responded "we don't want our theory to be contaminated."

So, all in all, I don't think that my work on genre analysis is recognized as being any kind of contribution to SFL; rather, it is firmly situated in the LSP field, where others, such indeed as Meriel, may have interest and expertise in Hallidayan types of functional grammar.

I hope this answers your interesting question, at least in part.

SB: Yes, indeed but it invites several more! There are two issues I'd like to take up. The nature of applied discourse analysis is something I shall return to in a moment, but first I'd like to probe your dissatisfaction with field, tenor and mode a little more.

JS: Field, mode and tenor emerged out of Halliday's early work on diatypic varieties, and for this it has doubtless been very helpful. Thirty years later we still have a tripartite division, and what's so magic about the number three? Right at the beginning Crystal and Davey had produced a rather more sophisticated scheme, but it never got the attention it deserved. If you look at particular communicative events or genres, you can see that sometimes not all three are of equal relevance, and at other times, you may need greater specification than the tripartite model provides. One instance concerns field and how it may have trouble in coping with topic or content when the purpose is unexpected, such as in a parody.

SB: Now I'd like to turn to applied discourse analysis. You said earlier that the impetus for much of your own work arose from the need to produce EAP materials. I wonder if you could tell us a little more about your early career as a classroom teacher and materials writer and how this led you into the discourse analysis of academic writing.

JS: I didn't do any "research" in my first five years as a teacher in Italy, Sweden and at the University of Libya in Benghazi. Although in the last post (1963-65) I did do some materials for the Faculty of Economics and Commerce, including helping Bill Frazier (who went on to spend many years in Saudi Arabia) on an Arabic-English glossary of Economics terminology.

I then spent a year as a postgraduate student at Leeds on an advanced diploma in Linguistics and ELT (I think it was upgraded to an MA the following year). It was there, inter alia, that I was taught category-and-scale syntax by Michael Gregory, as I mentioned earlier.

I then got a lectureship at the University of Libya in Tripoli, and soon after became head of the English Section for the College of Engineering. With James Cormick, I analysed during, say 1967-69, much of the seven required English-language technical textbooks used by the first year students in terms of the following:

a) Lexical verb frequencies counting types and tokens until we reached a total of 200 different lexical verbs (a total which we never reached in the math text-which told its own little story).

b) Frequencies of those verbs in terms of tense, voice, aspect and modality. This exercise in effect "told us" which tenses and aspects we did not need to teach, e.g. the pluperfect and the continuous aspect.
c) A first and more informal look at the chapter introductory sections and at the structure and form of the test-and-discussion sections at the end (if any).

Some of this stuff found its way into my first textbook *Writing Scientific English* (1971), and an account of all this got written up in an early book chapter "Writing Scientific English" published in that pioneering English for Specific Purposes collection edited by McKay and Mountford (Swales 1978a). The quantitative work was published in a small article which appeared in ESPMENA Bulletin, 4 (Swales 1978b), which we had started in Khartoum in 1974.

I did not do much further work along these lines until I returned to EAP in 1973 in the Sudan. I think there I began to gain a better understanding of texts, what their purposes might be, how text purpose and form might be related, and how this might be used for teaching and learning materials. But I didn't really see any of this until I began in Khartoum to struggle with producing materials for the first year students in the Faculty of Law —and then later for third and fourth year students in Geology and Architecture. The novelty of legal discourse especially was a wake-up call, forcing me to try and understand why these weird texts had evolved in their peculiar ways. Clearly these struggles led to all sorts of materials, and I worked alongside Arnold Spencer and Jim Croft on these. It was a given, of course, that no relevant materials would be available since law does not "travel well" from one culture to the next, and because (as we later discovered) publishers had little interest in this area.

Ian Pearson, my number 2 in ELSU, Khartoum, produced the focus series volume on "biology" based on his University of Khartoum EAP teaching experience, while Paul Fanning and I published that rather esoteric little number "English in the Medical Laboratory." This last went considerably further toward finding good form-function correspondences than I had done in Libya, but it was the law work that drove me towards discourse analysis. However, I didn't have a concept of genre then; that came I believe from reading a review of one of Geertz's books in some sociology journal in the serials section of the University of Aston library in about 1979. This was a "light-bulb-going-off" experience, which became an important part of *Aspects of Article Introductions* (Swales, 1981), which I wrote in the second half of 1980. But that, as they say, is another story.

SB: What do you now think of the early frequency work that found its way into *Writing Scientific English* and into some of your other early publications?

JS: There are two main points, I think. The first concerns the need for ANY KIND of research as a basis of pedagogical materials. Looking back, I wouldn't of course do exactly the same kinds of things that I did in the late 1960s, but the same kind of exclusionary principles apply. Since we found hardly any occurrences of continuous tenses in the Libyan engineering student target textbooks, so then they got no mention in *WSE*. We also found very few conditional clauses, so again I made a point of excluding those. However, the latter exclusion might, on reflection, be nothing more than what is called today "a sampling error"! Further, we certainly did not exclude progressives from the special language lab materials that Jim Cormick, Harvey Webb and I developed. I have in fact addressed the issue of research in textbooks a couple of times; first, in one of my very first articles —and I think the very first article ever published in *English for Specific Purposes* and much later (in about 1995) in the same journal. The first argued for previous textbook arrangements so that local inputs by local teachers would be encouraged, appropriate and necessary. The second attempted (unsuccessfully as it happened) to redress the general academic and intellectual disdain which attaches to ESL or composition textbooks in the US, where —at least in research universities— they are seen as purely commercial activities little connected to any research front or research eandeavor. I tried to illustrate this with extracts from *Academic Writing for Graduate Students* (Swales & Feak 1994). That piece fell, I think, on deaf ears. So, to conclude my first point, research is good for textbooks, equally whether your own or emanating from elsewhere, but that does not mean that EVERYTHING you say in a textbook or course pack has to be backed up by research; if so, we would never get done! Good faith efforts then.

My second point concerns quantification. In those early days, quantification was all. Sixties linguistic science! Today I use quantification as much as a heuristic, an exploratory technique, as I do to prove my case with the crushing weight of numbers. Scanning texts and transcripts for occurrences of X (whatever that might be) is a good preliminary way of getting to know a new corpus of material, or indeed for revisiting one you know well. So let me give you this week's example. The latest Morley Scholar to arrive for a two month research stay is Dr Julia Bamford of the University of Sienna, one of whose current research interests is the manifold uses of "here" in Economics lectures.

Heretofore I thought "here" was a pretty dull word actually, just one of those deictics and certainly less interesting in research writing than "this." So, to cut a longer story short, I have been going back this week to Hyland's corpus of 80 research articles (thanks Ken) and have been looking for instances of "here." There were, to my surprise, 300 of them; not massive, but not piddling either. Then I found that 80 of these occurred in sentence-initial position (great news for Hallidayan's who dream of pre-themes!). Now I am really interested. What am I doing when I write "Here my focus is" as opposed to "My focus here is"? And is "here" in writing deliberately vague about its textual scope (using Bunton's term) —"now"? in this work? in this paragraph? Watch for next installment!

SB: We will indeed. I would like to turn now to the CARS model and its predecessor, the four-move schema for research article introductions. Research attempting to validate the models has sometimes questioned their descriptive and predictive adequacy (e.g. Crookes, 1986). Some researchers (e.g. Taylor and Chen 1991) see the model as representing a basic pattern that allows for a range of permutations rather than as a description of the genre. There have also been attempts to extend the model. How do you view it today?

JS: Ah, I thought you would get to the CARS model sooner or later. Well what do I think about that after all these years? Certainly, it still seems to have pedagogical life and many instructors like teaching it because of its light-bulb-going-off effects in their students' minds. And certainly it seems to have been a boon for those looking for topics for masters theses; I haven't a clue worldwide how many thesis writers have over, say, the last 15 years, investigated and challenged the model with their own corpora of article introductions but it must be pushing up somewhere toward three figures. So it seems to have served —entirely coincidentally— some purpose of training people in this kind of discourse analysis.

As a result of these and of published studies, the model has been nibbled away at its edges, but its core has proved fairly robust. Originally, I now see that I didn't make it clear enough that I was basically talking about standard experimental research; indeed, we now know that the model would only lightly and limply characterise theoretical papers or those that make heavy use of computer modelling. Today's substantial literature on the topic also shows that that indicating-a-gap Move 2 is in reality an artifact of "big science," of competitive anglophone communities, of international research aspirations. We have learned that it doesn't apply on a local level in publications in minor research languages. However, the reasons for such differences turn out to be complex, shifting and hard to pin down.

As you may recollect from my Hong Kong presentation, my current thinking is that simple, fallible models work better for ESP students than complicated, prevaricating and fine-tuned ones. In fact, if I were doing this whole business again, I would no longer call it the CARS "model" because this suggests a somewhat imitative, follow-the-guidelines didactic procedure, while what I want to get at is something more like "well, here is a high-level strategic resource for you to play around with as you like." Recent research shows in fact that the model doesn't work well if you are attempting something new; the first papers in Chaos Theory and in AIDS research had to adopt different rhetorical strategies to get their points across.

You mention attempts to extend the model, like Lars Evenson's use of it to teach people how to do research itself, or to claim that it can be extended to many other genres. I have my doubts about this kind of endeavor. I have been having a look recently at introductions in art books devoted to individual artists (Degas, Picasso, etc.). All attempt some scholarly or intellectual novelty, but they do this in very different ways and in very different places. Nearly all also say that "their" artist has been under-appreciated in some way, or somehow misunderstood, and that this is now the time to set the record straight. I think novelty and hype (putting it a little unkindly) are pretty universal rhetorical tropes in scholarship, but the way they pattern does not always follow the rules of CARS.

Of course, I sometimes feel that I am too closely associated with that wretched model —"ah, Swales, the article-introduction man"— so that what I think are useful other contributions get shadowed, as it were. But the bandwagon goes on, sometimes with a novel twist. In a recent article in the *Journal of Business and Technical Communication* (a journal that your readers may not normally see), Danette Paul, Davida Charney and Aimee Kendall (2001) make some important arguments for saying that our studies of research writing have been focussed too much on origination, or on the texts themselves, and have been insufficiently concerned with their reception histories. As a result, they say that we are not in a good position to claim that good rhetoric (however defined) makes a contribution to scholarly and professional success. So I close this response with two quotes from this paper:

> Davida Charney and Aimee Kendall are currently preparing a study that will follow up Paul's observation that articles with introductions that closely follow the CARS model may win over more readers than may articles that do not.
>
> By comparing readers' reactions to introductions that violate the moves to those introductions within the bounds, we may be able to see the added value for scientists of appreciating the rhetoric of their community.

SB: I would like to ask something more about CARS at the risk of giving it more space than you would want it to have here. Perhaps because I was translating some ornithology papers when *Genre Analysis* was first published I´ve always found the ecological metaphor you used in the model a very useful way of thinking about the writing of a research paper. How did you come to use it?

JS: I have racked my brains but I cannot come up with a clear answer. I don't think it occurs in my 1983 and 1984 chapters on RA intros, so I guess I got the notion sometime in the second half of the 1980s. I note your reference to ornithology papers, and I would concomitantly like to think that it might have come out of my birdwatching activities. However, I doubt that this is actually so, although the idea is kind of nice. My best recollection is that I started thinking about claiming a topic as research territory and somehow proceeded from there.

SB: As you say, many teachers find that your models still have pedagogical life and that they like teaching them. That is certainly my own experience of them. That said, there has been quite a lot of debate about how far it is possible to teach genre, with some writers (e.g. Freedman and Medway) arguing that conventions are acquired through a process of socialisation and that explicit teaching somehow short circuits this process. In Australia too there has often been a good deal of opposition to the "genre theorists" from those who see teaching genre as somehow akin to imposing models on novice writers. Are genres acquired through a process of socialization and can genre conventions be taught?

JS: The second question is easier. Certainly "genre conventions" can be taught. For more than a hundred years, business correspondence courses have been successfully taught ("With regard to yours of the 16th ult. we would like to confirm dispatch of payment within 30 days of receipt of the goods"). Objects like the *APA Style Manual* do this too, as do a whole bunch of ESP textbooks. We can teach the conventions of the oral presentation, and indeed of the rationale behind it. That does not, of course, mean that products of such teaching are always or even regularly effective pieces of communication. In high school, I was taught to write Latin verse (in iambic pentameters as best I recollect), but I have no doubt that my efforts were execrable. And this leads me to another point. I believe that a valid distinction can be made between a stronger case for the explicit teaching of genres to non-native speakers of the target language —for reasons of cross-cultural confusion, reduced identification with a foreign language discourse (not quite the real "me"), and a greater necessary attention to the verbal service— than may be the case for native speakers. One of the strongest arguments against the teaching of genre comes from Aviva Freedman's "wearing suits to class" (Adam et al. 1996). However, I think her attitude might have been different if the MBA class had been composed of NNSs. We all know in a foreign language that we have trouble in seeing the big picture —"I understand all the sentences, but do I see the argument?). For all the reasons given above, NNSs like explicit genre teaching because they think it gives them a leg up. That doesn't mean to say, of course, that a focus on genre is all we should do, or that there isn't more to constructing an effective discourse than genre per se.

I waver/fudge/hedge somewhat on the socialization issue and on the concomitant need to be a member of a community of practice or whatever. I know there is a strong push toward this, especially in the US, perhaps partly because it attunes nicely with US views of the teacher as facilitator, friend and mentor. However, I do think there are some ironies here, especially as there has been a parallel development toward distance education. I also think that necessary socialization rather diminishes the efforts of isolated and successful individuals. Michigan's Morley scholar scheme brings together ESP folks who are doing outstanding work in "unlikely places," Lviv, Tunis, Mendoza, Sienna and the like. For nearly twenty years I taught in such places as Bari, Falun, Benghazi, Tripoli (Libya) and Khartoum. Hardly, the Oxfords, Cambridges or Stanfords of this world where you are supposed to be closely mentored and socialized into your discipline's generic practices. So I think we should be cautious about this. I think we should give a place for self-education and self-learning as well.

That said, I do think we can also draw a distinction between business and academic worlds, with professional ones, doctors, lawyers, somewhere in between. In business, socialization, becoming a member of the team, is clearly very important, although of course there remain strong injunctions for executives and the like "to think outside of the box"! In the academic world, we aspire to be named authors while this is rare in corporate discourse. This simple fact, plus the paradoxical expectation to be at the same time "complicitous" of our discipline and yet "contestatory" about its deficiencies, leads to greater personality factors.

SB: Yes, and the expectation is even harder to manage rhetorically for members of smaller discourse communities who seek to publish their work in international journals. Is there a place for diversity in discourse conventions or will the hegemony of, "big science" publishing in English ultimately mean that we will all be compelled to handle these personality factors in the same way, no matter where we come from?

JS: Another good and difficult question, and one that looms large in my occasional attempts to help my ex-colleagues from the Aarhus Business School in Denmark move their publications onto a broader international stage (such as Askehave & Swales 2001). We now know several things, as indeed confirmed by your own doctoral research (Burgess 1997). Smaller discourse communities, for a whole slew of reasons, can opt for a different audience design —with marked rhetorical effects— to those needed internationally. This is only what we might expect as linguists, especially those with Hallidayan leanings.

We also know that a number of forces have been increasing the pressure to publish in Anglophone international journals, particularly those that are included in the ISI datebases. Part of this has come from changes in editorial policy which have abandoned national languages, most obviously perhaps for science journals based in Germany, and part has come from new structures as to how to assess research productivity and quality.

So two subsidiary questions arise:

1. Is there room for alternative rhetorics —in so far as these are reflective of "national cultures" rather than small discourse communities.
2. Is the Anglicization of vehicles of publication and of rhetorical design reversible or resistible?

As you doubtless know, one of the main advocates of a larger rhetorical space has been Anna Mauranen (Mauranen 1993) who argues, as a Finnish scholar, that if she can no longer effectively publish in Finnish does that mean that she can no longer write in a Finnish way in English? This is a rainforest argument *au fond*. Will an anglophone "monopoly of the mind" stifle creativity and various kinds of alternative thinking? There are signs, I believe, in some areas of a greater flexibility here. There is particular concern in medical research that NNS are not getting as published as they should. This January, my colleague Chris Feak is teaming up with a distinguished emeritus professor of surgery to run workshops on this issue at a major US medical conference. Flowerdew, in a recent paper (Flowerdew 2000), suggests that in our field editors are broadly receptive to NNS contributors from wherever.

On the second question, my recent investigations suggest that the picture is pretty mixed, and that the answer lies more in discipline than in language. That local language use can survive, despite pressures to the contrary, when territory is important. Indeed, one of the most interesting papers on this comes from two researchers at the Spanish Research Council: J. Rey-Rocha & M.J. Martin-Sempere (1999).

As your readers will know, since 1989, the Spanish Administration has followed the international trend of giving top priority to publishing in ISI international journals, and Rey-Rocha and Martín-Sempere go on to note that none of the Spanish earth science journals fall into this elite category. Yet, they note, from 1990-1994, there was comparatively little movement toward the big journals by Spanish geologists and geographers. To cut a long story short, here is part of their conclusion:

> Spanish evaluation procedures are doing a disservice to Spanish journals in the earth sciences, jeopardising the national dissemination of results of some research works that either respond to some issues of a manifest national, regional or local interest, or that arise from the need to solve some specific problems of local interest. Although we cannot forget that science is an eminently international activity, whose results have to be necessarily disseminated amongst the international community, we can neither miss the point of its national utility (215).

Great quote, eh! Viva España.

And as a Parthian shot at this topic, I would observe that there is very weak contact between applied linguists concerned with academic discourse and information scientists in such matters as research language policy and citational practice (something I am in fact about to work on.) How many of your readers, for example, are aware of this excellent paper that I have quoted at some length?

SB: I suspect very few. I certainly didn't know about it.

Throughout our interview you have mentioned a number of possible directions that research into professional and academic discourse might take viz., further examination of the differing rhetorics of smaller, local discourse communities, looking into reception and not just production of genre realizations and more collaborative research conducted by applied linguists and information scientists. You also implied that there have perhaps been enough validations or challenges to the CARS model. I wonder if we could close our interview by talking a little about the future. What questions, in your view, should the next generation of researchers seek to answer?

JS: I am not really sure that a person who is now 63 is in the best position to peer into a crystal ball and come up with a likely vision of what is to come. So let's start with some of the Young Turks.

Dacia Dressen defended her dissertation here a couple of days ago. This was a complex account of how French geologists "recontextualize" their field research in their publications (now increasingly in English). The points to note here are I think the following: a) the use of Bourdieu as a theoretical framing; b) the focus on the "unsexy" parts of research articles (in contrast to all the recent attention on social interaction, metadiscourse, evaluation, reader management etc.); a strong interest in what is "not said," on textual silence, and how best genre theory can deal with this; c) and a methodology that deals with the macro and the micro (e.g. the history of French geology, and the field notes of a PhD student in NE Madagascar).

Fathi Helal is writing his dissertation in Tunis on the first AIDS papers, and will show how "discovery" papers are different in their introductions to the stream of later ones. He has been in contact with some of the major figures and so can construct an interesting "reader response" account. More broadly, we are beginning to see how much contextual circumstances shape the resulting textual forms. And this can be connected with efforts, including yours, to try and understand WHY (not how) certain sets of discourses are intriguingly similar but also challenging different to other sets of discourses.

A group of us associated with the Michigan Corpus of Academic Speech (MICASE) are struggling with the foundational question of whether academic/research talk is "more like" everyday conversation or more like academic prose. One hot topic among this group is why metaphors seem to be pretty rare in research talk (unlike political or economic talk), and what this might mean or not mean for our attempts to help non-native speakers. On the other hand, my study of dissertation defenses suggests that the role of humor in research talk as a kind of lubrication of sticky parts has been under-estimated. And this is a hugely important issue for junior researchers who are not broadly proficient in English. As one of my senior

Asian doctoral students said a couple of years ago when attending my dissertation writing class, "I understand everything except when everybody laughs; then I am an alien again."

And since I have moved on to myself, the book I am currently writing as a "successor" to Genre Analysis, spends quite a lot of time trying to link university speech and writing in various kinds of networks, chains and sets. As Shakespeare might have said, "genres come not as single spies but in battalions," and this we need to take into account. Everything is intertextual and interdiscursive even unto the, at this very moment, recognition that the Shakespeare quote will now likely make its way into other writings, perhaps as an epigraph.

Finally, we still need better accounts of citation, sources, paraphrase and all the other signatures of how we weave ourselves into the webs and warfs of the thoughts and writings of others. So let me close with a little story I came across the other day, which still continues to amuse me. Famous songster, Harry Bellafonte, was apparently a keen autodidact as a young man. So he goes up to the circulation desk at the public library in Kingston, Jamaica (or wherever) and says to the librarian, "Give me everything you've got by ibid." We need to know more about how "ibid" plays into our academic lives. One thing is sure, there's still lots to do, and no shortage of good projects for young entrants in our field to cut their teeth on.

SB: Thank you for agreeing to be interviewed and for providing us with so many leads to follow up.

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