PETER MATTHEWS

MATTHEWS, Peter Hugoe, FBA 1985; *b.* 1934; *m.* 1984 Lucienne Marie Jeanne Schleich; *Education:* Montpellier School, Paignton; Clifton College; St John's College, Cambridge, BA 1957, MA 1960, LittD 1988; *Career:* Lecturer, University College of North Wales, 1961–5; Lecturer, University of Reading 1965–9, Reader 1969–75, Professor 1975–80; Professor of Linguistics, University of Cambridge, 1980–2001 (Head of Department 1980–96); President of the Philological Society 1992–6; Assistant, later Joint, Editor *Journal of Linguistics* 1970–9; Member Editorial Board Cambridge Studies in Linguistics, etc., since 1980; Hon. Member Linguistic Society of America since 1994. *Major Publications: Inflectional Morphology* 1972; *Morphology* 1974 [2nd ed. 1991]; *Generative Grammar and Linguistic Competence* 1979; *Syntax* 1981; *Grammatical Theory in the United States from Bloomfield to Chomsky* 1993; *The Concise Oxford Dictionary of Linguistics* 1997; *A Short History of Structural Linguistics* 2001.

* * *

As a schoolboy I was hopeless with figures, a disability that I retain; but good at Ancient Greek and Latin. My main interests were in literature, and I came to Cambridge in 1954, after the then obligatory national service, intending to read classics for the first two years and English in my last. If I had followed that plan I might have ended up as a traditional literary scholar, grumbling, as I retired as early as possible, about the latest fashionable obfuscation from Paris or elsewhere.

Two things saved me. One was the character, as it then was, of the Cambridge English Faculty. Its dominant member was a critic, F. R. Leavis, whose biases of taste were decisive and off-putting. The other was the election of Sidney Allen to the Professorship of Comparative Philology. I attended his course on general linguistics in my second year, out of pure curiosity. In those days it was considered beneath the dignity of the more intelligent students in arts subjects to be seen too frequently at lectures that related to the examinations they were taking. Therefore there was ample leisure to explore ideas beyond them. I recall no more than passing details of what Allen said. But I do remember that I was fascinated, and had to learn more.

I will not pretend that, before then, I had shown much interest in language as such. I was bowled over by the language of Greek literature, Homer and the tragedians especially. Another reason for spurning the English Faculty was the confidence with which its students spouted rubbish about plays that they knew only in translations by Gilbert Murray. I have also kept an essay on formal patterning in Virgil's *Eclogues*, which I would have liked to rework later in a Festschrift for Roman Jakobson, who once found time to talk to me, as a research student, when he could easily have pleaded more important engagements. But my wider interests were in poetry in other languages, especially in Italian. I frankly do not know why, in my final year, I decided to do nothing but linguistics. Caution, and my tutor, counselled choosing philological options in the Classical Faculty, which would have obliged me to take further papers in philosophy, literature and history. But I found that, in the Faculty of Modern and Medieval Languages, I could do exactly what I wanted. This was to follow Latin and Italian right through, from the beginning to the modern period, in relation first to Indo-European and then to the other Romance languages. I did not understand at first how good a general introduction to philology that would be. But everything was there, from speculative reconstruction to close study of texts. I remember that the works that most excited me were ones that dealt in elegant abstractions: Benveniste on roots and suffixes in Indo-European (1935), or Martinet on change in phonological systems (especially Martinet 1951–2). I also took a paper on general linguistics, for which no supervision on the Cambridge pattern could be organized. I believe it pulled me down badly.

I was not then attracted by a career in universities. I therefore worked for two years for an insurance company, before deciding that, since I was doing so much academic reading in my spare time, it would make sense to come back to Cambridge as a research student. As an undergraduate I had been guided in Romance philology by Joe Cremona, and the proposal I put to him was to investigate the dating of Greek loan words into Latin and Romance, in relation to changes in the phonology of both languages. But he referred me to Allen, sensing doubtless that, despite my leanings as an undergraduate, I might do better as a general linguist. I have rarely received advice so crucial and perceptive. I would not have shone as a specialist in Romance philology, and I can only hope that, when I have myself guided students, I have occasionally shown a similar insight into their prospects.

Allen's advice was that I should get a copy of Chomsky's *Syntactic Structures* (1957), published two years earlier. As an undergraduate I had read Saussure's *Cours*, Bloomfield's *Language* and the French translation of Trubetzkoy's *Grundzüge*. That was most of what I knew about linguistics in general; and, in particular, I read nothing by Bloomfield's followers until, in the next phase of my education, I made a detailed study, lasting, as I recall, a week in which I did very little else, of Harris's *Methods* (1951). I therefore came to *Syntactic Structures* quite cold. It was an eye-opener, and, for a reader who responded to it before 1960, to precisely the things that later commentaries, including Chomsky's own, have tended to downplay. The excitement lay in the conception of a grammar as a formal system, in which the sentences of a given language, in a sense of 'sentence' and 'language' that went back to antiquity, were defined precisely, for the first time, by a set of rules that were as revealing as they were simple. Of the nature or development of what was later called a speaker's 'competence' there was less, at that time, than could be found in passing remarks by Harris or Hockett. I have since been accused of failing to see in *Syntactic Structures* things that many readers have convinced themselves are there. But they have been told what to look for; and tend to overlook what was actually said.

My PhD was never finished; and, although I have not consciously thrown away drafts, I no longer seem to have them. It would have dealt with prepositions in Italian, in a transformational framework that perhaps owed more to Harris (1957) than to Chomsky's theory as it was in fact unfolding. The method was to uncover patterns in the distribution of lexical items, in Harris's term of 'co-occurrence', which explicated differences of meaning; and that, basically, was as far as I remember getting. I do not think, for example, that I would have explored ideas like those of Fillmore in the later 1960s; though I did feel, after reading his first paper on this topic (1966), that he might be pulling what were once my chestnuts out of the fire. But I pulled none out myself, then or later. In 1961 I had my first job, as a Lecturer in a new department headed by Frank Palmer, at the University College of North Wales. At that time I knew no phonetics, and for my first term I was generously seconded to the School of Oriental and African Studies, for this ignorance to be remedied. That interrupted my research completely; and, when I returned to it, I realized that I would not want to publish my thesis. When I sought advice I was told, correctly in the circumstances of the early 1960s, that the doctorate itself was not essential. I therefore left it and pursued another current interest, in the inflectional morphology of Latin. This had started when, by chance, I had acquired the first three volumes of the *Grammatici Latini.* I remember sitting in the garden, idly leafing though Charisius, and suddenly imagining how a generativist might do it better.

These interests are reflected later in my first substantial group of publications, in 1965–6. Meanwhile, however, I had a brief flirtation with phonetics, through my term in SOAS, and another, in a year spent in America in 1963–4, with computational linguistics. By 1965 I had turned 30, and it was not until then that I began to settle down.

The first flirtation could easily have gone further. In SOAS at least, phonetics in Britain was by then emerging from what may, without disparagement, be called its Daniel Jones phase. Its instruments were mostly primitive; but, with a face mask and a larynx microphone, it was easy to see, for instance, that the consonants in English that were called 'voiced' often had no voicing whatever. Wave forms were at that time registered with a kymograph, with all the mess of smoke and varnish. But, in its own room well away from the pollution, the School also had an early Sonagraph. I was introduced to it by Jack Carnochan, and I have always suspected that we alone were using it. I remember few instruments from which so much could be learned so quickly. I was not destined, however, to work seriously in instrumental phonetics. A short while afterwards I read Fant's *Acoustic Theory of Speech Production* (1960), and felt strongly that this was the way the subject should be going. But, alas, the maths was quite beyond me.

To loosen my vocal organs, I was sent to classes on the phonetics of a variety of languages: Hausa, with Carnochan; Egyptian Arabic, with Terry Mitchell; Cambodian, with Eugénie Henderson; Malayalam, with Elizabeth Whitley. 'Phonetics' naturally embraced phonology, and I learned from their example not to make dogmatic divisions in this field. I was also given conventional training in the cardinal vowels and such-like, in the Jones tradition. But I was no good at it. At the end of the term N. C. Scott, who had struggled to teach me, wrote to Palmer saying that, though I tried hard, I would never make a phonetician. This letter later fell into my hands and was useful to me. Thirty or more years ago, most phoneticians used to insist on students of linguistics spending unbelievable hours in practical classes, for production and ear-training, in small groups. That was how they had themselves been taught, on diploma courses in, for example, Jones's old department at University College London. They were eventually forced to be more reasonable. But while that restrictive practice lasted any competent colleague, on the principle of equality of teaching hours, might be pressed to help them. I was very grateful for such clear assurance that I was not competent.

My second flirtation began at the RAND Corporation in Santa Monica, on a summer course attended by a number of linguists more distinguished than me, organized by David Hays. From there I went to Indiana University, as an Assistant to Fred Householder on a project funded for general information retrieval. Most readers of these reminiscences will not have seen computers of the 1960s, or set programmers to work in an assembly language or worse. But the dreams of early computational linguists were defeated, above all, by the limitations of technology. I think I realized this in Bloomington. I also realized that I would learn little about language by continuing in this field. Many people in the 1960s were seduced by the original fallacy of artificial intelligence: that, in programming a computer to do something, we would gain an insight into how our own brains handled similar problems. But if I ever thought this I was quickly disillusioned. For the systems then envisaged were constrained both by the difficulty of locating faults in programs, and by the limits of our hardware: in particular, the capacity of central processing units. To work efficiently a system had to be articulated as a sequence of sub-systems: first, for example, parse the syntax of a sentence; then 'parse' its 'semantics'. At each stage, rules for a specific language were one thing; general operating programs, to which such rules were input, were another. That way of thinking somehow, alas, passed into psycholinguistics. But there was no reason to suppose that human minds are similar.

From Bloomington, then, I returned to syntax and morphology. I also returned to teaching, which was definitely not my strong point. At Bangor I was let loose mainly on postgraduate students. But in 1965 Palmer was appointed to a new Professorship at Reading, and three lecturers, including me, moved with him. These were the years that followed the report of the Robbins committee (1963), when the university system was expanding rapidly. It was therefore possible for whole departments to be set up instantly in that way. Our brief at Reading was to develop linguistics in joint honours courses: with French, with German, with English literature, and so on. But we saw that, with little further effort, we could also offer single honours. This option proved more popular; and we were soon teaching both undergraduate and MA courses full time.

Moving to Reading, with Frank Palmer, was a vital stroke of good luck. When I was appointed three candidates were applying for two Lectureships; but, after the interviews, there were posts for all of us. I was told that the Vice-Chancellor had said of me, in particular, that I was a luxury the university ought to be able to afford. I hope that, in time, I was something more than that. But the faith that others had in me was still supported by little visible achievement.

I do not remember very much about the meetings at which we planned either of our courses. Both at Reading and in earlier years at Bangor our postgraduate class included many people who taught English as a foreign language; often, in the beginning, from South America. They were delightful students, and the British Council, who were funding most of them, did not waste their money. But I tried, for a while, to follow ideas in ‘applied linguistics’ and did not think they were a credit to our discipline. Too often in the 1960s, its practitioners seemed to believe that they could ‘apply’, quite literally, whatever linguistic theory was in fashion, and good practical results would follow. My conclusion was that I should simply teach linguistics, and leave the teachers themselves to find the ‘applications’, if there were any. But, although my memory of these developments is dim, I think my conscience did rest easier when, within a few years of our arrival in Reading, the Department had the resources to run separate courses for such students.

We were on safer ground with undergraduates, though, looking back, it is remarkable how few textbooks were available. As a student I had rarely opened such things; it was something else, like reading poetry in translation or attending bread-and-butter lectures, that was not done. But I seem to have accepted readily that our pupils could learn from them, and that the ones which were available in the 1960s, which at first were mostly American, covered the right topics either in the right way or in ways that were at least a suitable foundation for our own lectures. In our own country there was Robins’s *Introductory Survey* (1964), which we must have recommended as a starter; and, within a few years, we all worked successfully with Lyons's *Introduction to Theoretical Linguistics* (1968). Both were international in their outlook, and fitted admirably with our own view of the subject. In particular, we were determined not to teach it in a form that was nationalistically 'British'. Unlike so many of our colleagues in France, having lost hegemony to linguists in the United States, we did not turn parochial.

This is a delicate matter, and I trust that there are contributors to this volume who can represent the opposite view. But there were those who gave the impression, at least, that they regarded 'British linguistics' as distinctive and superior; and we did well, I believe, to resist them.

Firth, it will be recalled, had died in 1960. I did not meet him; though I had a keen sense, when I was attached to his department in the following autumn, that his ghost still ruled it. Certainly Bazell, who had succeeded him, and was an inspiration to anyone whose mind was up to it, was not the kind of scholar who aspires to found schools or lead those among us who do not themselves know where they are going. Into that role as putative leader stepped, or tried to step, Michael Halliday. His theories were described as 'Neo-Firthian', and were represented, rightly or wrongly, as developing ideas of which Firth would have approved. As such they were promoted vigorously, not least, early in the 1960s, through the new Linguistics Association of Great Britain. I knew little of the beginnings of the Association; there were rumours that it was at first more like a cell of the Communist Party than a normal learned society. But when I attended my first meeting, in London in 1961, it seemed riddled with Halliday's followers and sympathizers. In the middle of the decade, he gave at least one lecture abroad in which he seemed to be suggesting that his own views were the dominant tendency in British linguistics. This naturally annoyed some other British linguists. In America I had come to know John Lyons and Jimmy Thorne, both near contemporaries; and, on returning to this country, two of us at least believed that we were a triumvirate whose mission was to open colleagues' minds to new, especially to Chomsky's early, ideas. My first reaction at this point was to concoct a polemic for the second volume of the *Journal of Linguistics* (1966), to advertise, as plainly as I could, that Neo-Firthianism did not reign unchallenged. Its topic was less important, my main purpose being to object to, in the wording of a passage I was persuaded to delete, 'the large "made in Britain" label'.

I do not think that I accomplished that much by this exercise. It was certainly far less serious, and far less significant in my own intellectual history, than a long and critical review of Chomsky's *Aspects of the Theory of Syntax* (1965) which I contributed to the next volume (1967). This took me two months, and my trust in Chomsky's methods of academic (and, for that matter, political) argument has never been restored. But the skirmish with Halliday was one incident in an academic battle that, like other battles, might have gone the other way. That it did not was due much more to John Lyons. I hesitate to describe him as the Augustus of our triumvirate, since his methods were not Octavian's. But he played a central role as Editor, for its first five years, of the *Journal of Linguistics*. This was established while I was out of the country, and I know little of the details of what went on. But it seemed miraculous that a journal that was to be published for the Linguistics Association, whose direction I had earlier so much distrusted, should be edited independently by someone who I so much admired. It was certainly the very best thing for the future of the subject. There was no whiff of parochialism, and no doctrinal bias either, in a periodical whose circulation shot up rapidly.

It was also a miracle for me personally, since, at the very beginning, I was one of its main contributors. I was then still pretty well unknown, having published only a review article (1961), which Allen had encouraged me, as a research student, to send to *Archivum Linguisticum*. But in 1964 I finally began to scribble to some purpose, and I do not know how else my work could have appeared, or have been noticed, so fast.

By then the Linguistics Association had itself evolved; and, for some years, it was very useful to me both to attend its meetings and to read papers to them. Most sessions were then plenary, and one could easily speak for 40 or 50 minutes. It was therefore possible to address the Association on broad issues; not on clitic movement in Ruritanian, or the theoretical implications of consonant harmony in Glubbdubdrib. From 1967 onwards there are records of its meetings on the inside cover of the *Journal of Linguistics*; and they are, to me, a sad reminder of how much the nature of such conferences has changed. The last time I seem to have spoken was in the autumn of 1972, on the topic of 'How seriously has transformational grammar failed in its objectives?' I am not sure when I last attended a meeting; but I remember clearly one in the later 1970s, when the officers, in pushing through a new system for selecting speakers, also deigned to explain to us the kind of paper they preferred. Glubbdubdrib would have been perfect; and, since such things are better read than heard, I have had little to do with the Association since. But I look back on the meetings I attended with true pleasure. I suppose we are all seduced by rosy memories. But mine is of a discipline that was still unified, in which everyone of consequence was interested in what everyone else was doing, and discussed it freely. That is also how I remember meetings of the Linguistic Association of America, also plenary, when I was based in Bloomington. The controversies were the liveliest I have ever heard, but on matters that everyone agreed were vital.

By the mid-1970s specialists were laying proprietary hands on more and more of the subject. I remember, for example, a discussion in the old style in our department at Reading, when we all pitched into Roger Brown's interpretation of children's language (Brown 1973). I reviewed his book in the *Journal of Linguistics* (1975), of which I was then an editor, as an important contribution to our subject in general. It did not occur to me that, since I was not a professional ‘developmental psycholinguist’, I might be incompetent. But a colleague’s intervention was rejected by the board of editors of a specialist journal, with the comment that the author was not in the field. I believe I was right to be shocked. But the evaluation of such books is now routinely the prerogative of other ‘experts’, who do not usually have the gift to see their field as those outside might see it.

I was at Reading until 1980, when I returned to Cambridge as the university’s first Professor of Linguistics. Before then I had never run a department, and, apart from meetings about teaching and eventual service on the Council of the Philological Society, I had spent perhaps, on average, one hour a year on committees. Reading promoted me to a Readership and, in 1975, to a personal Professorship. But, even before then, I was determined not to seek an established chair until I was ready for it. I could admittedly afford this attitude: I was as yet unmarried, and often far too busy to spend what I earned. But in the 1960s the establishment in many subjects had expanded too fast. Good people were being scattered across universities too thinly, and too many promoted beyond their ability or experience. It was important, I thought, that linguistics should not follow that example. In 1976 a new Chair was established in Oxford, and was the first I considered seriously. Unfortunately, for the first time in my career, a book I was engaged to write was causing me real difficulty. It was finally published five years later (1981). But for the moment I had lost my confidence; and, recollecting in tranquillity, I think that I was not emotionally in a state to take on what would certainly have been a hard task. The next year I was invited by Bob Uhlenbeck to spend a year at NIAS, the research institute in Holland for whose foundation he had been mainly responsible. While I was there I got my book straight; and, when the Chair in Cambridge was proposed, I was very happy for my name to be considered. I had been away for two decades, apart from one year when, in another interval of unpaid leave, King’s College had most generously elected me to a Fellowship. There were now two decades more before I retired.

The University of Cambridge is one that new professors come to love gradually. Their power is formally minimal, and power in general is not concentrated in a single hierarchy. Changes can take time, especially if the interests of colleges are affected. A Professor will also have a college Fellowship, and this helps one to understand the way the collegiate structure works in practice. I have never regretted, in particular, the four years that I spent in the early 1980s on the council of my own college. But one can easily get impatient.

One crucial factor is that colleges are responsible for admitting undergraduates. At Reading, there had been a quota for linguistics, as for other honours courses, and we met it as we saw fit. But in Cambridge applications are to individual colleges, and their quotas relate to broader faculties. Since the Department of Linguistics is part of the Faculty of Modern and Medieval Languages, any college might admit sixth-formers interested in the subject under that head. But whether any do so will be up to them entirely. New undergraduates are then faced with options that include, for example, excellent courses on the 'structure and varieties' of French, German and other languages. In later years, they can choose freely among papers on the histories of these languages, on comparative Romance, Germanic or Slavonic, and in branches of general linguistics. But they will be advised, again in colleges, by Directors of Studies who may not be friendly to such subjects. A few try very hard to force their pupils to take literary papers, sometimes, in the first year, by concealing from them that alternatives exist. This is, alas, the flip-side of a system that has many educational advantages. But, despite it, there have been years when, for example, the paper on Dante has had fewer takers than our option in Phonetics. Much as I love *The Divine Comedy*, I note this with satisfaction.

At the end of the 1970s the Department of Linguistics had three lecturers, and included another member of staff, a technical officer and three technicians, responsible for what is now the university's Language Centre. Palmer had had a 'language laboratory' in his first department at Bangor, and in the 1960s, when the one in Cambridge was planned, this arrangement may have seemed to make administrative sense. But by 1980 it did not and, though it was to take the best part of 10 years to hive off the centre, it was one thing I was set on doing. Beyond that, I had no ambition other than to foster the development of linguistics in the best way that I could. I was aware that I should not consider 'linguistics' as established only in my Department. Within that I could have immediate influence. But in Cambridge the majority of linguists are outside it. Some are in institutions, like the Computing Laboratory, quite beyond my orbit. Others are in a range of language faculties. I have been able to meddle in these at best indirectly.

The Department itself soon had three further teaching officers, and by stages has come to offer undergraduates more than twice as many options as it used to. But the expansion had to be, in part, opportunistic. In the beginning the university gave us one more Lectureship, which I was keen to fill with someone who could teach historical linguistics. We then got two more posts through special schemes that ran in the early 1980s. One in the history of grammar was a so-called 'New Blood' Lectureship, which I proposed in the knowledge that Vivien Law would be a candidate. I had inherited a paper in this field with no one there to teach it; and, although I coped at first as well as I was able, I would have suppressed it if, Heaven help me, I had had to lecture seriously on the Middle Ages. A little later, Francis Nolan put up a proposal under a scheme for 'Information Technology', which expanded our coverage of phonetics. I cannot say that, on a rational calculation of new needs, either a second phonetician or a historian of linguistics might have been our first priority. But universities were entering on a period of cut-backs, the ebb in part of the tide that had carried my generation forward, and if we had not seized opportunities like these we would without doubt have got nothing. Elsewhere in the faculty lectureships were already being lost.

The rest is a chronicle of this development and that development, and I think posterity should be spared it. But, in general, I have had two main anxieties. One was simply that I should have a successor. My Professorship was originally for one tenure, and the bitterest failure would have been if, when I retired, the university had let it go. I am therefore delighted that, despite continued tightening of belts, it is now established, with my Faculty's enthusiastic backing, like any other.

Another concern was that the university should not lose posts in the linguistics of individual languages. This has now become so easy: so-and-so retires and, since belts do have to be tightened, what is simpler than to drop a subject which perhaps, through so-and-so's own policy, has never had that many students? Some literary posts are similarly vulnerable. Why keep one in medieval Latin when more undergraduates are interested in film studies or French literature since 1900? So much depends on the people who now teach such subjects, and I am very glad that our philological establishment, for which, of course, I have never been directly responsible, is in general sounder than it was two decades ago. The Oriental Faculty went through a period in which it seemed to be attempting suicide by a thousand cuts. It was at that time that we lost, in particular, the Chair in Sanskrit. But the Classics Faculty retained one that is now effectively in Greek and Latin linguistics, although, when Allen retired in the early 1980s, we had to resist an argument that my own had made it redundant. Under its wing the study of Indo-European flourishes. In my own faculty, Romance linguistics has always done so; and, after interregna whose varied causes I will not go into, I also have good colleagues in Germanic and Slavonic.

The exception is the English Faculty. It has within it a Department of Anglo-Saxon, Norse and Celtic, which has never been a cause of despair. But when I arrived the English Tripos had a paper in the History of the English Language, which, for reasons I again prefer to forget, no prudent candidate would take. This was later taught by Sylvia Adamson and, since she taught well, it became so popular that two lecturers were soon needed. But she left for Manchester in 1999 and, as I write, the subject has been put to sleep. It is to me quite scandalous that the history of our language can no longer be studied in a leading institution. But in the English Faculty, as in mine, there are people who think that only the study of literature is central. In mine they are most easily found by turning over stones in colleges. There they are more prominent, and this does not seem to be scandalous at all.

In the 1990s I was to serve for four years as the President of the Philological Society, and for three I also chaired what is now the Linguistics and Philology Section of the British Academy. I remember writing at least two separate letters of protest, under these hats, when the director of SOAS proposed that their Department of Phonetics and Linguistics, which I had known in its great days, should disappear. It was a comfort to reflect that that at least could never have happened in Cambridge in that way. When I retired from these roles I was also resolved that someone else should head my own Department. I had done it, leave apart, for 16 years; and, in the beginning, with such friendly and co-operative colleagues in my faculty, that job and a Professor's were compatible. But by the end of the 1980s governments, through their cat's-paws, were increasingly messing up the work of universities. This is a broader story, and someone some day may be able to tell it without bitterness. But one form of interference even affected my development as a scholar. I had decided that it did not matter, from now on, how many publications were on my c.v. What other post, for example, was I likely to apply for? I therefore started to explore a long-term project, for an encyclopedic study of grammatical categories. But, by the time I was ready to commit myself, our universities were about to fall foul of short-term 'research assessments'. It seemed that, if my department was not to suffer, I would need a regular supply of '[ˈriːsəːtʃ]' to submit. I therefore cast around for other topics, and, although it is still 30 years since I submitted papers blind to refereed journals, my publications have been bittier and more varied than I had hoped. I was determined not to write in fields in which my contributions might seem negative: my inaugural lecture (1982) should, I decided, be my last piece in that vein. As regards the theory of syntax, in particular, I have therefore found peace in internal exile. I had meanwhile promised Giulio Lepschy, an old colleague in the Italian department at Reading, to contribute on the history of linguistics in the Greek and Roman periods, for the survey he was editing. This was finished, in its English version, in 1986 and, by the time it was published (1990), I had also been persuaded to do a new edition of my introductory *Morphology* (1991). Such things were perfectly respectable; but it was only at that point that I once more focused on new projects of my own. My wife, in particular, was then rightly pressing me to do so.

My underlying difficulty, for many years, has been in trying to keep up with my subject. By 'my subject' I mean simply linguistics; and in the 1960s I read critically, in detail, most of what appeared. But by the early 1970s matters were beginning to get out of hand. The photocopier was a fairly new invention, and was widely abused, much as the internet can be abused now, as a means of putting trivial or half-baked papers into circulation. This did not affect my own work in morphology, since the culprits were not in that field. But I still had to decide if their stuff was worth following. I thought not; and, before long, most of it was seen to have got nowhere. Since then I have become increasingly suspicious of whatever work is most in fashion. Too much of what is published is by pseudo-theorists chasing their own tails, and overshadows what is of true value. I suppose that all scholars have their own solution to this problem. Mine has been, in part, to do my own work in fields that are not fashionable. In the past 10 years, for example, I have written two books on the history of ideas in twentieth-century linguistics, which does not attract researchers in droves. An earlier exception was my work on syntax, and I found it simpler, in that case, to develop my own answers to the problems, and review what others had written afterwards. When I was younger, at least, such methods ensured that I did not get bogged down in my speciality, with no leisure to study and think beyond it.

My larger strategy has simply been to read, for preference, the things that others might be tempted to neglect. I used when young to follow a pack of beagles in South Devon, and one day, in talking to the kennelman, my brother and I asked him why he often set out at an angle of 90 degrees from the rest of us. He explained that hares run in circles. If he walked off to the left and the hare turned to the right, no one would notice he was not around. But if it bore to the left, as he no doubt thought it would, people like us would be amazed how well he understood the lay of the country. I like to suppose that when I worked on morphology I was subconsciously following that lesson. When I began some people even doubted that there was such a topic; but, in the end, a sufficient hue and cry overtook me. I have also followed it consciously in branches of linguistics on which I have never written. Sometimes I have, indeed, appeared to some of my friends to understand the country better than I do. At other times I have instead gone left when the field has gone right. But academic work is definitely more fun if one does not run with the hounds.

Was that not, in brief, why many of us came into linguistics anyway?

REFERENCES

BENVENISTE, EMILE, 1935. *Origines de la formation des noms en indo-européen*, Paris: Maisonneuve.

BROWN, ROGER, 1973. *A First Language: The Early Stages*, Cambridge, MA: Harvard University Press.

CHOMSKY, NOAM, 1957. *Syntactic Structures*, The Hague: Mouton.

CHOMSKY, NOAM, 1965. *Aspects of the Theory of Syntax*, Cambridge, MA: MIT Press.

FANT, GUNNAR, 1960. *Acoustic Theory of Speech Production*, The Hague: Mouton.

FILLMORE, CHARLES J., 1966. 'A proposal concerning English prepositions', in Francis P. Dinneen (ed.), *Report of the Seventeenth Annual Round Table Meeting on Linguistics and Language Studies*, Washington, DC: Georgetown University Press, 19–33.

HARRIS, ZELLIG S., 1951. *Methods in Structural Linguistics*, Chicago: University of Chicago Press.

HARRIS, ZELLIG S., 1957. 'Co-occurrence and transformation in linguistic structure', *Language* 33, 283–340.

LYONS, JOHN, 1968. *Introduction to Theoretical Linguistics*, Cambridge: Cambridge University Press.

MARTINET, ANDRÉ, 1951–2. 'The unvoicing of Old Spanish sibilants', *Romance Philology* 5, 133–56.

MATTHEWS, PETER, 1961. 'Transformational grammar', *Archivum Linguisticum* 13, 196–209.

Matthews, Peter, 1966. 'The concept of rank in Neo-Firthian grammar', *Journal of Linguistics* 2, 101–10.

Matthews, Peter, 1967. 'Review of Chomsky, 1965', *Journal of Linguistics* 3, 119–52.

Matthews, Peter, 1975. 'Review of Brown, 1973', *Journal of Linguistics* 11, 322–43.

Matthews, Peter, 1981. *Syntax*, Cambridge: Cambridge University Press.

Matthews, Peter, 1982. *Do Languages Obey General Laws?*, inaugural lecture, Cambridge: Cambridge University Press.

Matthews, Peter, 1990. 'La linguistica greco-latina', in Giulio Lepschy (ed.), *Storia della linguistica*, vol. 1, Bologna: Il Mulino, 187–310.

Matthews, Peter, 1991. *Morphology*, 2nd ed., Cambridge: Cambridge University Press.

Robbins et al., 1963. *Higher Education: Report of the Committee appointed by the Prime Minister under the Chairmanship of Lord Robbins*, London: HMSO.

Robins, R. H., 1964. *General Linguistics: An Introductory Survey*, London: Longman.