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DIACHRONY AND SYNCHRONY IN TWENTIETH-CENTURY LEXICAL SEMANTICS: OLD WINE IN NEW BOTTLES?[1]

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1. Introduction: history, chronology and numerology

It was a great privilege to have been invited to be one of the speakers at the Symposium celebrating the 150th anniversary of the Philological Society. My own full membership of the Society went back no further than thirty-five years and my association with it had been neither as long nor as intimate as that of Professor Robins, who addressed us on the topic of 'The London School and the Philological Society', or of many others who attended the Symposium. But both the Philological Society and the London School played a crucial role in my intellectual development (if I may use so pretentious a term) in what, as far as linguistics is concerned, were my formative years. For a good part of that time, between 1957 and 1963, I myself was a peripheral member of the London School, albeit an adopted member with eclectic and unorthodox views on certain points of theory (especially in respect of my commitment to generative grammar); and, together with my more senior colleagues at the School of Oriental and African Studies, including

[1] It so happened that the Anniversary Symposium held in 1992 was also a personal anniversary for me: it coincided almost exactly with the 30th anniversary of the first talk that I ever gave to the Society, on 24 November 1962. My title on that occasion was 'Structural semantics with special reference to Greek' (cf. Lyons, 1962). This talk was based on my doctoral dissertation on the vocabulary of Plato – or, to be more precise, on 'Some lexical sub-systems in the vocabulary of Plato' – published in the following year by the Society with the less technical title *Structural Semantics: An Analysis of Part of the Vocabulary of Plato*. The term 'sub-system', used in the title of my dissertation reflects the influence, *via* my supervisors, Professor Allen and Professor Robins, of London School polysystemicism. The approach to semantics that I eventually developed was in most respects distinct from that of the London School, but I was, and up to a point have continued to be, both a polysystemicist and a contextualist.

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Professor Robins himself, I was a regular attender at the Society's meetings.[2]

My choice of topic for my contribution to the Symposium was influenced by several considerations. That I should speak on a topic related to semantics was, I think, expected by Council and the organizers of the Symposium. Also, the fact that my first talk to the Society 30 years previously had been devoted to a particular, structuralist, version of lexical semantics, as was my first book, published by the Society, made this topic nostalgically attractive to me.

When it came to the narrower topic of 'diachrony and synchrony in lexical semantics', which I proposed to Council and which was accepted by them, my selection of this topic was conditioned by the statement in the letter of invitation, to the effect that speakers should deal with 'important issues in the past work of the Society and current issues in theoretical linguistics'. The distinction between diachrony and synchrony, in lexical semantics and more generally, is certainly one such issue. I am not sure that, in addressing it, I shall be putting 'old wine in new bottles' (if I may explicitly evoke the theme or rubric for the Symposium which was published in the original announcement). It may be, rather, a matter of putting new wine in old bottles or, better still, in accordance with established oenological practice in the less famous châteaux, of blending the older cépages with the new and drawing it off from the cuves, as the occasion arises, in recycled bottles. But I would not want you to press the oenological metaphor too hard, since I for one, if challenged, would find it difficult at times to distinguish between the container and the contents and also between the old and the new. However that may be, my choice of topic and my manner of addressing it were conditioned, as I have said, by the highly suggestive theme which Council chose for the Symposium as a whole and which I have chosen as a subtitle for the published version of my talk.[3]

[2] My status as a member of the London School – i.e., as a so-called Firthian – has often been misrepresented, notably by Langendoen (1968, 1969). For comments on this, see the 'Epilogue' to my (1962) article on prosodic analysis, written in 1987 and published in Lyons (1991: 103–109, especially notes 23, 24, 26).

[3] The advertised subtitle of the original version of the Symposium paper was the one that is now used for section 2.

In my choice of topic, I was also influenced by the fact the Symposium was taking place almost exactly 75 years after the publication of Saussure's *Cours*. Now, if we take the publication of the *Cours* in 1916 to mark the birth of modern, structural and synchronic linguistics, as do most Whiggist historians of what is called mainstream linguistics ('mainstream linguistics' itself is, of course, a Whiggist term: cf. Lyons, 1989), we can say that the history of the subject between the foundation of the Philological Society in 1842 and its 150th anniversary in 1992 divides neatly and equally into two periods, a pre-modern, primarily philological and in the later stages predominantly diachronic, period, on the one hand, and, on the other, a period that became increasingly modern, less exclusively comparative-philological and eventually, as far as linguistic theory and a good deal of descriptive work is concerned, in the 1950s, when the London School was so strongly represented in the Society, predominantly synchronic (and structuralist).

It is worth noting at this point that added to the rubric in the original announcement, after 'Old wine in new bottles', was the phrase 'linguistics and philology 1800–2000'. This explicit coupling of 'linguistics' with 'philology' could, in principle, be construed as implying either their separation or their fusion. There may have been some tension, at particular times in the past, between the philological and the linguistic (in the narrow sense of 'linguistic'), as there may also have been some tension between the theoretical and the empirical (and applied). But if this has been the case at times in the past, it is surely not the case today. In my contribution to the Symposium, I have taken the same broad view of linguistics that the Society itself has taken in recent years: I have taken the view that linguistics now subsumes what is traditionally referred to as historical and comparative philology.

Let us now divide the two 75-year periods separated by the publication of Saussure's *Cours* into two (more or less) equal subperiods (and sub-sub-periods). In doing so, I am deliberately following the example set by Charles Hockett in his 1965 Presidential Address to the Linguistic Society of America and, like him, indulging in a certain amount of what he called numerological persiflage.[4] Hockett, it will be recalled, identified four significant

dates in the history of modern linguistics and associated with each of them a theoretical 'breakthrough': 1786, 1875, 1916, 1957. I quote:-

> On 2 February 1786, in Calcutta, Sir William Jones delivered an address to the Asiatic Society, in which occurs a passage that has since repeatedly been hailed as the first clear statement of the fundamental assumption of the comparative method. We may justifiably take that event as the birth of modern linguistics. Between Sir William's address and the present Thirty-Ninth Annual Meeting of the Linguistic Society of America there is a span of 178 years. Half of 178 is 89, a prime number. If we add that to 1786 we reach the year 1875, in which appeared Karl Verner's 'Eine Ausnahme der ersten Lautverschiebung'. Thereafter, two successive steps of 41 years – 41 is also a prime number – bring us first to the posthumous publication of Ferdinand de Saussure's *Cours de linguistique générale* and then to Noam Chomsky's *Syntactic Structures*.
>
> 'I have allowed myself this bit of numerology', he continues, 'because I know you will not take it seriously. But behind this persiflage there is a sober intent. Our fraternity has accomplished a great deal in the short span of 178 years; yet in my opinion there have been only four major breakthroughs. All else we have done relates to those four in one way or another' (Hockett 1965: 185).

The four breakthroughs identified by Hockett are associated by him with what he calls the genetic hypothesis, the regularity hypothesis, the quantization hypothesis and the accountability hypothesis. This is an interestingly idiosyncratic way of referring to both the Saussurean and the Chomskyan revolutions in linguistics But we may let that pass. We can agree that Saussure's

[4] I deliberately drew upon and quoted Hockett's presidential address, not only because of its light-hearted but seriously motivated numerology, but also because, on an occasion celebrating an important milestone in the history of our Society, I wanted to refer, if only indirectly, to the much younger Linguistic Society of America and, again indirectly, to concede that by the mid-1960s, if not before, it was playing the dominant role in the formation of what was shortly to be seen internationally as mainstream theoretical linguistics.

Cours and Chomsky's *Syntactic Structures* were theoretically revolutionary, epoch-making, works.

I well remember reading these words of Hockett, just after I had delivered my Inaugural Lecture in Edinburgh (cf. Lyons, [1965] 1991: 179–201). I also remember thinking to myself at the time (and this is a further reason for quoting the numerological passage from Hockett in the present context) that, since the next significant breakthrough in linguistics was not due until 1998 (41 years after the publication of *Syntactic Structures*), when I could expect to be retired or very close to retirement, I would not have to live through another theoretical revolution in the course of my professional career. And I was happy to be able to report that so far there had indeed been no further revolutionary breakthrough in linguistics since 1957; or, if there had been, I had not noticed it happening. But I also noted that 1998 was fast approaching and that ('who knows?') the blending of the old wine and the new in the proceedings of the Anniversary Symposium might yet, with 6 years in cask, yield a heady and revolutionary (near-millennial) vintage, fully comparable with those of 1916 and 1957. Whether or not this proved to be the case, I said, only time would tell.[5] My present task, as I have said, is to comment historically on the current state of diachronic and synchronic lexical semantics and to do so with particular reference to 'past work of the [Philological] Society and current issues in theoretical linguistics'.

But now for my own attempt at numerology; and behind my persiflage, as there was behind Hockett's, there is a sober intent: though this may not be immediately apparent, much of what I say in this section of the paper is highly relevant to my general theme, not only in that it provides the chronological framework for our consideration of 'past work of the Society', but also in that it gives me the opportunity of making, non-technically, theoretical (and metatheoretical) points that can be picked up later in more technical language in relation to 'current issues'.

[5] As it turns out, this article is being published in *Transactions*, rather than in a special Anniversary volume containing the proceedings of the Symposium and is going to press later than anticipated – in July 1999. I have made only minimal changes to the text. I should perhaps update it at this point by saying that, to the best of my knowledge, 1998 has passed without the perhaps predictable revolution.

Let us grant that the three dates established by Hockett which fall within our period are indeed historically significant; and most linguists would probably concur. Professor Robins in his contribution to the Philological Society's commemorative volume on the Neogrammarians agreed that they were – except that, as he noted, the Philological Society recognised 1876, rather than 1875, as the *annus mirabilis* (cf. Robins, 1978:1). Many of us would also wish to insert the influential work of Rask, Bopp or Grimm into the sequence. So let us do that. The result is a five-stage division by dates, the periods in question being labelled as follows:

i) The age of the prophets: initiated by Jones (1786).
ii) The age of the founding fathers: initiated by Rask (1816), Bopp (1816) or Grimm (1822).
iii) The classical period of comparative philology: initiated by the Neogrammarians in the mid-1870s.
iv) The structuralist (post-Saussurean) period of modern linguistics: initiated by Saussure (1916).
v) The Chomskyan – and post-Chomskyan – period of formalization: initiated by Chomsky (1957).

The Philological Society, it will be noted, was founded between the age of the founding fathers and that of the classical period of comparative philology. Initially, as Professor Davies made clear in her contribution to the Symposium, there was little interest in the Society in comparative philology as such, despite the fact that it elected Bopp and Grimm as honorary members in the first year of its existence.

At this point, I wish to introduce for my own purposes and operating on rather different numerological principles, an additional, contrapuntal series of significant dates. If we divide the period between 1842 and 1916 into two equal sub-periods, we arrive at the year 1879: this was, of course, the year of publication of Saussure's *Mémoire*, which foreshadowed structuralism and applied, in advance of its formal proclamation, the principle of the priority of the synchronic over the diachronic and the method of internal reconstruction. The *Mémoire* was published, it will be noted, only two or three years after the Neogrammarians' *annus mirabilis* (or *anni mirabiles*) and also within a year or so both of

Henry Sweet's two Presidential Addresses to the Society (1877, 1878) and of James Murray's succession to the Presidency and assumption of the editorship of what came to be called the *Oxford English Dictionary on Historical Principles*. The period of the late 1870s was a time of ferment, as also of increasing professionalism, in the Philological Society.

If we similarly divide the period between 1916 and 1992 into two equal sub-periods, we come to 1954, which, as it happens, is the year in which I became a student member of the Society. More to the point, it was the year in which the Society gave official recognition to structural linguistics, under that label, by holding a discussion, on 12 February, led by J. R. Firth and L. R. Palmer. *Transactions* for that year included Haas's justifiably well-known paper 'On defining linguistic units', which starts by asserting that 'despite the perplexing varieties of schools and terminologies so characteristic of recent writing in Descriptive Linguistics', one of the two essential points upon which there is agreement is that 'Descriptive Linguistics has come to be Structural Linguistics' (1954: 54).[6] Haas's view of structural linguistics was, of course, interestingly eclectic, drawing its inspiration, as it did, from both the Prague School and what was coming to be known as the London School. Firth's contribution to the discussion in February 1955 was the basis for his paper entitled 'Structural linguistics', published in the 1955 volume of *Transactions* (1955: 83–103). In it, Firth explicitly distinguished 'philology' from 'linguistics' and, whilst paying tribute to the past achievements and the continued validity of the former, confirmed his own commitment to the latter.

The following year, 1957, was, as Hockett noted, the year of publication of *Syntactic Structures*. It was also, of course, the year in which *Studies in Linguistic Analysis* appeared, as a Special Volume of the Philological Society. The mid-to-late 1950s, as I have already

[6] The same point was made by W. Sidney Allen in his influential (and at the time widely misunderstood) Inaugural Lecture (1957). Allen had moved from London to Cambridge, to take up the Chair of Comparative Philology, in 1955. His Inaugural Lecture contains one of the best, and theoretically most interesting, introductions to London-School linguistics in relation to other kinds of contemporary structural linguistics written at the time by someone who was very much involved in its development in this, its most creative, phase.

remarked, were vintage years for the London School, as well as being the years during which the balance or emphasis in the Philological Society switched decisively from philology to linguistics and from the diachronic-comparative to the synchronic.[7] These two developments are not unconnected. As Professor Robins had emphasized, the London School was particularly influential in the Philological Society: at this time: Firth himself was its President from 1954 to 1957; the late Professor N. C. Scott was Secretary from 1954 to 1961 (and Professor Robins himself Secretary from 1962 to 1987); most of its members were regular attenders at meetings of the Society, and several of them were prominent as speakers. *Transactions*, together with the *Bulletin of the School of Oriental and African Studies*, was the principal organ for the dissemination of the distinctive features of London School structuralism: polysystemicism, an emphasis on the syntagmatic, and a rejection of the distinction between 'langue' and 'parole' (or any comparable conceptual and terminological distinction that was current in the 1950s or became so later: 'system' and 'process', 'competence' and 'performance', 'semantics' and 'pragmatics', etc.). I will turn to the London School approach to semantics, including lexical semantics, in due course. But first let me continue with, and complete, my numerological interlude.

If we divide the period between 1916 and 1954 into two equal subperiods (or sub-sub-periods), we arrive at the year 1935, which marks another significant point in our contrapuntal series: 1935 was the year in which both Firth's 'Technique' and Malinowski's *Coral Gardens* first appeared in print. I will come back to these two works in a moment. But let us note at this point that, in his *Principles of Semantics,* Stephen Ullmann, selected 1931, rather than 1935, as 'the most crucial date in the history of semantics' (1951: 2). It was Ullmann's book, especially in its second edition (1957), which more than any other single work made the principles of post-Saussurean structural semantics, particularly the theory of lexical

[7] Allen (1957) is perhaps, once again, the best supporting reference for this statement (together with Allen's own references and commentary). But Allen, like Robins, was untypical of most members of the London School, at that time, in the (appropriately eclectic) catholicity of his structuralism, as also in his immediate recognition of the importance of Chomsky's *Syntactic Structures* (1957).

fields ('Wortfelder') or semantic fields ('Sinnfelder') familiar to British scholars, during what I have described as *anni mirabiles* in the history of the Philological Society, and put structural semantics – more precisely, structural lexical semantics – in its historical context. In doing so, he referred to a wide range of twentieth-century works (in several languages). Ullmann chose 1931 as 'the crucial date' because it was the year in which both Trier's *Wortschatz* and Gustav Stern's *Meaning and Change of Meaning* were published, the latter representing the culmination of the traditional diachronic approach to lexical semantics and the former demonstrating the validity and fecundity of a particular version of the newer structuralist approach, which gave priority to the synchronic. (It was, however, Trier's 1934 paper, entitled 'Das sprachliche Feld' which popularized – if that is the right word – the notion of what generally came to be known subsequently, in English, as semantic fields and made more accessible the principal findings of his *Wortschatz* monograph.) For Ullmann, and for the majority of linguists writing in the 1950s, 'semantics' meant primarily, if not exclusively, lexical semantics. This, as we shall see in the following section, was not the case in an earlier period.

Interestingly different from both Trier's *Wortschatz* and Stern's *Meaning and Change of Meaning (*but in certain respects comparable with Bühler's *Sprachtheorie*, 1934, to which Ullman gives little attention) were, on the one hand, Alan Gardiner's 1934 contribution to the discussion of linguistic theory, published in *Transactions* (his justifiably influential book, *The Theory of Speech and Language*, having been published in 1932) and, on the other, Firth's 'Technique of semantics' (1935), also published in *Transactions*, and Malinowski's *Coral Gardens and their Magic* (1935), which I mentioned earlier. Taken together, these three works, despite their individual differences, all contributed to the development of what was later seen as a characteristically British – Firthian, and subsequently, with further developments and refinements, neo-Firthian – functionalist and contextualist, but thoroughly structuralist, theory of semantics. Firthian semantics was emphatically not restricted to lexical semantics; and it is not until the 1950s, when the London School was coming to be known as such, that there emerged a distinctively

Firthian theory of lexical semantics (based on the notion of collocation).

So far, I have been establishing a chronological (and thematic) framework. I could go on in this manner, noting significant dates and saying why they are significant. But this will suffice for present purposes. Let me now explain what these purposes are and, like Hockett, reveal my own 'sober intent'.

First of all, and most obviously, I wanted (as Hockett did) to chart some of the milestones on the path of progress. And, in so expressing myself, I am, of course, deliberately nailing my Whiggist flag to the mast. I make no excuses for this (cf. Lyons, 1989). But I would wish to cast doubt on any periodization along a single time-line, even for a single theme or idea, whether by centuries or by such significant datable events as the publication of a seminal book or the holding of an international congress. Although the dates I have come up with, by applying my deliberately whimsical numerological principles, are undoubtedly significant, at least in retrospect, they are to some degree historically arbitrary. We can find precursors for Jones, Grimm, Verner, Saussure or Chomsky – or for any other great scholar of the past. And their work anyway developed in a context which was (dialectically, as thesis to antithesis) as much a part of the original and revolutionary idea – the genetic hypothesis, the regularity of sound-change, the priority of relations over entities, or whatever – as the revolutionary idea itself.

In fact, and this is really my main purpose – the soberest part of my intent – what one may refer to as the paradox of the structuralist principle of the priority of the synchronic over the diachronic applies just as much in the history of ideas as it does in the history of languages, and for similar reasons: the principle is both unchallengeable and yet in the limit inapplicable (see section 4 below). No wonder that Saussure himself failed to reconcile diachronic atomism with synchronic structuralism. (No wonder also that, at the end of his career, he was reduced to inarticulacy by his despair at the impossibility, for him at least, of saying anything coherent about language in language. One knows the feeling!)

Having established the chronological framework, let me now provide a few statistics relating to the different periods that we

have identified in the history of the Philological Society. It is not easy to classify the five hundred and eighty or so articles published in the *Proceedings* and *Transactions* between 1844 and 1916.[8] But some generalisations can be made. Obviously enough, for reasons which are well enough known, but which I will mention and comment upon in the next section of my paper, no article uses the word 'semantics' or 'semantic' in its title before the publication of Bréal (1897); less obviously, no title uses these words between 1897 and 1916. More surprisingly perhaps, only a handful of articles throughout the whole of this period use the word 'meaning' in their title. But there are many articles on etymology, especially by such scholars as Skeat and Weekley; and there are many on what is called the 'use' of particular tenses, moods and other grammatical categories, which nowadays many, including me, would classify under non-lexical, grammatical (more precisely, categorial, rather than structural or configurational), meaning. (As we shall see later, however, there is an ambiguity in the terms 'word' and 'lexical', as they are currently employed in linguistics, which means that, under one interpretation of either or both, some part of categorial meaning can be properly described as both grammatical and lexical.) Also to be noted are the famous papers by Henry Sweet on 'Words, logic, grammar' (1875–6) and the first Annual Address of the President to the Society, Alexander Ellis, 'On the relation of thought to sound as the pivot of philological research', delivered at the Anniversary Meeting on Friday, 17th May, 1872. Both of these papers, though they do not claim to be dealing with semantics, are definitely inspired by what we would nowadays call a semantic point of view. They were referred to 84 years later, in a context which is highly relevant to my topic, by J. R. Firth, in his Presidential Address.

When it comes to the later periods we have the benefit of Professor Collinge's *Index* (1968) for 1917–1966 and Dr Wheeler's *Index* (1992) for 1967–1992, both of which list articles, not only under their author and the language or language family to which

[8] I have not generally listed in the references papers from this period. Readers who wish to identify the papers referred to or follow up on the detail should consult the Index published in TPhS [28] for the years 1917–20 (1932). They will find that it is not always easy to determine the content from the title.

they refer, but also under the topic or branch of linguistics and philology with which they deal.[9]

Of the 268 articles published in *Transactions* in the half-century between 1917 and 1966 only nine are classified under 'Semantics' in Collinge (1968: 19). But several of the other articles classified by him under 'Etymology', 'Lexicography', and 'Onomastics' at least impinge upon semantics, including lexical semantics, in a sufficiently broad sense of the term. So, too, do several articles classified under 'Linguistic theory', notably Gardiner (1934), Brough (1951) and, of course, Firth (1955, 1956). That said, however, there is no question but that there are relatively few articles in that period which deal with semantics as such and very few that deal with what would nowadays be referred to as lexical semantics.

Of particular importance in the history of the Philological Society, and more generally, in the period from 1857 to 1934, are the papers relating to what later became the *Oxford English Dictionary*, but which in the present context should perhaps be referred to, as it was throughout this period, as *The Society's Dictionary*. Especially noteworthy in this connection are the three papers relating to the original 'Proposal of a New English Dictionary' printed in *Transactions* for 1857–60 and the highly informative reports by its successive editors, Murray, Bradley, Craigie and Onions. These papers, too, were referred to by Firth. The official title of the Dictionary, when published, was of course *A New English Dictionary on Historical Principles*; and what was new about the dictionary (when it was planned in 1857) was the very fact that it was organized on historical principles. The Society's dictionary, therefore, was from the outset the product, as far as its definitions and their organisation are concerned, of what one might nowadays call applied diachronic lexical semantics. I will take up this point in section 4.

As we have noted, relatively few articles devoted to semantics, and even fewer articles dealing with synchronic lexical semantics, were published in *Transactions* in the period covered by Collinge

[9] The earlier index to *Proceedings* and subsequently to *Transaction* (see note 8 above) grouped articles under 'English subjects' and 'Other subjects'. This is, of course, a reflection of the Society's origins and its primary concerns for at least the first half of its 150-year life (see section 4 below).

(1968), which included the *anni mirabiles* of both the 1930s and the 1950s. At first sight, the situation would seem to have changed significantly in the quarter-century that has passed since then (as the situation changed more generally during this period, in this respect, in what may be thought of as mainstream linguistics). Wheeler (1992) classifies 15 of the articles published between 1967 and 1992 under 'Semantics'. But many of these are also concerned with other topics, and some of them have these other topics as their primary concern. It is one of the principles adopted by Wheeler that 'a single article may appear under . . . up to three topic headings' (1992:3); and it is clear that he might well have carried much further than he did this eminently justifiable principle of multiple classification. In short, the increased representation of semantics in the proceedings of the Philological Society in the period between 1967 and 1992 is not quite as striking as it might appear to be if we compare Collinge (1968) with Wheeler (1992).

But this is perhaps something of a mechanical and fruitless exercise anyway. The list of topics with which Wheeler (1992) operates differs from that used by Collinge (1967); and this difference reflects, to some degree, developments that have taken place recently in linguistics. The most important of these perhaps is the now widely accepted distinction between 'semantics' and 'pragmatics': Wheeler (1992) classifies three articles under 'pragmatics' which at the time they were published would not have been so classified, because the term had not yet made its entry into the linguist's stock-in-trade.[10] The distinction between semantics and

[10] One of the pleasures, innocent and at times productive, of looking through indexes is that it gives full scope for serendipity. I was surprised to find a paper in *Transactions* from the 1850s entitled 'On pragmatised legends' (Malden, 1854). This discovery led me to the entry for the verb 'pragmatize' in *The Society's Dictionary*, where it is defined, with the support of a citation from 1834, as '[to endeavour] to extract historical truth out of mythic legends'. I was sorely tempted to relate this use of the term, anachronistically and somewhat whimsically (but with 'sober intent'), to what I will refer to later as the depragmatization of semantics, against which Firth, in particular, but also Gardiner and Malinowski, inveighed in the 1930s. It is interesting to note that the *Dictionary* cites Malinowski's *Coral Gardens* (1935) for 'pragmatic' used of utterances that have 'an active and effective influence . . . within a given context'. I had not remembered this definition. The modern, technical, use of 'pragmatics', in which it contrasts with 'semantics', is generally traced back to Charles Sanders Peirce (cf. Lyons 1977: 99ff.).

pragmatics (independently of the use that is made of it in Wheeler's *Index*) is one to which we shall return.

2. Michel Bréal and his successors

When I chose the heading for this section of my paper, I deliberately used the word 'successor', rather than the ambiguous word 'follower', because I did not wish to give the impression that I was concerned exclusively, or even primarily, with the tradition of lexical semantics which continues and develops the ideas attributable to Bréal. In fact, I shall have less to say about this tradition than I had originally intended. There is now readily accessible, as there was not until recently, a considerable body of work that deals with 19th-century and early-20th-century semantics and with the part played in its development , not only by Bréal, but also by his predecessors, his contemporaries and his successors.[11]

In choosing the heading for this section, I had in mind two generalizations, relevant to my topic, which are commonly made, and which I myself frequently make when I am lecturing on semantics. The first is that the branch of linguistics to which the term 'semantics' was applied by Bréal and his contemporaries at the turn of the century was predominantly diachronic and did not adopt the synchronic (and structuralist) point of view until the 1930s (and of course, as practised by many linguists, continued to be predominantly diachronic until much later than this). The second generalization is that throughout most of this century the branch of linguistics to which the term 'semantics' was applied dealt more or less exclusively with what was referred to as the meaning of words: i.e. to what is nowadays commonly called lexical meaning. I put the name of Bréal in my subtitle simply because he is generally and, as far as I know correctly, regarded as the person who first used the term 'semantics' (or to be more precise the French word from which the English word was almost immediately borrowed, in the late-19th century) to label what he and others described at the time as a new

[11] Particularly important for the period from 1830 to 1930 is Nerlich (1992), to which reference may be made for background information not given in the present article and for an an introduction to, and detailed commentary upon, the primary and secondary sources.

branch of linguistics (or philology) and, in coining the term, explicitly restricted its scope to the study of changes in the meaning of words.

Bréal first used the term in his 1883 article, the contents of which were absorbed into his well-known book, *Essai de sémantique* (1897), the English version of which was published in 1900. The term 'semantics' was adopted by certain scholars in England and in the United States, even before the publication of Bréal's book, which popularized it. Thereafter, 'semantics' soon came to replace 'semasiology', which was being used in English from the mid-century (based on the German 'Semasiologie' as used by Reisig, 1829) for what is defined in the Society's Dictionary as 'that branch of philology which deals with the meanings of words, sense development and the like'. The etymology and history of these and other related terms, is well known, and there is no need to go into it here (cf. Read 1948; Ullmann 1950). My topic is diachrony and synchrony in lexical semantics, and that is the focus of such remarks as I will make here about Bréal and his successors.

Since the occasion for the preparation and presentation of this paper was the celebration of the 150th anniversary of the Philological Society, I should mention, and give due emphasis to, the fact that the English edition of Bréal's book (1900) contains a 50–page laudatory Preface and a 25–page Appendix by P.J. Postgate, who had long been an active and influential member of the Philological Society, and was one of its Vice-Presidents in the early years of this century. It is also worth mentioning, perhaps, that it was Lady Victoria Welby who, having corresponded with Bréal, was responsible for getting his book published in England. As far as I know, she had no connection with the Philological Society and her interests were very different from those of most of its members. But she was, of course, a very important figure at the time in the popularization and development of semiotics (or what she called significs): she corresponded with Bréal and with Charles Sanders Peirce, and was as an associate of Ogden and Richards.[12]

[12] She wrote several influential books and articles, notably *Grains of Sense* (1903), *What is Meaning?* (1903) and 'Significs' (1911). It is also interesting to note that the Welby Prize which, influenced by Postgate, she established for an essay on significs (i.e. semantics) was won by Frederick Tönnies (the Essay was published in *Mind,*

I myself first read Bréal's book, in one of the French editions, between 1954 and 1956, when I was about to embark on my doctoral research on the vocabulary of Plato. The context in which I read it was totally unhistorical. Having specialized in classical philology as an undergraduate, I was familiar with the ideas of the Neogrammarians, including those of Hermann Paul's *Prinzipien* (1880) on the role of analogy in language-change. I was also familiar with some of the general works of such scholars as Meillet, Vendryes and Jespersen, which had assimilated the ideas of the Neogrammarians and were in various ways modifying them. But I did not consciously relate the ideas that I found in any one of these authors to the ideas I found in any of the others. As far as Bréal's *Essai* is concerned, the context for me (and no doubt for many others who came into linguistics at that time) was set initially by Ullmann (1950), for whom (as for most of his contemporaries) 'semantics' referred primarily to lexical (or lexicological) semantics, even if in principle his division of the field of linguistics, like that of Bréal or Postgate, allowed for grammatical (or syntactic) semantics

1899/1900), to whom we owe the distinction between 'Gesellschaft' and Gemeinschaft' ('society' and 'community'), which has played such an important role in Continental, especially German, anthropology and sociology . I have argued elsewhere that the distinction between 'communion' and 'communication' in Malinowski's theory of meaning must be related historically to the 'Gesellschaft'/ Gemeinschaft' dichotomy (and to its derivates: Lyons 1991c). The term 'communion', employed by Malinowski (in collocation with 'phatic') is widely misunderstood by linguists. (Jakobson's 1960, unfortunate use of 'phatic' has, of course, contributed to the misunderstanding.) In semantics, Malinowski laid particular emphasis on what I would refer to as the socio-expressive (interpersonal and intersubjective), rather than the descriptive or propositional, meaning of utterances: i.e., on communion rather than communication. So too, of course, did J. R. Firth, who collaborated with, and, as is well known, was greatly influenced, by Malinowski. It is impossible to follow up in a single article all the lines of intellectual influence, direct and indirect, that link one scholar with another (even when these links can be traced) and thus, subsequently, one school of linguistics with another. Recognized schools of linguistics (the London School, the Prague School, the Glossematicians, the Bloomfieldian School, the Chomskyan School, etc.) will always tend to emphasize what divides them, rather than what they have in common. I have mentioned the point about 'communication' and 'communion' and its link with the 'Gesellschaft'/'Gemeinschaft' distinction, partly because, to the best of my knowledge, it is not generally recognized in histories of twentieth-century semantics, but mainly to illustrate the complexity of the network of connections among representatives of what standard accounts of the historical development of linguistics (including my own avowedly 'Whiggist' account in Lyons 1968, and elsewhere) deal with as separate schools or movements.

as its complementary. The fact that I did not read the *Essai* again until very recently (although I have dipped into it for particular points over the years), coupled with the further fact that I had not previously looked properly at the English edition, meant that when I did re-read it for the present purpose, I was surprised to discover that (despite the fact that semantics is explicitly defined as 'l'étude de la signification des mots') much of the *Essai* – not to mention much of Postgate's Preface and Appendix – is devoted to what we would now call non-lexical meaning.[13] This is not the impression one gets from references to it in the later literature, including, importantly, Carnoy (1927), who clearly saw himself as following in the footsteps of Bréal (1897).[14] Why there should have been this apparent contradiction between Bréal's explicit definition of semantics as 'l'étude des significations des mots' and his application of this definition in the *Essai* is a question that may be left for the following section.

So, one of the of the two generalizations that, until recently, I had been in the habit of making about Bréal's (and his contemporaries' and immediate successors') interpretation of the new term 'semantics' was historically incorrect (in terms of the present-day semanticist's understanding of the term 'lexical'). The first of my two generalizations, however, was unaffected by my re-reading of the *Essai*: as later authorities correctly record, it was indeed diachronic, not synchronic, semantics that Bréal was concerned to establish as a new science.

A second question that came to mind when I re-read Bréal's *Essai* was why he himself, and others such as Postgate and Lady Welby in this country, thought that, in the *Essai* and in the relevant articles

[13] It is interesting, in this connection, to read Joshua Whatmough's Introduction, in conjunction with Postgate's Preface, in the Dover Publications reprint (1962) of the original English edition. Whatmough is writing from the viewpoint of modern mid-twentieth-century linguistics (and his own rather idiosyncratic interpretation of it). One would think that Postgate and Whatmough are talking about two different books.

[14] Carnoy's book, in my view, is very much undervalued in general introductions to twentieth-century lexical semantics. Admittedly, his terminology is rather daunting (even to classicists) and perhaps unnecessarily refined. But he carried the Bréal-type classification of the different types and sub-types of meaning to the limit and, in doing so, helped, at that time, to codify and clarify the classificatory principles themselves.

that foreshadowed it in the preceding decade and a half or so, he was launching a new and previously unbaptized discipline ('une science nouvelle').

There was nothing original about Bréal's major distinction between the study of meaning and the study of form, or about his assumption that syntax is (as we would put it) semantically based. This view was eminently traditional: it had not been challenged by the Neogrammarians, and it was not to be seriously challenged in mainstream linguistics until well into the twentieth century (by the post-Bloomfieldians). When it comes to the (so-called) laws of semantic change themselves, including the famous laws of specialization and irradiation, it is not clear that there is anything strikingly original, from a theoretical point of view, in Bréal's formulation of them or in his discussion of the examples he cited to illustrate them. The same can be said, I think, of his treatment of polysemy, of the role of analogy and metaphor in semantic change, and perhaps also of his invocation of the notion of subjectivity (to which he devotes a whole chapter).[15] He was not the first to identify as causal factors in language-change a set of general principles which, generally speaking, had traditionally been seen as synchronically operative in the individual's use of language (i. e., as stylistic or, to use a modern term – or rather, an old term in a modern sense – pragmatic). The fact that he was the first to collate and (up to a point) systematize these principles (and to illustrate them in detail) goes some way towards explaining and justifying Bréal's claim to be the founder of a new branch of linguistics or, as he put it, a new science.

It may also be conceded – and this is perhaps the important point as far as the dialectical development of linguistic theory is concerned

[15] The notion of subjectivity has long held a central place in Continental, especially French, linguistics, and has of course recently been taken up, and developed in interesting ways, in mainstream Anglo-American diachronic (lexical and non-lexical) semantics (cf. Yaguello 1994; Stein and Wright 1995). The role of subjectivity in both synchronic and diachronic semantics is certainly one of the 'current issues in linguistic theory' with which we should be concerned. There is a direct line of descent, of course, in French linguistics from Bréal, through Meillet, to Benveniste, in respect of the importance they attached to subjectivity (and also to social factors). But subjectivity was prominent too in Bühler (1934), which reflected, and itself influenced, Prague-School views of meaning. London-School semantics did not explicitly invoke the notion of subjectivity (Firth himself, at least, being hostile to anything that smacked of mentalism), but, in my view, could easily have accommodated it.

– that, in the context of the orthodox attitude towards language-change in what I described, in section 1, as the classical period of comparative philology, Bréal's approach was original in the sense that it was different from that of the acknowledged major theorists and practitioners of the day. But, paradoxically perhaps, his originality in this respect, such as it was, derived, not from the assertion and exposition of a novel point of view, but from his re-assertion of what was in fact a rather conservative view of the nature of language. And it was a view with which, in contrast with that of the Neogrammarian revolutionaries of the 1870s, both the general public and many philologists could feel comfortable.

Bréal' s approach to semantics was consistent with his decision to translate, as the basis for his lectures as the Collège de France and in order to make more readily accessible in France the fruits of German scholarship, Bopp's by then dated *Vergleichende Grammatik* (in its second, 1851, edition), rather than Schleicher's more recent *Compendium* (1857).[16] And it should not be forgotten, in this connection, that Bréal was primarily a comparative philologist: he became a semanticist almost by default. The driving force behind Bréal's presentation of his approach to semantics was his hostility, first of all to Schleicher's organicism, and subsequently to the mechanistic positivism of the Neogrammarians and their almost exclusive concern with form. Throughout the *Essai*, as in his earlier articles, he emphasized the purposive and the instrumental functions of language; he also emphasized its social basis. His so-called laws of semantic change, which were presented as laws of the mind ("des lois intellectuelles"); were not laws at all, but general tendencies: unlike the Neogrammarians' sound-laws, they were not conceived as having a nomic, or imperative, sense or as operating, even in theory, without exceptions. It was no doubt this aspect of his work – his mentalism and anti-positivism – that made it so congenial to those of his contemporaries, including Postgate, who saluted it, in the context of late-19th-century linguistics and philology, as both original and inspiring.

[16] Despite his opposition to what we (like the Neogrammarians themselves) think of as the more scientific approach to linguistics promoted by the Neogrammarian insistence on the principle of regularity (and in the limit 'Ausnahmslosigkeit'), it was Bréal who was responsible for Saussure's appointment at the Collège de France.

One is reminded, in this respect, of Chomsky's re-assertion of mentalism, in the early 1960s, in the context of post-Bloomfieldian mechanistic positivism. Of course, one must not push such comparisons too hard. In both cases, however, the oenological metaphor with which I have been supplied and which I am using as my subtitle – 'old wine in new bottles' – inevitably comes to mind, as it will do throughout this article. But I will not continue further in this vein. There are those, no doubt, who would say that, in this section, I have underestimated the originality of Bréal's contribution to the development of semantics. This may be so. But the main point I want to make here (and is applicable throughout) is one that I made many years ago when I was trying to persuade a sceptical audience that Chomskyan transformational-generative grammar was, in its aims, if not in its methods, terminology and notation, eminently traditional, but could not for that reason be dismissed as lacking in originality and theoretically uninteresting: *plus ça change, plus ça n'est plus la même chose* or, to be more explicit, *plus cela paraît la même chose, plus cela a en fait changé* (cf. Lyons, [1965] 1991: 189). The bottling makes all the difference.

I have suggested that one reason for the immediate popularity of Bréal's *Essai* (independently of the originality of the views he was expressing) was its forceful representation of an alternative view of language change to the one which by the 1880s had established itself as orthodoxy. Another reason why it had the impact that it did have at the time, and continued to have for decades thereafter, was that it was written in a very readable style.[17]

That Bréal's *Essai*, in both the French and the English version, was successful in interesting many philologists, as well as the general public, in 'the new science' of (what we would call) diachronic lexical semantics is beyond doubt. And yet, as will be clear from the statistics given in the preceding section, there is little evidence of an increased interest in dischronic lexical semantics in the articles published in *Transactions of the Philological Society* in the early decades of this century. Given the Society's abiding concern with the production of a dictionary of English 'on historical principles' (and

[17] As Meillet was to put it in his obituary article (re-published in Sebeok (1966), Bréal 'savait écrire et prenait la peine d' écrire' and the *Essai* was 'une oeuvre d'art'.

Postgate's powerful support for Bréal's point of view), this is somewhat surprising. As we shall see presently, the Society's most distinctive contribution to semantics did not come until later (during what might be called the London School period); and, when it did come, it owed very little to the tradition of diachronic lexical semantics popularized, if not actually initiated, by Bréal.

3. Semantic theory and theoretical semantics; lexical and non-lexical semantics

My brief is to relate my topic, diachrony and synchrony in lexical semantics, to 'important issues in past work of the Society' and to 'current issues in theoretical linguistics'. This necessitates a few general comments, first of all, on linguistic theory and theoretical linguistics and, then, on the distinction between lexical and non-lexical semantics.

Clearly, the term 'semantics' is theory-laden, and what it covers will be determined to some degree or another by the theory of meaning with which it is associated, explicitly or implicitly, in different periods or by different schools of linguistics. The coverage of the composite term 'lexical semantics' will be determined, not only by the particular interpretation that one gives to the head-noun 'semantics', but also by the way in which the distinction between the grammatical and the lexical – between the grammar and the lexicon – is drawn in particular theories of the structure of natural languages. But what do we mean these days by 'linguistic theory'? And is this what has always been meant by the term during the period with which we are concerned?

In a number of recent publications, I have been drawing what I hope is a useful and a historically justifiable distinction between the terms 'linguistic theory' and 'theoretical linguistics', to label what have recently emerged as two rather different, but complementary and equally important, sub-branches of linguistics, both of which are, in their way, theoretical. Briefly, the distinction is based on the difference between an older and a newer, or a weaker and a stronger, sense of the word 'theory'. By 'theory' in the older or weaker sense – and this is the only sense that the term can bear for the greater part of the period with which we are concerned – I mean a set of general

principles which (according to whether it is descriptive or explanatory) informs and guides the description or explanation of a given body of data which it takes as its subject matter. It is in this, the older or weaker, sense of 'theory' that 'theory' is commonly opposed to 'practice', the adjective 'theoretical' being correspondingly opposed to 'practical'. By 'theory' in the newer and stronger sense I mean a mathematically precise formal system within which theorems can be proved by deduction from the initial postulates or axioms and, if the theory is empirical rather than purely formal (and can be put satisfactorily into correspondence with the data that it purports to describe or explain), can be interpreted as embodying empirically falsifiable or confirmable predictions.

Theories in the stronger sense, let us call them type-2 theories in contrast with the older type-1 theories, are of relatively recent origin even in the natural sciences. In linguistics they originated just before the second World War, I suppose, with Hjelmslev and his collaborators. But Glossematic theories of the structure of languages never attracted more than a minority of linguists, For most of us, the formalization of linguistic theory – the theoreticization of linguistics: i.e. the conversion of linguistic theory (or parts of linguistic theory) into theoretical linguistics – is seen as being one of the products (and I would say the principal product) of the so-called Chomskyan revolution. It is for this reason that in my global periodization of the history of linguistics in section 1 I referred to the post-Chomskyan period as the age of formalization. I might equally well have called it the age of theoreticization.

I hasten to add, at this point, that in calling type-1 theories weaker and type-2 theories stronger, I am not suggesting that type-2 theories are in all respects superior to type-1 theories or that type-2 theories have rendered type-1 theories obsolete, though many theoreticians might consider this to be the case. On the contrary, I take the view that much (type-1) linguistic theory, traditional or modern, is far richer and empirically sounder than any contemporary branch of theoretical linguistics. For some time they have been, and for the foreseeable future they will be, in a complementary, dialectical (and dialogic), relation, such that each is advanced by respecting, sympathetically considering, and appropriately drawing upon the insights and achievements of the other.

I think it is fair to say, at this point, that theoretical linguistics, in contrast with linguistic theory, has never been strongly represented in the Society's proceedings and publications.[18] Indeed, there have been those, possibly a majority of its members, including J. R. Firth and other prominent adherents of the London School, who were distinctly hostile to it. It is perhaps arguable that London School phonology (generally referred to as prosodic analysis), as it was developed and exemplified, not by Firth himself, but by some of his followers, had been at least partly formalized (i.e., theoreticized) by the mid-1950s; it was certainly formalizable – at least to the degree that other contemporary phonological theories, such as classical post-Bloomfieldian phonemics or distinctive-feature analysis, were being formalized. But no attempt was made to formalize the London-School theory of semantics either by Firth himself or by his collaborators.[19] Indeed, the whole tenor of its presentation and exemplification was definitely anti-theoretical, in the type-2 sense of the term 'theoretical'.

I will not seek to defend or justify here what I have just been saying about linguistic theory and theoretical linguistics: this I have done in some detail elsewhere (Lyons 1991a: 27–45). In my discussion of diachrony and synchrony in lexical semantics, I will simply take for granted the validity of the distinction between type-1 and type-2 theories and apply this distinction first to semantics and then more specifically to lexical semantics. But I will interpret the phrase 'theoretical linguistics 'in that part of my brief which refers to

[18] In his contribution to the Symposium, Professor Matthews discussed the impact of linguistic theory on the Philological Society in the 20th Century (see the published version of his article in the present volume). Although he did not, of course, operate with the distinction that I draw between theoretical linguistics and linguistic theory, what he had to say on role of was consistent, I think, with the view that I took and have made explicit here.

[19] There was a striking difference, in this respect, between the Firthians and the neo-Firthians. Michael Halliday's 'Categories' (1961), which drew not only on Firthian ideas, but also, directly or indirectly, on ideas that came from other contemporary schools of structuralism, did of course come close to formalizing a neo-Firthian, non-generative, theory of grammatical structure at the very time that Chomsky's ideas were just beginning to occupy the dominant position in theoretical (and meta-theoretical) linguistics that they continued to occupy for at least the next quarter-century. There was no comparable attempt to theoreticize a neo-Firthian theory of semantics, except perhaps at the level of what was called 'lexis' (cf. Sinclair 1966).

current issues in theoretical inguistics as covering both type-1 and type-2 theories, as it was clearly intended that I should. A particular reason for doing so, of course, is that the status of (type-2) theoretical semantics is itself one of the major 'current issues in theoretical [or meta-theoretical] linguistics'.

Type-2 theoretical semantics, especially in its dominant version based on the principle of truth-functionality, is usually referred to as formal semantics. This, therefore, is the term that I will use for it in what follows (with some reluctance, given the origins of the term 'formal semantics' and the many senses that the word 'formal' has borne in 20th-century linguistics). In a more comprehensive discussion of the question, a further distinction should be drawn: between linguistic and non-linguistic semantics and, consequently, between linguistic and non-linguistic formal semantics (cf. Lyons 1995a). The relations between linguistic and non-linguistic semantics are not, in fact, irrelevant to my topic; and in what respect this is so will be made clear presently. Meanwhile, let me establish the terminological convention that, unless and until they are further qualified, the terms 'semantic' and 'semantics' (and their compounds: 'formal semantics', 'lexical semantics', etc.) are to be interpreted as referring to linguistic semantics (and its subdivisions).

In the first two sections of this paper, I have been talking as if the terms 'semantics' and 'lexical semantics' are unproblematical in their application both to 'past work in the history of the society' and to 'current issues in theoretical linguistics'. But this is not of course true. As I have just said, both terms – like all the technical terms of the philologist's or linguist's metalanguage – are theory-laden; and one of the major advances made in mid-to-late 20th-century linguistics (in the period of formalization: the theoreticization of linguistic theory) consists in the realization that this is so, in working out the implications of this realization, and in the resultant increased precision and sophistication, not only of current (type-2) theoretical linguistics, but also of current (type-1) linguistic theory.

Earlier linguistic theory, and in particular earlier semantic theory, inevitably strikes present-day theoretically-minded linguists as unscientific and imprecise and, in the limit perhaps, riddled with ambiguities and equivocations to such a degree as to be literally

uninterpretable. When it comes to any attempt that we might make to eliminate these apparent ambiguities and equivocations by reformulating in terms of our own technical metalanguage the points being made in the pronouncements of past theorists (and theoreticians) of the subject (in so far as we understand them), we are constantly at risk of importing into our discussion of these pronouncements our own particular brand of Whiggism (see section 1). This is a risk that must be acknowledged. That said, however, I think it is clear that, over the period with which we are concerned, there is a sufficient degree of constancy in the meaning of at least the central terms in the non-technical metalanguage of linguistics (including such everyday words as 'word' and 'meaning') for us to be able to interpret the relevant texts of the past in relation to 'current issues in theoretical inguistics', without inappropriate anachronism.

I assumed of course that this is so in my discussion of the ideas of Bréal and his successors in the preceding section. I assumed, in particular, that when scholars of the late-19th and early-20th century defined semantics as the study of meaning or, more specifically, as the study of the meaning of words, these definitions have the same import for us (pre-theoretically) as they had for them. I did, however, point out that there was an apparent contradiction between Bréal's explicit restriction of semantics to what we would nowadays call lexical semantics and his treatment, in several chapters of the *Essai*, of topics that for us fall within the scope of grammatical (i.e., non-lexical) semantics.

The apparent contradiction is resolved as soon as we recognize, first, that the French lexeme 'mot' (like the English lexeme 'word' and its translation-equivalents in many European languages) has several senses, and, second, that, it is only if we interpret it in one, rather than the other, of these senses – in the sense of 'lexeme' or 'lexical item' – that the phrase 'la signification des mots' ('the meaning of words') is (more or less) equivalent to what we would nowadays call lexical meaning. On the other hand, if we interpret it as having the same sense as 'word-form' (or, alternatively, allow that it could have both senses simultaneously), we can see that it was quite reasonable for Bréal and Postgate to deal with the meaning of grammatical categories and even word-order (i.e., the

order of word-forms) under their definition of 'semantics'. In inflecting languages, such as Ancient Greek and Classical Latin (and these were the languages with which they and most of their colleagues were primarily concerned), a good deal of grammatical (i.e., non-lexical) meaning is encoded morpho-syntactically in word-forms. It was natural therefore that they should include this part of the grammatical meaning of phrases, clauses and sentences under the rubric of 'the meaning of words'. Subsequent developments in the 'new science' had the effect that by the late 1920s, if not earlier, and for some 30 or 40 years thereafter it was indeed restricted to what we now call lexical semantics. This seems to be the reason why nowadays we almost automatically take Bréal's definition to be similarly restrictive.

So much, then, for the initially puzzling apparent contradiction between Bréal's definition of 'semantics' and his application of this definition. Once we see that there is this apparent contradiction and the reason for it, it is easy enough to make the necessary adjustments and, as later commentators have done (without necessarily noticing the apparent contradiction), to relate Bréal's work to those of his predecessors and successors in what we now identify as a continuous tradition of (diachronic) lexical semantics, starting in, say, the 1820s, with the work of Reisig, and divisible into what could be seen retrospectively by the mid-20th century as 'three distinct phases' (Ullmann 1962: 5 ff.; cf. Kronasser 1952: 29ff.; Baldinger 1957: 4ff.).

But there are more troublesome problems attaching to the term 'lexical meaning', as far as the recent history of semantics is concerned. Strictly speaking, in (type-2) theoretical linguistics we cannot interpret the term 'lexical' except in relation to some particular theory or model of the structure of a language within which the lexicon is organized in relation to (or as an integral part of) the grammar. The traditional distinction between the lexicon, or vocabulary, and the grammar (including both inflexional morphology and syntax) was of course well established (initially in relation to the classical languages) for centuries; and it was not seriously challenged until modern times. It is for this reason (as also because we can interpret with confidence the everyday-metalanguage terms 'word' and 'meaning') that, as I put it earlier,

we can make the necessary 'adjustments' when we seek to relate the work of Bréal to that of his predecessors and successors: it is for this reason that we are able to decide when they are dealing with what we call lexical semantics (and when they are not), even though they never use the term 'lexical' to qualify either 'semantics' or 'meaning'.

There is, of course, no longer any consensus among linguists, and more particularly among (type-2) theoretical linguists, about the validity of the distinction between the grammar and the lexicon or, granted the validity of the distinction, about where and how it should be drawn. Since the 1970s – in what might be referred to as the post-classical period of (Chomskyan and non-Chomskyan) generative grammar – several rival theories of grammatical and lexical structure have been developed (including Government-Binding Theory, Generalized Phrase Structure Grammar, various kinds of Relational Grammar, and Categorial Grammar) which have, in various ways, challenged the more or less traditional notion of the lexeme (or lexical item) that was formalized in Chomsky's *Aspects* (1965). As we shall see in due course, at least some of these differences are relevant to our topic. For the present, I will continue to operate, as I have been doing tacitly so far, with the view of lexemes that is reflected in the organization of standard reference dictionaries of English and other familiar languages, including notably the Society's Dictionary in both its original edition (with the Supplements) and its more recent, thoroughly revised but not radically reorganized, second edition (*The New Oxford English Dictionary*, 1989).

4. Diachronic and synchronic lexical semantics: (i) 'past work of the Society'

There is a sense in which the Philological Society had a specific and avowed institutionalized interest in diachronic lexical semantics from as early as 1858 or 1860. It was on 12 May 1860 that the *Canones lexicographici* were approved (cf. Coleridge 1860: 305). These were the "Rules for the guidance of the Editor of the Society's Dictionary", which gave effect to the historic decision, on 7 January 1858, that what had originally been intended to be no more than

a supplement to existing dictionaries, such as Johnson (1755) and especially Richardson (1836), should be instead a completely new dictionary and that, like Richardson's (and Liddell and Scott's *Greek-English Lexicon*, 1843), should (as we noted earlier) be 'based on historical principles' and should illustrate the development of the meaning or meanings of each lexeme with dated quotations. Of course, one would not have expected the distinction between the synchronic and the diachronic to have been drawn explicitly (and in these terms) in the pre-Saussurean period. None the less, implicit in the Society's adoption of 'historical principles' and of their development in the *Canones lexicographici*, and more especially in the arguments that led to their adoption, there was, arguably, at least a vague recognition of the importance of drawing a distinction between the synchronic and the diachronic. I will come back to this point. I will also come back to the fact that, in the event, the Society's Dictionary came to be restricted to what later came to be called Standard English.[20] For this too is relevant to our main theme.

There is some dispute as to who should receive the main credit for the decision to produce a comprehensive dictionary of the language (a 'lexicon totius Anglicitatis': cf. Murray 1943: 43–45), rather than a list of 'unregistered words': Richard Chevenix Trench or Frederick James Furnivall (cf. Aarsleff 1967: 231ff.). There is little doubt, however, that it was Furnivall, Secretary of the Society for almost 50 years (1825 -1910), who took over the editorship of the Dictionary when Herbert Coleridge died prematurely in 1861 and, in that capacity, kept the project going by collecting words himself (as he continued to collect them until the end of his life) and by cajoling and bullying others to do the same until eventually, in 1879, Oxford University Press assumed responsibility for publication and James Murray was appointed as Editor (cf. K. M. E. Murray 1977: 140ff.). Furnivall himself was a prodigiously hard worker, disputatious and opinionated in his relations with his collaborators, but, despite the multiplicity and diversity of his other commitments, unquestionably

[20] The proposal that the Dictionary should also include words from non-Standard dialects was rejected by the editorial committee that drafted the final version of the *Canones*.

effective in organizing the work that had to be done (cf. Munro 1911).[21]

However, Furnivall, unlike his French contemporary, Michel Bréal (Secretary of the much younger Société de linguistique de Paris – founded in 1865 – from 1868 until his death in 1910) did not have a professional linguist's or philologist's interest in either lexicography as such or semantics. His own motivating interest in the Dictionary (as also in Early English texts) derived from his view, which he shared with many other members of the Society, including most notably Trench (1851, 1855), that a nation's language and literature (the two, in their view, being inseparable) were part of that nation's history and reflected its institutions and moral values. In this respect, his and his colleagues' attitude may not have been very different from that of many contemporary German and French lexicographers, or indeed from that of Bréal, who in the *Essai*, as we have seen, stressed the social function of language. But what I particularly wish to emphasize here is the fact that Furnivall, the Philological Society's Secretary, was, as far as the study of language (including etymology and lexicography) is concerned, very much an amateur. So too, for the first few decades of its existence, were many, perhaps a majority, of the Society's members.[22] Indeed, the Society's amateurism was a matter of reproach from those members of the Society who did take a more professional, and more genuinely linguistic, view.

Not surprisingly, given the Society's commitment to its Dictionary, over the years there have been many articles in its *Transactions* (and, for the earliest period, *Proceedings*) on lexicography and etymology, including, as was mentioned in section 1, highly professional contributions by scholars of the calibre of Walter Skeat and Ernest Weekley (not to mention the Reports of its successive editors, Coleridge, Murray, Craigie and Onions). As I mentioned earlier, in

[21] Furnivall was a notoriously energetic 'Victorian scholar adventurer' and (apart from much else) a great founder of literary societies, including the Early English Text Society, whose publications provided invaluable materials for the Society's Dictionary, especially for what we now call Middle English (cf. Benzie 1993).

[22] One important measure of the Society's amateurism, as Professor Davies has discussed in her contribution to the Anniversay Symposium, was its failure to give proper attention to the more scientific approach to the study of language that had for some time been prominent in Germany.

the Society's *Canones lexicographici* there was (arguably) at least a vague recognition of the importance of distinguishing the synchronic from the diachronic. I think it is fair to say, however, that no noticeable theoretical developments in either synchronic or diachronic semantics as such are reflected in the articles to which I am referring. In particular, there is no hint of the necessity of making the distinction between the synchronic and the diachronic dependent upon the prior adoption of a structural approach to the description of the vocabulary (and grammar) of a language. Unlike Saussure's *Mémoire,* which, as I said in section 1, can be read (with hindsight) as an exercise in diachronic structural linguistics *avant la lettre*, the Society's *Canones* cannot be said to have been based upon or to have reflected, even vaguely, a structural approach to diachronic lexical semantics (including etymology); and the Society's Dictionary, which in due course issued from the application of the editorial guidelines embodied in the *Canones*, was from the outset, and has remained, wholly atomistic (i.e., non-structuralist).

The Society's concern with lexicography ('on historical principles') and its consequential commitment to an interest in diachronic lexical semantics from the mid-19th century cannot, therefore, be said to have contributed significantly to a better understanding of diachrony and synchrony in lexical semantics. On the other hand, the Dictionary itself, despite its restriction to what we now call Standard English (in so far as this is manifest in written texts), has provided lexical semanticists with an invaluable research tool.

It has been suggested that the Dictionary, under Murray's editorship, actually created Standard English, or rather 'the myth of Standard English' (Harris 1990b). To say this is, in my view, to overstate the case (cf. Lyons 1990). What the Dictionary did was to codify, authoritatively and no doubt to some considerable degree prescriptively, what was already emerging in the mid-19th century (among the educated classes) as a relatively uniform variety of (written) English and enhance its prestige and support its claim to be English *tout court* and unqualified.

Similarly, the Dictionary, by virtue of its acknowedged authority, undoubtedly played an important role in promoting, though it certainly did not create, what I have elsewhere called the etymolo-

gical fallacy and the myth of literal meaning. Taken together, the etymological fallacy and the myth of literal meaning have distorted a good deal of descriptive semantics in the past and, in my view, continue to exert an often unrecognized distorting influence on contemporary semantic theory.

By the etymological fallacy I mean acceptance of the principle that the original meaning of a word is its true, primary, or basic, meaning. Very few linguists, nowadays, and perhaps also very few lexicographers, would subscribe to this principle when it is formulated as baldly as I have just formulated it. It leads to absurd consequences if it is applied consistently and without modification throughout the vocabulary of a language; and it runs counter, of course, to the by now generally accepted principle of the independence (and priority) of synchronic analysis.

It may be worth noting, however, that the historical method applied in Richardson's (1836) dictionary, which, as we have seen, served as a model when the Society's Dictionary was being planned (and especially when it was still being thought of as a supplementary 'Register of unwritten words') was intended by him to give effect to Horne Tooke's (1786) basic lexicographical principle, or axiom, which did indeed identify the real meaning of words with their actual, or hypothesized, original meaning. Tooke's more fanciful, and totally unscientific, etymologies (and his belief that all words could be derived from nominal roots) may have been ridiculed by those members of the Society whose sympathies lay with the emergent, more scientific, discipline of comparative philology.[23] But there can be little doubt that many others were as attracted by Tooke's approach as the general public was.

As to the myth of literal meaning, which may be coupled with the etymological fallacy or be maintained independently of it: this is, in my view, indeed a myth, in the (non-pejorative) cultural-anthropological (or social-anthropological) sense of the term. For present

[23] It should not be forgotten, either, that, although his principles for the reconstruction of earlier forms were unquestionably more soundly based than those of amateur etymologists such as Horne Tooke, Bopp, who believed that all words could be derived from verbal roots, like many comparative philologists of his generation (the generation of the founding fathers), was no less prone to the etymological fallacy.

purposes, I need not go into the historical origins of the notion of literal meaning and the mythical status that it (legitimately) enjoys in the interpretation of legal, scriptural and literary texts, sacred and secular, in certain societies (cf. Lyons 1991c). There are just two points that I wish to emphasize here. The first is that the Society's Dictionary, by virtue of the unique authority it came to acquire, has played as important a role in reinforcing the notion of literal meaning, as it has done in the definition of Standard English. The second is that the traditional notion of literal meaning (which is commonly taken for granted and frequently applied, with reference to authoritative dictionaries of the language, in contexts in which it is not legitimately applicable) is, to say the least, empirically unsound. By this I mean that there is no firm empirical evidence to support the view that all words and phrases in the vocabularies of natural languages have – as a matter of fact – a literal meaning: i.e., an inherent meaning (or, if they are polysemous, a set of related meanings), which is proper to them, constant and determinate (and determinable), and context-independent. It does not follow, of course, that a comparable notion of literal meaning is theoretically unjustifiable. Indeed, whether such a notion of literal meaning is theoretically justified (under a particular idealization of what are known or assumed to be the facts of the matter) is one of the major 'current issues' in lexical semantics. This is why I have mentioned it here in connection with what has been a major part of 'the past work of the Society': its role in the planning and production of its historically-based Dictionary.

Of course, there have been important contributions to descriptive lexical semantics, synchronic and diachronic, in the non-lexicographical part of the Society's 'past work'. However, as far as theoretical lexical semantics is concerned (in the broad sense of 'theoretical'), the only major contribution to be noted here, I think, is the establishment and elaboration of the London School notion of collocational meaning (as part of its more comprehensive contextual theory of semantics). But this, like the rest of Firthian linguistic theory, with the partial exception of phonology, was never formalized (in the present-day sense of this term).

5. Diachronic and synchronic lexical semantics: (ii) 'current issues in theoretical linguistics'

There can be no question of resolving even to one's own satisfaction, still less to that of one's peers, the outstanding theoretically significant 'current issues' relating to diachrony and synchrony in lexical semantics. All that can be done in the present context is to identify some of them and, on particular points of controversy, declare a view. Much of what I have to say in this final section follows from points made in earlier sections and in other previously published works. I will start with, and give pride of place to, the current status of the distinction between the diachronic and synchronic description of languages.

The Saussurean distinction between the synchronic and diachronic points of view, as it came to be formulated in the earlier part of this century and as it is normally explained in the textbooks, is, at first sight, straightforward enough: a synchronic description of a language describes that language at a particular point in time (without regard to preceding or following states of the same language); a diachronic description of the language describes the historical development of that language through time. As experience has shown, however, the theoretical and practical implications of applying this distinction are far from clear. And the distinction itself is by no means as straightforward as it appears at first sight. It may therefore be worthwhile making a few brief general comments before we relate the distinction more particularly to lexical semantics.

It is almost axiomatic in historical linguistics that all so-called natural languages change from one state (état de langue) to another over time, and that they do so necessarily or naturally (in what may or may not be a different sense of 'natural' and 'naturally': cf. Lyons 1991a: 46–72). This proposition may well be true if it is interpreted, loosely, as an empirical generalization. If we press the terms 'naturally' and 'necessarily' and understand the proposition as a whole to have a causal, or nomic, sense, it is obviously false: both Schleicherian organicism and Neogrammarian necessity (restricted though the latter may have been to phonological structure and hedged, as it was, by unexplained, contingent, temporal and geo-

graphical qualifications) are, I take it, no longer positions that would be defended by any professional historical linguist or comparative philologist.

The convenient terminological distinction between synchronic and diachronic description, with which we all operate (if only because it is convenient) must not be understood to imply that time is itself a causal factor in language-change. The passage of time merely allows for the complex interaction of various factors (physiological, social and functional) to bring about what is subsequently describable, from a diachronic or historical point of view, as language-change. This point is perhaps obvious enough nowadays, and it would not be worth mentioning, if it were not for the fact that we have inherited a terminology which, as commonly used, seems to imply the opposite.

The notion of diachronic change – i.e., of transition between successive synchronic states of the same language – makes sense, as an empirical generalization, only if (a) it is applied with respect to language-states that are relatively far removed from one another in time and (b) we recognize the fiction of the homogeneity of language-systems for what it is (cf, Lyons 1981: 24ff., 57–58). If it is assumed that language-change involves the diachronic transformation of what is at any one time a homogeneous, or uniform, system, the whole distinction between the diachronic and the synchronic creates insoluble theoretical pseudo-problems. No language (in the everyday, pre-theoretical, sense of 'language') is ever either uniform or stable; and if we take two diachronically indexed states of a language that are not widely separated in time, we are likely to find that most of the differences between them are also present as synchronic (socioculturally or geographically determined) dialectal variation at both the earlier and the later time. From the microscopic point of view – as distinct from the macroscopic point of view which one normally adopts in historical linguistics – it is impossible to draw a sharp distinction between (diachronic) change and (synchronic) variation. This point is of course widely accepted nowadays, in theory if not always in practice.

One pseudo-problem that has troubled some linguists in the past can be formulated as follows: if the English of today is different

from the English of, let us say, three hundred years ago, they are not the same language; and, if they are not the same language, what justification do we have for calling them by the same name? Instead of saying that the language, more precisely the language-system, has changed, should we not say that one language – one language-system – has been replaced by another? And if we do say this, how do we conceive of the process of replacement? Is it gradual or cataclysmic? The notion of the gradual replacement of one system with another hardly makes sense; and the notion of a cataclysmic, or sudden, replacement of one language-system by another at a particular point in time flies in the face of the appearances. What we have here, of course, is but a particular version of the more general metaphysical paradox of identity through change. The paradox is resolved, in my view, by recognizing that languages – i.e., language-systems (langues in the Saussurean and post-Saussurean sense of 'langue') – have no existence (except possibly as idiolects) in the so-called real world (the world of first-order existence).[24] In so far as they are both theoretically justifiable and of descriptive validity, language-systems, such as English (or even Standard English) are fictional constructs which, if they are theoretically respectable, depend upon a motivated process of abstraction and idealization. This point, properly understood, I take to be uncontroversial; but failure to make it explicit in the past has often led to unnecessary and fruitless argument. Everything that I have to say in this section is to be interpreted in terms of the point that has just been made, coupled with the points made in section 3 relating to theoretical linguistics and linguistic theory.

I would also take the view that, currently and for the foreseeable future, different branches of linguistics should continue to operate, as in effect they do, with differently indexed models of the language-system and that arguments about the ontological status of languages which do not accept this principle of methodological pluralism are based on false premisses (cf. Lyons 1991a: 12–26). But this further point, which is certainly controversial and legitimately debatable,

[24] This is of course Chomsky's view (on what is recognized as one of the major 'current issues in theoretical linguistics'), as it was also Firth's and probably that of other members of the London School, though they have not formulated it in the way that I have done.

need not concern us here. The crucial point is that every model of a language-system is either tacitly or explicitly indexed (in the sense in which the formal semanticist's models, or possible worlds, are indexed by a so-called point of reference) and that the synchronic/diachronic dimension of variation which supplies us with a temporal index is only one of several dimensions of variation – geographical, stylistic, social, etc. – which provide us either with other indices or with other components of a composite index. This is just as true of the (unformalized and inexplicitly indexed) models of Standard English whose structure is described (and codified), whether synchronically or diachronically, in traditional grammars and dictionaries (each such model being a model of a more or less different language) as it is of the more or less explicitly indexed models of the sociolinguist or stylistician.

Saussure's distinction of synchronic and diachronic description is usually taken by structuralists to imply that historical considerations are irrelevant to an understanding of how a language operates at any particular time. It is also commonly held to imply that, whereas synchronic description is independent of diachronic, diachronic description presupposes the prior synchronic analysis of the successive states through which languages have passed in the course of their historical development. As far as lexical semantics is concerned, this implies that the older, meaning of a word has no privileged status and cannot be properly described, on those grounds alone, as the correct or primary meaning. To hold the contrary view, as we have noted in the preceding section, is to fall victim to the etymological fallacy.

The principle of the independence, and methodological priority of synchronic description has long been widely accepted by linguists. It is not clear, however, that some of the concepts with which semanticists and lexicographers operate, such as the concepts of homonymy and polysemy or literal meaning, do not covertly reintroduce diachrony into what purports to be a purely synchronic analysis of the senses of polysemous lexemes.

The question of literal meaning was mentioned in the previous section. Literal meaning is defined differently in different disciplines and for different purposes. For example, the notion of literal meaning used in legal interpretation or scriptural exegesis is not

necessarily the same as that used by lexicographers. Still less is it the same as the notion of context-free literal meaning that is commonly invoked in present-day (type-1 and type-2) linguistic theory as the basis for the distinction between semantics (in the narrower sense) and pragmatics. It requires but little knowledge of the work that has been done, over the years, in descriptive lexicology – or indeed in the branch of applied lexical semantics known as lexicography – to see that this notion of context-free literal meaning is empirically suspect and should not be taken for granted. This is all that needs to be said, in the present connection, about one of the 'current [and contentious] issues in theoretical linguistics' to which the past work of the Society is relevant.

As far as homonymy and polysemy are concerned, the possibility that both the 'one-word-or-two/several-words?' issue and 'which-is-the-basic/primary-meaning?' issue are being resolved, explicitly or tacitly, 'on historical principles', rather than synchronically, has long been a commonplace of theoretical discussion. There is little that needs to be said here except that these two issues are still with us; and that, in my view at least, they are to be resolved, not by empirical arguments about the facts of the matter, but by theoretical fiat, in the light of the explicit recognition by descriptive semanticists that the synchronic language-system is a fictional construct (motivated, of course, in particular instances of indeterminacy by considerations of 'rough justice': cf. Householder 1957; Lyons 1991a: 107–109).

As we saw in section 2, Bréal was by no means the first scholar to set out to classify various kinds of change of lexical meaning which gave rise to what would usually be regarded both by native speakers and descriptive semanticists and lexicographers as polysemy, but which may in due course be described synchronically as homonymy; and many of the principles that he called 'laws' were recognizably identifiable with traditional rhetorical tropes and figures of speech or such processes as generalization, specialization, contextual contamination. The same is true of the many other taxonomic schemes produced by his successors up to and beyond the classic culminating work of Gustav Stern in 1931 (cf. Cremona, 1959). It is not clear that we can significantly improve on such work in diachronic lexical semantics except (a) by improving the communal database and

explicitly indexing the dimensions of variation and (b) by recognizing that there is no such thing in the real world as 'une langue une' (in the societal sense of 'langue') and taking full account of (multidimensional) synchronic variation, operating in conjunction with the psychological and contextual factors that Bréal and his successors saw as the principal determinants, to produce what is seen retrospectively as diachronic change.

There is much else that could be said no doubt, about the relevance of past work of the Philological Society to 'current issues in theoretical linguistics', in the field of lexical semantics. Some of this is implicit in what I have said, en passant, in the preceding sections or in recent and forthcoming publications.

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