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THE STRUCTURE OF LINGUISTIC REVOLUTIONS*

JOHN E. JOSEPH
The University of Hong Kong

> Let us assume that crises are a necessary precondition for the emergence of novel theories ...
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> Thomas S. Kuhn (1962)
>
> You say you want a revolution ...
>
> John Lennon & Paul McCartney (1968)

Among the questions never asked about modern linguistics is where and how it acquired its insatiable taste for revolution. Rare is the linguist who does not consider himself part of a linguistic revolution accomplished in recent memory, or in progress, or both. Alternatively she may be part of a past revolution that got it right, then was superseded by a false counter-revolution that will soon give way, with the old order being restored — a revolution in the true, 360° sense (or a second coming, for the SIL crowd). Nothing is so brash as a new Ph.D. sure that his revolution has at last broken through the darkness, while few things are sadder than a linguist jaded by the realization that her revolution will never live up to the promises made in youthful exuberance.

We have somehow become addicted to it. Revolution is the master plot for linguistic history, what gives sense to our work and careers, what makes it worth getting out of bed in the morning. Of course this condition is not exclusive to linguists, but pervades academic life, and real life too, though

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* On the occasion of: Randy Allen Harris, *The Linguistics Wars* (Oxford and New York: Oxford University Press, 1993), ix + 356 pp.; and Stephen O. Murray, *Theory Groups and the Study of Language in North America: A social history* (Amsterdam and Philadelphia: John Benjamins, 1994), xix + 594 pp. I am grateful to Konrad Koerner, Stephen Matthews, and John Stonham for helpful comments on an earlier draft.

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political revolution is *démodé* in the post-Thatcher-Reagan-Deng-Yeltsin-Chirac era. Academia was not always thus. For many centuries, researchers and teachers alike saw their work as building steadily upon a cumulative tradition. They did not wait breathlessly from day to day for the foundational error of that tradition to be discovered and the bright new path toward truth to be revealed. But by the late 18th century that had begun to change, and not long into the 19th, scientific revolution was itself institutionalized as the normal expectation. Perhaps the French Revolution brought this on, or the Industrial Revolution, but more likely they proceeded from the same underlying cause, whatever it may have been.

The book upon whose title I have calqued my own, *The Structure of Scientific Revolutions*, does not allow for this kind of permanent ideology of revolution within a field or an entire culture. Yet its author, Thomas S. Kuhn (b.1922), was from 1979 until his retirement in 1991 a member of the Department of Linguistics and Philosophy at MIT, the main setting of Randy Allen Harris' *The Linguistics Wars* and a key locus for Stephen O. Murray's *Theory Groups and the Study of Language in North America.* The department is unique in having fostered over the last forty years not one, but easily a dozen large-scale or small-scale linguistic revolutions. Most of them have been revolutions against earlier revolutions spawned there as well. More amazingly still, many of them have been led by the same man, revolting against his own program of a few years before: Noam Chomsky, Serial Revolutionary. Kuhn never took advantage of his strategic position within Revolution Central to analyze the recent history of linguistics in relation to his theory, or vice-versa.[1] If he was saving it as a project for his retirement, he will find that much of the dirty work has now been done for him.

There is still plenty left for him to handle, though, since the books in question take off in two different directions from his seminal work. Murray focuses on the sociological aspects of linguistic revolutions, Harris on the rhetorical ones. For Kuhn, revolutions are essentially intellectual events, with a crucial social and linguistic dimension. For Murray, they are essentially

[1] This is actually surprising, given that the manuscript of *The Structure of Scientific Revolutions*, solicited for the logical positivist book series International Encyclopedia of Unified Science, passed through the hands of two important philosophers of language, Rudolf Carnap (see Reisch 1991, Earman 1993) and Charles Morris (see Kuhn 1970: viii), and that Kuhn acknowledges "B. L. Whorf's speculations about the effect of language on world view" as one of a handful of influences (including also Piaget and Quine) to which "I am indebted [...] in more ways than I can now reconstruct or evaluate" (1970: vi-vii).

social events with a crucial intellectual dimension, plus a rhetorical one that acts as a smokescreen. For Harris, they are essentially rhetorical events (in a sense to be discussed further on), with a crucial social dimension, plus an intellectual dimension that, while by no means insignificant, is itself ultimately a rhetorical and social construct. The way in which revolutions are conceived of and analyzed is at the heart of the two very different approaches to the history of linguistics which Murray and Harris represent.

Even apart from this, their books are not exactly comparable. Harris focuses on a single group and a limited period, while Murray's lens takes a wider angle. Harris writes for a general audience, Murray for an academic one. But both concentrate on with what they consider to be the most important and interesting developments in modern linguistics. Both, too, are iconoclasts. Neither bows to the sacred cows of the discipline, and if those cows are found to be not so sacred after all, each is ready to milk them for all the controversy they are worth. Their historical methodologies also have similarities: both rely on interviews and solicited testimony, Harris almost exclusively so. Murray adds to this an impressive amount of archival digging, and many years of reflection and personal insight. Not that Harris is lacking in reflectiveness or insight, but Murray has a decade's head start.

Murray's forte is taking the reader into the great linguists' kitchens, by means of their letters, personal recollections of their students, and whatever other data he can conjure up — institutional records, book sales, charts showing numbers of articles published in prestigious journals, and so on. Even for Sapir, the one American linguist on whom we have a first-rate biography (Darnell 1990), Murray's chapter adds a significant dimension to our understanding of the man and his work. No one has told the story of how sociolinguistics developed in the U.S. with the wealth of detail and personal insight that Murray brings to it; in that area alone his book will endure as a primary source for as long as American linguistic history is studied. This is not to say that all sociolinguists, ethnographers of communication, discourse analysts et al. will see eye to eye with his account (though it is nowhere near as potentially controversial as his handling of the generativists). But he was the only person interviewing the founders of the field in the 1970s, when their memories were still fresh, just as he was one of the rare people interviewing the surviving students of Sapir, few of whom are now left.

At the same time, Murray's book contains something to anger just about everyone. Devout Chomskyans will be outraged, as will devout Whitneyans,

Sapirians, Whorfians, Labovians, and not a few of the surviving neo-Bloomfieldians — all to Murray's delight, one suspects. Followers of other linguistic gurus will be disappointed by the lack of attention paid to them. Even readers with no theoretical commitments may be put off by the fact that the author's lens seems to careen unsystematically in and out of focus, scrutinizing one area in the most minute detail while ignoring another completely, with no attempt at a rationale. But for anyone with a keen and unbiased interest in the history of linguistics in the 20th century, none of this should disguise the fact that Murray has produced a work of real importance. In some areas, such as the careers of Sapir and some of his students, Chomsky's relations with the neo-Bloomfieldians, and the early history of sociolinguistics, it is unparalleled.

The book's title should not be taken to imply that it treats "the study of language in North America" with complete thoroughness. The particular areas just mentioned receive minute coverage, and that is already a great deal. A wide range of other figures and developments are covered *en passant*, including other students of Boas, the histories of the Linguistic Institute and the Linguistic Atlas of the U.S., Whorf, Dorothy Lee, European structuralism in America after W.W. II, and developments in Chomskyan linguistics in the 1960s and 1970s. Yet to take one striking example, no serious consideration is given to historical linguistics (except for reconstruction of American Indian languages), even though it was the dominant mode in American linguistics for the first half of this century. True, it represents a European heritage and was the main obstacle that an anthropologically based, synchronically oriented American linguistics had to overcome. Still, most of the major figures in 20th-century American linguistics (Sapir, Bloomfield, Chomsky, Labov, Greenberg) came out of an originally non-Americanist historical linguistic orientation, bringing its methodology with them essentially intact. Indeed Labov and Greenberg have never given up that orientation.[2] I am not arguing that a book like this one must include a full survey of historical linguistics, only that more care might have been taken to explain that a complete picture of American linguistics will not emerge from these pages, only a very thorough understanding of those aspects which are treated here.

[2] The ignoring of historical linguistics may be a hangover from the 1983 book, where Dell Hymes, who came from an anthropological background and never developed a significant interest in historical linguistics, emerges as the spiritual heir to Sapir and the true center of the post-1950 linguistic universe. In the optic of that book, it is not surprising that historical linguistics is neglected as much as Chomsky (the anti-Hymes) is vilified (see below).

As the title of the 1983 book indicates, Murray cast it as a case study of a theory of group formation in social sciences. It was an attempt to further Kuhn's (1970[1962]) model of scientific revolutions, and in so doing to move beyond it. For Kuhn, in Murray's view, "the triumph of adherents to one theory is a social process, not the automatic diffusion of superior ideas" (4). However Kuhn gave limited attention to the nature of that social process.[3] The late 1960s and 1970s saw attempts to formalize and even quantify its study, for instance by Crane (1972), Mullins (1973), and most importantly for Murray's own work, Griffith & Miller (1970) and Griffith & Mullins (1972). The Griffith-Mullins theory holds, in Murray's words, that "all coherent scientific groups have three factors: good ideas, intellectual leadership and organizational leadership [...] Without all three factors, no scientific group will emerge. All are necessary, no one is sufficient" (22-23). The early versions of Murray's study were offered as a test case of this model, rather than a history of American linguistics as such.

For Murray, the academic politics of linguistics is not a sideshow to the development of scientific ideas. It is the main event. In Kuhn's model the sociology of scientific revolutions is (arguably) still driven by ideas, which cycle inexorably from paradigm to anomaly to crisis, and then, via a change in world view, to resolution.[4] A new paradigm emerges, and the revolution as social process will be grounded in it. Murray reverses figure and ground. He agrees that good ideas, ones which solve earlier puzzles and, just as crucially, suggest new paths for research, have indeed been an essential element in the formation of successful linguistic 'schools'. But he maintains that academic leadership and organizational leadership have proven to be every bit as important. If any of these three elements is missing, Murray says, no linguistic

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3 In the Postscript (dated 1969) to the second edition (1970:176ff.), Kuhn gives brief consideration to the work of Crane, Mullins and other sociologists of science, and states: "If this book were being rewritten, it would [...] open with a discussion of the community structure of science, a topic that has recently become a significant subject of sociological research and that historians of science are also beginning to take seriously" (p.176). He adds however that "the intuitive notion of community [...] underlies much in the earlier chapters of this book" (*ibid.*).

4 It is in Kuhn's discussion of "revolutions as changes of world view" (1970:111-135), and particularly of how world view is embodied in language, that the influence of Whorf can be detected (see n.3 above). Yet Whorf is not cited in this section, and the insight could have come quite as well from the logical positivists or the Cambridge analytical philosophers (see further Joseph forthcoming). Kuhn has recently denied, however, that his approach to the history of science was directly influenced by Carnap, noting that only many years after the fact did he appreciate how closely Carnap had anticipated his model (see Kuhn 1993:313ff.).

revolution, however small, can succeed. Revolutions, then, are two parts sociological and one part intellectual.

That, at least, is the case for *genuine* revolutions. For what sets Murray most apart from both Kuhn and Harris is his desire, and the purported ability of his structural sociological model, to distinguish the 'mere' rhetoric of revolution from 'real' revolution on the conceptual and methodological levels.[5] It is not clear that there has ever been a real revolutionary American linguist in Murray's eyes. Certainly all those before Boas fall wide of the mark, including Whitney, about whom Murray writes:

> In some sense a revolutionary in international linguistics, at home, in only-slightly-institutionalized Americanist linguistics, Whitney was an orthodox follower of the Duponceau tradition [...] Whitney's revolutionary rhetoric against the neogrammarians' elders was not accompanied by a positive theory of language change. (44-46)

This view becomes possible when one equates American with Americanist linguistics and downplays the institutionalization of German-style historical linguistics in America. Whitney played a key role in the latter process, which laid the template for 20th-century developments in the wake of Boas. Murray does confer the revolutionary mantle upon Boas, but with some reservations where linguistic methodology is concerned. While "Boas clearly broke with the ruling evolutionary paradigm in anthropology" (p.64), he was disinclined toward theory, preferring that his students gather large amounts of data without drawing theoretical conclusions from them.

> There can be little dispute that Boas was both an organizational and an intellectual leader; although he was not a 'theorist' in the usual sense (rather more an anti-theorist), there was a Boasian paradigm. It involved replacing unilinear schemes of evolution with explanation by diffusion through culture contact for observed similarities among cultures. (63)

Boas' methodology removed language and other objects of anthropological study from their cultural context, and it was left for his students to move the paradigm toward a more holistic approach. In linguistics, the student most

[5] In a similar spirit, Koerner (1989) appears to confine *revolution* to just those cases where no continuity with the existing conceptual and methodological framework can be found. He therefore questions whether a Chomskyan revolution can be said to have occurred. In Joseph (1991) I argue that not only does this go against the common usage of the term, but that under these conditions it is doubtful that any revolution, scientific or political, could ever be said to have taken place. Most revolutions are essentially rhetorical, with the substantive change being one of personnel — who is in charge of the government, who defines the mainstream. What Koerner objects to is the idea, commonplace in some quarters, that linguistics 'began' when Noam Chomsky revolutionized the study of language in 1957. Of course no reader of this journal could take that idea seriously. But I do not see the point of trying to dispel it by imposing upon the word *revolution* a definition that it does not normally take.

responsible for that shift was Sapir, who unlike Boas "took native self-understanding seriously" (p.93).[6] For Murray, Sapir's critical contributions to linguistics took place from about 1909 until the early 1920s, when he became a 'star' and began publishing far more theoretical essays than fieldwork-based studies. Of the remainder of Sapir's career at Chicago and Yale, Murray is witheringly critical:

> As an internationally-recognized social scientist in the late 1920s and 1930s [...] he devoted even more time to failed attempts to build institutions with himself as the king of the roost [... H]e showed a spectacular inability to distinguish real possibilities from fantasies in which the world accorded him the high valuation he viewed as obviously his due. (95)
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> Sapir found every position he occupied too demanding. Although he complained of too little time to do his own real work, he took every opportunity to distract himself with organizational activities rather than writing up linguistic materials or undertaking ethnographic research. (102)
>
> He was undercut so often, that it is hard not to wonder whether he was self-defeating [...] I do not think that the increasing failure to prepare data for publication, to write planned books and monographs, to lay out the case for his synchronic or diachronic models, and the failure to produce exemplars of analyzing personality, intra-cultural variability, or any language studied after 1910 can be attributed entirely to external enemies or to bad luck. (109-110)

One wonders why Murray feels so compelled to harp on Sapir's uncompleted projects rather than the vast amount that he did accomplish in his 55 years, the last two in failing health. I should think that his 1929 classification of North American Indian languages and his 1933 "Réalité psychologique des phonèmes" alone would save Sapir from any charge of resting on his laurels, but I also believe that Murray underrates the significance of Sapir's work on an international auxiliary language (recently re-evaluated by Falk 1995) and his other attempts to extend the boundaries of linguistics beyond what less far-reaching minds found comfortable.[7]

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[6] However, as John Stonham has pointed out to me, Boas' language materials incorporate more implicit native analysis than meets the eye.

[7] To my surprise, linguists with whom I have discussed Murray's book object far more strongly to his criticisms of Sapir than to his much less temperate ones of Chomsky. It may be that, whereas Chomsky has been a figure of controversy for his entire career, and Bloomfield is attackable because Chomsky made him so, Sapir has attained the status of a sacred icon who is above criticism. Such veneration is something historians of linguistics have a duty to rationalize, and if that is Murray's aim in harping on Sapir's failures, there may indeed be some need for it. The facts Murray reports are well attested, not only in his book but in Darnell (1990), and Joseph (forthcoming) discusses some of the books Sapir planned or even contracted to write but never produced. It is only the harshness of Murray's interpretation of these facts that I am questioning.

Sapir's 'school' did not survive him in the way that Boas' or Bloomfield's would do: "After his death, ties among those who saw themselves as 'Sapir's students' were not particularly maintained. There was no successor as an organizational leader and many of those proud to label themselves 'Sapir's students' did not pursue 'Sapirian' linguistics" (109). While Murray does not explicitly blame Sapir for this, the general tone of failure gives the impression that here was another task he left undone in his mature years. If that is the intent, it is not entirely fair, given Sapir's premature death, Whorf's even more premature death, the political persecution of Sapir's protégé Morris Swadesh in the McCarthy era (recounted by Murray, 206-208), and countless other 'variables' that Murray's sociological model is ill-equipped to handle.

Although Bloomfield receives a more sympathetic personal portrayal, Murray does not consider him to have been an original thinker. His organizational leadership had its great success in the founding of the Linguistic Society of America rather than of any academic program. Murray points out that in his Yale years, Bloomfield directed only one doctoral thesis, Isidore Dyen's (133). Rather, his importance lay in "his codification of general, descriptive methods" (132).

> Without advancing claims for novelty or priority of linguistic analysis, Leonard Bloomfield synthesized the best available knowledge on language structure and language change in what became the classic textbook for both beginning and advanced students of linguistics for several scientific generations. (134)

Murray takes every opportunity to contrast Bloomfield's personal humility with Sapir's sense of self-importance; but it is worth remembering that Bloomfield did not *have* to advance claims for the priority of linguistic analysis because Sapir was out doing that work, not just for himself, but for all linguists.

The neo-Bloomfieldian school attained mainstream status, in Murray's view, not because of its founder's good ideas, but because of effective leadership. From the 1920s on Bloomfield enjoyed the good offices of Edgar Sturtevant, Charles C. Fries, George Melville Bolling, and Hans Kurath promoting his initiatives and his work; and his later followers included such first-rate organizational leaders as J Milton Cowan, Bernard Bloch, Archibald Hill, and Henry Lee Smith, Jr. Moreover, there was a second generation of intellectual leadership ready to take over upon (or even before) the master's demise — Zellig Harris, Charles Hockett, George Trager, and Kenneth Lee

Pike — who, even while taking 'neo-Bloomfieldianism' in quite different directions, never lost their essential group identity.

When he comes to both Chomsky and Labov, Murray's main concern is to show that their revolutionary rhetoric is not supported on the intellectual and methodological level, where one finds much more continuity with their predecessors than their claims of radical originality would suggest. Murray does not attempt to disguise the fact that everything he is against in the study of language is embodied in the person of Chomsky, whom he compares explicitly with Stalin and Mao (p.445):

> Cohort after cohort of MIT students emulates Chomsky's contemptuous rhetoric dismissing those who have not accepted his latest revelations. The contents change, the style remains the same. Constant purges, persistent rhetoric about a sacred 'revolution' accompanied by persistent misgovernment as judged by any external standards recall Stalin (see Murray 1989: 155-160). The 'Cultural Revolution' of Mao Zedong (another Stalinist) is an even better political analog for the Chomskian regime: the aging dictator questions the zeal and loyalty of his former followers, and encouraging [*sic*] the ferocity of the young, who gladly challenge 'traditions' of all ages.

No one can accuse Murray of not summoning up vivid images. Hitler presumably is not an apt counterpoint since he was relatively consistent in his program and his allegiances. No doubt someone who considers him or herself a victim of the 'regime' might think that in the grand scheme of things this kind of guerilla discourse (which Murray condemns in the Chomskyans themselves, especially the young Paul Postal) is the only way to make a dent in the received legends of Chomsky, dispensed so effectively by him and Newmeyer (1986) as to be taken for hard fact by many in the field. But for a historian to equate a scholar whose tactics he dislikes with the great mass murderers of the century hardly inspires confidence in his detachment and objectivity.[8]

Still, the point is not to exchange one received history for another, and it is to be hoped that even such a grotesquery as the preceding quotation will not keep open-minded readers from seeing that there may indeed be another side to the golden legends. Chomskynauts, like true believers everywhere, have

[8] Harris opens his section on "The Extended Standard Theory" (p.144) with a quotation from Mao on tactics for confusing the enemy; unlike Murray he does not make the comparison with Chomsky explicit. He also quotes the following opinion from the collective author Bennison Gray (1974: 4): "Chomsky sounds quite Trotskyish compared to the logico-linguistic Stalinists of the younger generation", with James McCawley (of all people) cited specifically as one of the Stalinists (Harris, p.224).

minds hermetically sealed against any such demythification. But a new generation of the disaffected has arisen among partisans of GB who resent Chomsky's abandoning it for minimalism, and they should be prime candidates for Murrayan re-education. Yet the book would be stronger if Murray treated Chomsky as he does Sapir and Bloomfield, as neither a demon nor an "angel" (Jay Keyser's term for Chomsky, in Harris, p.77), but a man. Then it would be less tempting to read Murray the fearless revisionist historian as Murray the *enfant terrible* with an axe to grind.

The biggest bombshell Murray has produced is his evidence (first presented in Murray 1980) refuting the accounts of Chomsky's early years, particularly his reception by the neo-Bloomfieldian linguistics establishment. In interviews with Chomsky, and by Newmeyer's account, the young Chomsky was an isolated figure, beset on every side by resistance to his ideas from his elders. In particular, his articles were rejected by the linguistics journals of the time, and he was unable to convince publishers to accept his lengthy *Logical Structure of Linguistic Theory* (*LSLT*, eventually published as Chomsky 1975). Murray cites letters from Bernard Bloch, the editor of *Language*, soliciting articles from Chomsky, together with other indications that, through the early 1960s, the neo-Bloomfieldians perceived Chomsky as one of their own, and if anything promoted his work, as when Archibald Hill invited Chomsky to participate in the Third (1958) and Fourth (1959) Texas Conferences on Problems of Linguistic Analysis in English. Murray also shows that two different publishers were interested in publishing *LSLT* in the 1950s, and that the only traceable rejection of an article submission by Chomsky came not from a neo-Bloomfieldian journal, but from *Word*, edited by a non-Bloomfieldian, André Martinet.[9]

Like most people, Chomsky is too close to his own history to see it objectively. Moreover, it is clear from Harris' interviews with many of Chomsky's former close associates that he takes anyone presenting counter-evidence to his own version of his life as engaging in an attack on him, his work, and everything he stands for. This is unfortunate. For many of us who have never been either Chomskyans or anti-Chomskyans, Chomsky is history. His own accounts of his relation to the neo-Bloomfieldians read like classic hero myths, key elements of which include the hero's being self-generated and overcoming obstacles placed in his path. It would take nothing away from

[9] On Martinet's recent interpretation of Chomskyan linguistics as part of Jewish cultural universalism, see Joseph (1994a).

Chomsky's greatness if he tried coming to grips with Murray's account, not as an attack, but as a potential source of insight into the workings of his own mind. But I wouldn't hold my breath.

The title of this incarnation of Murray's book reflects a lessened concern with the sociological model as guarantor of his objectivity toward the various linguistic theories he chronicles and assesses. Following an important development in postmodernist anthropological writing, he does not strive for a fiction of objectivity, but inserts himself directly into the study. Thus he deals at some length with his own training in "third generation Berkeley sociolinguistics" (447-471). He provides useful primary data, yet this is one of the less interesting sections of the book. It is another case — and this is the other side of the postmodernist argument — of closeness distorting perspective.[10] Still, the chapter allows the reader to see where Murray is coming from in his own view, and to judge his perceptions accordingly.[11] That, along with an "appendix on methods" (491-502), contributes to the sense of his trustworthiness as a historian, in spite of his transparent biases.

Murray's concluding chapter begins (p.479) by quoting Clifford Geertz to the effect that there is no ascent to truth without descent to cases. But in fact the descent to cases, if done properly, *is itself* the ascent to truth. He goes on to say that "Unfortunately, cases do not add themselves up. Having stirred up more than a little dust in some valleys of historical detail, I want to reascend the mountains of sociological abstraction" (*ibid.*). But cases *do* add themselves up, when presented to intelligent readers in a historical analysis as insightful as the one Murray provides. Murray's Mountain is one of historical detail painstakingly and thoughtfully marshalled, that overshadows his socio-

[10] It is certainly true that the sociologist or anthropologist is always part of the scene he or she is studying, so that the attempt at absolute self-negation can only produce a false objectivity that distorts reality. The analyst should acknowledge his or her presence and try to deal with it openly and honestly. But it is difficult to bring this off well. With no disrespect intended to either Hockett or Murray, Murray on Berkeley sociolinguistics in the early 1980s sounds more like Hockett on the neo-Bloomfieldians than like Murray on the neo-Bloomfieldians.

[11] The 1983 version made Murray seem more of a detached outsider than he actually was. The new account emphasizes his own postdoctoral training in 1980-82 at Berkeley's Language Behavior Research Laboratory principally under John J. Gumperz, which helps explain, up to a point, why Hymes emerged as the hero in the older book (much as James McCawley does for Harris, by virtue of being the least flawed in a cast of far from perfect beings). Hymes was an organizational and intellectual mainstay of all the various sociolinguistics approaches not committed to Labovian statistics and rules (and therefore ultimately in bed with the generativists). In the new book, all anthropological and non-Labovian sociolinguistic roads no longer lead to Hymes, and Murray even says that the group with which he worked at Berkeley assumed that Hymes was unsympathetic to their work.

logical abstractions — which, it must be said, are based on a model that is now a quarter-century old and no longer in widespread use in the history and philosophy of science.[12] Still, the sociological framework is less obtrusive here than in the earlier book, and many readers will find it instructive to be led through the methodology behind the study.

Unlike Murray, Harris is not explicitly out to examine the structure of linguistic revolutions, but to chronicle and analyze a particular series of revolutions, together with their received history. His book originated as his doctoral thesis in a rhetoric program (Harris 1990), so it is not surprising that he takes up the rhetorical strand in Kuhn's thought as seriously as he does; but it is also the case that under the influence of Richard Rorty and others, a rhetorically-based approach to the history of academic disciplines has become dominant in the U.S. during the last ten years. Quite apart from its approach to history, Harris' book is itself a model of good rhetoric in its subtle way of allowing cases to "add themselves up" (or appearing to do so) without exposing all the nuts and bolts. Harris' grand conclusions — largely identical to Murray's (though some differences are noted below) — emanate deductively from the cases with more persuasive power than when the analyst bashes us over the head with them.

Unfortunately, when dealing with linguistics before Chomsky, Harris can barely hold his own. Most readers of this journal will probably like the book better if they do not read anything before p.28, starting with the sentence "Lo, in the east, Chomsky arose" (variants of which recur as a leitmotif through the chapters that follow). The "Grand Prix review of linguistic history" (p.16) which precedes is intended to orient readers with no background in linguistics — and, no doubt, many who have one. But anyone with a thorough knowledge of the discipline's history risks being left breathless, not just at the speed with which Harris careens around the track, but at the number of times historical accuracy goes crashing into the wall.[13]

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[12] The heyday of the sociological approach in these fields ended in the early to mid-1980s, as various strands of the 'rhetoric of science' came to define the mainstream. Murray virtually ignores these developments, leaving his methodology in something of a time warp. See however Fuller (1993:xii) on recent attempts to resuscitate the sociology of science by including it within "the emerging interdisciplinary complex known as Science and Technology Studies (STS)".

[13] I consider it to be wrong, questionable, or at least in need of supporting evidence when we are told axiomatically and without qualification that, with regard to the ancient period, "the Stoics found [that] the links between signifier and signifed are the product of convention,

Were the inaccuracies scattered across many chapters, their cumulative effect would be negligible. But they are packed into seven pages, and are followed by a badly botched account of behaviorism and logical positivism. Harris uses the terms *positivism* and *logical positivism* interchangeably, apparently unaware that they designate two entirely different movements with very little in common. The confusion leads him to state that "Banishing everything not directly verifiable, for the behaviorists and for Bloomfield, was the way to be a science. They knew this because the logical positivists told them so" (25). Of course there are interesting connections between behaviorism and logical positivism, but the former movement preceded the latter, and there is no evidence of the sort of influence here claimed.[14] This kind of thing makes one appreciate Murray's historical precision all the more, for whatever his biases, he does not get his facts muddled, even when discussing matters far removed from his central focus.

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consensus, and reason" (p.12 — what about the Sophists?), that the analogy-anomaly dispute "subsided with the discovery of more regularities in language, such as the critical distinction between inflectional morphemes and the semantically heavier, more idiosyncratic, derivational morphemes, and with the general neglect of meaning" (13); moving up to the late medieval period, that "The scholastics [...] were driven from the intellectual scene by the increased concern for empirical research and mathematical modeling" (14); in the 19th century, that "historical linguists in the nineteenth century were after the one 'pure' Adamic language, spoken from Eden to the collapse of the Nimrod's tower" (12), that Humboldt "was one of the few nineteenth-century scholars of language not primarily concerned with its historical aspects" (19), that the "self-styled neogrammarians" attacked the comparativists "toward the end of the century, in a power shift that many linguists regard as a 'false revolution' — in fact, as the prototypical false revolution" (15), that the most famous neogrammarians were Karl Brugmann and Hermann Paul (15), that Whitney "was trained among the German neogrammarians" (19); and in the 20th century, that Saussure "was strongly influenced by Durkheim, and his overwhelming tendency is to face toward sociology", and that not only are Sapir and Bloomfield the most important figures in the development of American structuralism, but that "given the subsequent direction of the field, the most important of these two [*sic*], far and away, is Bloomfield" (19).

[14] Harris goes on: "Positivism has ancient roots, reaching back to the Epicureans [a surprising choice: JEJ] and beyond, by way of the powerful British thinkers of the eighteenth century, Locke, Berkeley, and Hume [but not of Bacon before them, or Comte after them?: JEJ]; it is an articulation, somewhat extreme, of the grand philosophical tradition which says that all knowledge comes from the senses — empiricism. But its formal beginnings are with the famous *Wiener Kreiss* [*sic* — not famous enough to avoid misspelling!: JEJ] of twenties Vienna, a circle of thoroughly empirical philosophers [an empirically false observation: JEJ] who took Wittgenstein's insufferably titled *Tractatus Logico-Philosophicus* (1961 [1921]) [— no, *brilliantly* titled: "logical-philosophical treatise" is a simple 'picture' of what it designates, subtly engaging one of the book's principal themes: JEJ] as their defining document" (p.25).

Otherwise Harris' account of behaviorism is okay, though he does turn Albert Weiss into "Alfred" (24, 339),[15] and fails to consider what the 'mentalist' alternative would have been for linguists in the 1920s. Psychology in its academically entrenched modes, philosophical (à la James) and experimental (à la Wundt), was swept up in the enormous success of psychoanalysis, which had captured the popular imagination and which did not see any natural limits to what it could potentially explain. On the other hand, Gestalt psychology, which was stealing the thunder of earlier 'structural' inquiry, was in its infancy and its applications to language were not obvious, even to people like Sapir and Whorf who were interested in developing them.[16] Also in its infancy was German neo-Humboldtian mentalist linguistics, developed for example by Weisgerber, whose line on mind, race, and culture was enough to the liking of Hitler's government as to earn him a high administrative post during W.W. II (see further Falk & Joseph 1996). When Bloomfield referred to the mentalism of his day as "mystical", he was not using snide rhetoric to take a lofty stand on the mind-body problem, but passing a matter-of-fact judgment on Freud, Jung, and the neo-Humboldtians that even Chomsky in his most anti-Bloomfieldian mode would be hard put to contradict.[17]

With the appearance of Chomsky, Harris is on much firmer ground, and given what the book is about, that is what really matters. Not a little of the firmness he owes to Murray, who is cited many times in the book (though, oddly, omitted from its index). Harris seems to have added no archival research to the vast amount that Murray had done. But he has successfully solicited a tremendous amount of new information from individuals involved in the development of transformational-generative grammar from its beginnings through the late 1970s. The persons in question, sensing that Harris had done his homework, understood the major issues involved, and was prepared to be sympathetic to both sides in the generative vs. interpretative semantics debate, seem to have responded to him with great candor. They supply vivid and personal recollections that not only make the book fascinating to read, but

[15] He also turns Benveniste into "Beneviste" (62), Boole into "Booles" (117), Di Pietro into "DiPeitro" (317, 339) and even Lakoff into "Ladoff" (113).

[16] On the development of Gestalt psychology and its relations with both behaviorism and the 'cognitive revolution', see David J. Murray (1995).

[17] Accounts of Bloomfield's behaviorism that start from the perspective of Chomsky's (1959) review of Skinner inevitably compress history in such a way that Bloomfield ends up seeming to be a Skinnerian. Murray (231-233) shows that the neo-Bloomfieldians did not read Chomsky on Skinner as in any way an assault on *their* behaviorism — quite the contrary.

give a human dimension to a tale that has long since passed over to the realm of myth.

I said earlier that Murray's account of Chomsky fails to treat him as a man. In *The Linguistics Wars* he appears as all too human. Harris has an awesome ability to peer into the soul even of someone who for his entire public life has kept successfully behind a professional and political facade. Harris' achievement is to have understood that the facade itself — together with the work, the career, the scattered autobiographical comments, and the archival data — is the key to the person beneath it. He certainly recognizes that Chomsky is no ordinary mortal, but one who fosters both loyalty and opprobrium with unusual intensity, even serially in the same individuals. Such was the case with the first-generation generative semanticists, all of whom began as Chomsky's acolytes. But again, in his reaction to their defections, the man behind the mask is very human indeed.

The core of the 'war' story has been published in this journal (Harris 1993). The publication in 1965 of *Aspects of the Theory of Syntax* codified transformational-generative linguistics (TG), which was genuinely revolutionary in rejecting the goals of earlier structuralist work. Rather than describing the phonological and morphological structures of languages (with syntax usually as an afterthought), and reconstructing earlier stages in their history, TG set out to discover the innate mental structures that allow the child to acquire complete knowledge of the language in an astonishingly short time, despite lack of instruction and exposure to degenerate data. Since mental structures were the goal, the best way to get to them was not through the analysis of a corpus produced by informants, but through the introspectively accessed knowledge of the linguist. Part of this knowledge was that the surface structure of sentences represents a transformation or series of transformations from an underlying deep structure.[18] But no sooner was the new 'paradigm' codified than some of its star exponents wanted to take it a step further (p.102):

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[18] For example, in early versions of the theory the surface structures *John hit Bill* and *Bill was hit by John* (these were violent times) emanate from the same deep structure, the second undergoing a passive transformation that switches subject and object position and inserts AUX (*was -ed)* and the preposition *by*. Chomsky soon realized that such powerful transformations, adding and deleting features and words seemingly without limit, had to be constrained, and moved to a model in which only movement transformations were allowed. More complete overviews of developments in transformational-generative linguistics can be found in Newmeyer (1986), Harlow (1994), Joseph (1994b: 4796-4797), and the Introduction to Droste & Joseph (1991), in addition to Harris' very lucid exposition.

> Everybody was happy with this work, but no one was content (with the possible exception of Katz). Postal, the hands-down-sharpest syntactician, expressed his unwillingness to stay put by digging more deeply under the surface, working in a direction which rapidly came to be known as *abstract syntax*, and several other linguists joined this project — most notably George Lakoff, Háj Ross, and James McCawley. Together they pushed syntax deeper and deeper until, to the extent that semantics had substance at the time, their deep structures became virtually indistinguishable from semantic representation [...] They took Chomsky at his word and made a grammar in which logical form had a central place. At this point, early in 1967, the program mutated into generative semantics, and Chomsky was displeased.

However Chomsky had gone off to Berkeley for a year to lecture and to complete his *Cartesian Linguistics* (1966), and while the cat was away, the MITnik mice played. By the time Chomsky returned to MIT, the generative semantics (GS) revolution was in full force, and he could not contain it. In part this was because he was outnumbered by his own best former students. Yet something else was in the soup that Harris does not consider. The notion of equating deep structure with semantic representation — a notion Chomsky never intended, though he claims to have been aware of it and flirted with it briefly before coming to his senses — was commonly read into his thinking by non-specialists from the early 1960s on. It is an extremely powerful and compelling idea, implying as it does that meaning is fundamentally the same across all languages, and that the differences among languages are merely superficial.[19] Despite the fact that it was not what Chomsky meant, its spread as the vulgar interpretation of his universalism contributed more than a little to the growth of his influence. The GS faction had picked up his strong card and played it against him. The general consensus as the 1960s ended was that Chomsky no longer appeared to be in the game.

Murray's account of GS (431-446) owes as much to Harris as Harris on the early years of TG owes to Murray. It is not surprising, therefore, that they agree on the basics: the demise of GS by the mid-1970s came about because "a lack of organizational leadership led to the dissolution of [a] distinct group [...]" (Murray, p.436). Murray declines to analyze the demise further until "further sorting out [of] the variables of the Griffith-Mullins model of

[19] It is the exact opposite of what Saussure had instilled into structuralism: that meanings (in their psychological realization, as 'signifieds') are language specific, and just as much a part of the language as the sounds and forms that express them. But even Saussure was firmly enough attached to the opposite, universalist view to have depicted signifieds as pictures of things, remaining the same across languages, in his diagrams of the linguistic sign (*Cours*, 99-101).

theory group formation" is possible; for now, he has "culled what there is in a source I distrust", namely, Newmeyer (Murray, 445-446). Yet Harris makes a very convincing case for what happened, and it is a series of events the "variables" of which no sociological model could possibly accomodate.

The generative semanticists were largely hoist with their own stylistic petards. The sharp humor which ran through their work — in stark contrast to the gravity of everything Chomsky produced —made it hard to tell when they were being serious. The 'fuzzy' and 'squishy' theories that Lakoff and Ross respectively were advocating (see Harris, p.230) were serious and important, prefiguring Langacker's cognitive (or 'space') grammar and potentially opening a door for late Wittgensteinian insights on language to be incorporated into linguistic theory. But it was hard to tell, given the sarcastic and nonsensical sample sentences they filled their papers with.[20] Then when their students aped the style without the substance, outsiders began to detect a "whiff of amateurism" (Hagège 1981:21, cited by Harris, p.203).

The self-inflicted *coup de grâce*, says Harris, was Lakoff's 1971 paper "On Generative Semantics", written as a satire of Chomsky's "Deep Structure, Surface Structure, and Semantic Interpretation" (in Chomsky 1972). Lakoff claims that Danny Steinberg asked him to provide a paper for a collective volume on semantics at just a month's notice, not enough time to prepare anything serious (Harris, p.225). The satire is all below the surface, at least until late in the article, where Lakoff begins proposing such overtly absurd rule features as [± PEDRO]. But even a joke as broad as this one was not obvious in the context of the GS style. Moreover, the title seemed to proclaim the paper as "an important manifesto of the approach" (Raskin 1975:462, cited by Harris, p.227). Thus it was read seriously, and those who read it found in it "a principal sign of the departure of coherence" in GS (Harris, p.226), just when they were expecting a paradigm statement.

It is a good thing for Harris that the story works so well on the human level. With thirty years' hindsight, the whole business seems quite preposterous, all bluster and little real intellectual engagement. The sad fact emerges tnat GS, far from being a revolution on the plane of ideas, was merely the

[20] McCawley was the king of hilariously obscene sample sentences, Ross of ridiculously named transformations. Harris provides a fine selection, including "Postal's *Euphemistic Genital Deletion* (for sentences where certain graphic nouns are demurely spirited away, as in *John is too big for Mary*, or *Max is playing with himself again*" (p.202). Today, alas, using such sentences would expose one to a charge of sexual harrassment.

reductio ad absurdum of some key leading ideas behind the original TG revolution. Ultimately the long experience at Revolution Central carried the day, combined with the fact that its old graduates (those not completely disaffected by the GS experience) were reaching maturity and moving into administrative roles which gave them a significant voice in hiring. A survey of job descriptions in linguistics across the 1980s shows how effectively government-and-binding theory (GB) kept other approaches at bay within the academy, even though they were reported to have greater appeal for the rapidly expanding field of computer programming. In what is really the only great success story in modern linguistic history, GB tacitly absorbed what insights it could from GS, and MIT became the Microsoft of the field, without even the U.S. Justice Department to control its expansion. But then, Bill Gates is not a Serial Revolutionary.[21] The minimalist program that put the brakes on GB could only have come from you-know-who.

Harris' and Murray's general depictions of linguistics in the second half of the 20th century have more in common with each other than either has with the 'received' account of Newmeyer. This is so even though Harris presents himself, not unjustly on the whole, as a non-partisan intermediary between a pro-Chomskyan Newmeyer and an anti-Chomskyan Murray. This means that Murray and Harris agree on the issues most of their readers will care most fervently about. But, as I indicated earlier, their handling of linguistic revolutions is not identical. Where Murray would reduce them to the sociological formula [good ideas + intellectual leadership + organizational leadership → coherent theory group], one comes away from Harris with the sense that Murray's formula leaves out two key ingredients: the personalities of the individuals involved, and sheer chance. Again, personality is not absent from Murray's historical chronicles, but the abstractions he aims toward are an attempt to rise above the accidents of individual traits and events. Call Harris' sense of history more humanistic, more rhetorical, or more cynical, or all three at once; but the notion that Murray's or anyone else's model could capture what is most essential to this story of the 'linguistics wars' suggests to me a view of history that gives up too much richness in exchange for a simplistic fiction of structure.

I have so far avoided comment on Harris' style. The word it brings to

[21] I have not seen Windows 95 as of this writing, but do not believe that it is comparable to the Revised Extended Standard Theory (REST).

mind is *jaunty*, which the *New Shorter Oxford Dictionary* defines as "Having or expressing a sprightly confident manner; perky, carefree, debonair". *Jaunty* derives from French *gentil*, usually translated into English as *nice*, a word which in its turn derives from Latin *nescius* "ignorant" and through the mid-16th century had the meaning "foolish, stupid, senseless". Various readers will find some or all of these epithets applicable to Harris' style. Personally, I rather like it, though I find the ironizing hipness a bit excessive at times, and fear that the colloquialisms and American pop culture references are likely to date the book very quickly. Actually I should be taking lessons here rather than giving them, for not only has Harris' book sold extremely well, but many people have commented (on the Linguist List, for example) about the clarity and elegance of its style. Elegance may be in the eye of the beholder, but the clarity of Harris' prose is beyond doubt, even when he is describing some of the more arcane intricacies of early generativism that are barely penetrable in their original versions.

No one seriously interested in the development of 20th-century linguistics can do without either of these books, which to a large extent are complementary. Murray's is, as advertised, a social history, written from the outside in. Harris, perhaps reflecting Chomsky's mentalist orientation, penetrates the minds of his subjects to tell their story from the inside out. In an ideal world, anyone wishing to read Harris would be required to study Murray first for necessary background and to have a sense of where "the linguistics wars" fit into the grander scheme of things, and anyone who had worked through Murray would get to read Harris for dessert. Neither is the ultimate recipe book for would-be revolutionaries, or the last word on 20th-century American linguistics. But their authors should give us many more words on the subject in the years ahead, or so (he said in a transparent allusion to *The Linguistics Wars,* p.328 n.20) we can all hope.

Author's address:

John E. Joseph
Department of English
University of Hong Kong
Pokfulam Road
HONG KONG
e-mail: jejoseph@hkucc.hku.hk

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