DISCUSSION NOTE

Getting the word out: The early generativists' multipronged efforts to diffuse their ideas

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This discussion note revolves around the early days of generative grammar, that is to say the late 1950s and the 1960s. A number of commentators have claimed that MIT linguists in this period formed an elitist in-group, talking only to each other by means of inaccessible 'underground' publications and thereby erecting a barrier between themselves and the outside world of linguistics. I attempt to refute such claims. We see that the early generativists used every means at their disposal at the time to diffuse their ideas: publishing single-authored books, journal articles, anthology chapters, and technical reports; aiding the writing of textbooks; giving conference talks; teaching at LSA (Linguistic Society of America) Institutes; and hosting numerous visitors to MIT. And in particular, there was no significant 'underground' literature to obstruct the acceptance of the new theory.*

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1. INTRODUCTION. As early as 1964 a commentator had written: '[T]ransformational grammar has established itself as the reference point for discussion of linguistic theory … it remains the case that it has been Chomsky who has effectively opened the American linguistic scene to its present free and fruitful discussion' (Hymes 1964:25). A year later, there was talk of a 'Chomskyan revolution' in the field (Bach 1965b:111–12, Levin 1965:92, Thorne 1965:74).[1] How did the theory of transformational-generative grammar succeed so rapidly, that is, in seven or eight years after the publication of *Syntactic structures* (Chomsky 1957)? The most important reason by far is that a wide spectrum of the linguistic community found the theory to be either correct or on the right track (for extensive discussion, see Newmeyer 1986:Ch. 2). The time was right for a radical shift in focus from the description of linguistic forms to a generative mechanism that provided structural analyses of these forms. Generative grammar provided new and powerful tools for linguistic analysis, which played a crucial role in attracting people to the enterprise. At the same time, the early generativists provided concrete and detailed accounts of long-standing problems for grammatical theory, such as the discontinuous nature of the English auxiliary morphemes, long-distance dependencies, the

* I dedicate this publication to the memory of Robert B. Lees, my Ph.D. advisor and friend. Thanks to Thomas Bever, Greg Carlson, Julia Falk, Robert Freidin, Jay Keyser, Barbara Partee, and David Perlmutter for reading the entire prefinal manuscript and making some valuable suggestions. I also wish to thank the great number of colleagues who took the time to share their thoughts and memories with me about the old days: Stephen Anderson, Emmon Bach, Ray Cattell, Paul Chapin, Donna Christian, Noam Chomsky, Charles Fillmore, Lily Fillmore, Bruce Fraser, Donald Freeman, Lila Gleitman, Allene Grognet, Morris Halle, Florence Warshawsky Harris, James Harris, Randy Harris, Paul Kiparsky, Susumu Kuno, Robin Lakoff, Terence Langendoen, Jean-Claude Milner, Wayne O'Neil, David Pesetsky, Paul Postal, Peter Reich, Neil Smith, Donca Steriade, Rudolph Troike, Peter Trudgill, Anna Wierzbicka, and Arnold Zwicky. It goes without saying that none of these individuals is responsible for any assertions made in this discussion note.

[1] It needs to be stressed that Chomsky himself has always denied that the field has undergone a 'Chomskyan revolution' (see especially Chomsky 1982:42–43). In a recent lecture, he told the audience: 'My feeling about linguistics is that we're in a pre-Galilean stage. Maybe there will come a scientific revolution, but we are still struggling towards it' (Chomsky 2011).

intricacies of Slavic morphophonemics, and much more. These accounts demonstrated to a sizeable number of linguists that the foundations of the theory led directly to empirical results, and were not merely speculative philosophy.

Needless to say, a significant percentage of linguists rejected generative grammar in the 1950s and 1960s, and it remains the case today that a significant percentage would not consider themselves to be advocates of the theory. But even among practitioners and sympathizers, there has long been a feeling that generative grammar succeeded DESPITE the professional behavior of its early developers, and not in any way BECAUSE OF that behavior. To read some of the critical literature that has appeared over the past half-century, the early MIT linguists formed an elitist in-group, interested in talking only to each other by means of inaccessible 'underground' publications. Indeed, it has been argued that they went to such lengths to erect a barrier between themselves and the outside world of linguistics that the early period of generative grammar was no less than a 'decade of private knowledge', a term that first appears in Ney 1975.

The most comprehensive 'decade of private knowledge' assessment appears as a central feature of a recent University of Toronto dissertation written by Janet Nielsen (Nielsen 2010). Nielsen is by no means antigenerative, writing that '[B]y bringing problematic data constructions, formalization, language creativity, and psychological validity to the forefront, Chomsky's program fundamentally changed the conception of explanation in syntax' (Nielsen 2010:122). Nevertheless, she charges that:

> [F]rom 1957 to 1968 transformational grammar operated an underground culture: research was DELIBERATELY kept out of mainstream journals, and work was narrowly circulated in mimeograph form among a select group of insiders. Those with close connections to MIT, where the transformational school was centered, had privileged access to new research—and those outside found it difficult, and at times impossible, to access transformational work. (Nielsen 2010:23, emphasis added)

Nielsen goes on to write that 'MIT transformationalists SHUNNED ACADEMIC JOURNALS, deliberately kept their work private, and maintained a tight network of communicants' (pp. 242–43, emphasis added). Indeed, '[a]ll the key figures in early transformational grammar, from Chomsky to Lees to Postal to McCawley, produced underground documents. This culture of private knowledge was, as we will see, a mechanism for group cohesion, a statement against mainstream publishing outlets, and a reflection of rapid research progress' (p. 252). She gives many examples that purport to demonstrate a culture of private knowledge, most of which are illustrated and addressed below.

The goal of this discussion piece is to reply to Nielsen's assessment and to those commentaries on which it is based. I argue that they are all either greatly exaggerated or flat out wrong. As we see in what follows, the early generativists had a knack for availing themselves of every possible means of spreading the word about the new theory.

One might wonder what purpose is served by examining in detail the actions of linguists from so long ago. If for no other reason, I would argue that it is never out of place to 'set the record straight'. If the claims by Nielsen and others went unanswered, they would set a baseline for future historiographers of linguistics. That would be unfortunate. Moreover, questions about the propagation of scientific ideas have always been of wide-ranging interest. There is still active discussion, for example, about the role of the Royal Society in the rapid success of Newtonian physics. Also, we are now at about the point where there is enough distance between then and now to allow an intelligent conversation about what went on and why a half-century ago. And finally, and more poignantly, the active participants in the early days of generative grammar are either no longer with us or are well past retirement age. In a certain number of years, it will no longer be possible to draw on the first-hand testimonials that help bring history to life.

This discussion piece is organized as follows. I begin with an overview of the early days of the MIT linguistics program (§2), and then call attention to the existence of claims that members of this program resisted publication of their research results (§3). Section 4 reviews the publication record of prominent generativists from this period and demonstrates that they were profuse publishers. I next take on what is often called the 'underground literature' put out by generative grammarians at that time, showing that most of it was quite readily available to any interested party (§5), and then detail the means other than publication that were employed at the time to help propagate the theory (§6). Finally, I attempt to explain the origins of the idea that MIT linguists were possessors of an occult 'private knowledge' (§7) and offer a brief conclusion (§8).

2. The beginnings of generative grammar at mit. It seems reasonable to begin with a brief sketch of the development of the linguistics program at MIT.[2] The early history of the program was tightly interwoven with the Research Laboratory of Electronics (RLE) at that institution. In 1946, when the RLE was founded, one of its four major research groups was called 'Communications and Related Subjects'. At the RLE, William Locke, then Chairman of the Department of Modern Languages, directed a group doing research on the acoustics of speech. In 1951 Morris Halle, at the time a student of Roman Jakobson's at Harvard, was hired by the Department, where he taught Russian and German, and by the RLE, where he did phonetic research. 'It can be said that Morris Halle's official arrival marked the actual beginning of the linguistics group at MIT, not only because he was its first member but also because it is generally acknowledged that he has been its chief architect and builder as well' (Harris & Harris 1974:np). Another language-related activity at the RLE at the time was machine translation, spearheaded by Locke and, after 1953, by Victor Yngve.

The picture changed dramatically with the hiring of Noam Chomsky in 1955. Chomsky was to receive a Ph.D. in linguistics from the University of Pennsylvania (under Zellig Harris) and was completing four years as a Junior Fellow at Harvard. With Chomsky's hiring (again, both in Modern Languages and the RLE), research on syntactic theory was to become increasingly prominent. New appointments soon followed: Joseph Applegate and G. Hubert Matthews in 1956, Edward Klima in 1957, Jerry Fodor in 1959, and Jerrold J. Katz in 1961.[3] In 1960, Roman Jakobson, who had been a visiting professor at MIT in 1957, was appointed an Institute Professor in the Department of Modern Languages while still retaining his affiliation with Harvard. In 1959 the first two Ph.D. theses were completed under the direction of Chomsky and Halle, one by George Hughes on speech analysis and another by Robert B. Lees on English nominalizations. Given the inability of Modern Languages to award a doctorate, together with the RLE connection, the degree-granting department was Electrical Engineering.

In 1961 a graduate program in linguistics was approved at MIT under the Department of Modern Languages, leading to the degree of Doctor of Philosophy in Linguistics. Seven students were admitted in the fall of that year and ten more in the fall of 1962. Thirteen received the Ph.D. degree in 1965, one in 1966, and five in 1967. In terms of hiring, Paul Postal, a Yale Ph.D., joined the department in 1963 (and left after two years), Kenneth Hale, an Indiana Ph.D., was hired in 1966, and Wayne O'Neil, a Wisconsin Ph.D., in 1968. The other hires in the 1960s were all graduates of the program: Paul Kiparsky, James Harris, and John R. Ross.

[2] What follows draws heavily on the discussion in Harris & Harris 1974.

[3] Applegate, Matthews, and Klima were in Modern Languages, and Katz and Fodor in the Department of Humanities. All were at some point affiliated with the RLE.

3. Accusations of 'private knowledge'. In a paper published in 1975, James Ney excoriated the early generative grammarians for what he saw as their practice of circulating their research findings only among themselves, resulting in the 1960s and early 1970s being what he called 'a decade of private knowledge':

> [I]t is the intent of this article … to examine the dissemination of information amongst the community of linguists and scholars interested in the study of language. From this, the attempt will be made to show that for linguists the period from the early 1960s to the early 1970s was indeed a decade of private knowledge … (Ney 1975:143)

If one was not part of the 'inner circle', then it appears that one was out of luck:

> [T]he early grammars seemed to be circulated in unpublished form amongst a coterie of linguists which constituted the inner circle of the early transformational movement. These grammars and other studies were referred to in footnotes in the published journal articles but appeared to be generally unobtainable for scores of scholars interested in the study of language. During the latter part of the decade this problem was alleviated somewhat by the appearance of numerous linguistics journals and by the apparent ease with which many scholars were able to publish books and monographs in the area of linguistic studies. There are, however, still far too many reports to various government agencies and other such materials on the topic of general linguistic theory that are difficult for the average scholar to obtain. For both of these reasons,[4] the decade under consideration has been a decade of private knowledge for linguistics. (Ney 1975:143–44)

Ney's complaint echoed others that had been made more than a decade earlier. For example, the Dutch linguist E. M. Uhlenbeck had written that:

> [n]ot all of Chomsky's 13 publications on syntax are generally available. To his thesis called 'The logical structure of linguistic theory' quoted by himself and by Lees several times, I had no access. His two contributions to the 3rd and 4th conference on problems in the analysis of English held in Texas were also not accessible. The same goes for his article on 'Explanatory models in linguistics' … (Uhlenbeck 1963:3)

A few years later Fred Householder remarked that 'these young men, in the first instance, talk only or chiefly to each other; exchanging Xeroxes and dittoed copies for a long time before communicating their paradoxes to the outside world' (Householder 1971:xi), while P. H. Matthews in a review of one of the first compendia of generative literature remarked that:

> of the eighty transfomationalist papers listed [in the references to Jacobs & Rosenbaum 1970] forty are available in microfilm or mimeograph at best, and upwards of fifteen are scarcely available at all ('Unpublished, untitled paper on … , MIT', 'Remarks delivered at … ', and so on). Surely this is a serious discourtesy to all but a handful of readers. (Matthews 1972:125)

These implied charges of elitism were reinforced by Robert Stockwell's observation that 'if you weren't on the right mailing lists you could be seriously out-of-date in a few months' (Stockwell 1998:238) and by H. Allan Gleason's recollection that:

> an objection [to a generativist paper at an LSA meeting] would be answered by the simple assertion that the matter had been taken care of and warranted no further discussion. This would be supported by the citing of a paper in the underground press—a paper whose existence the objector had no way of knowing about, and which he would for some years have no opportunity to see. (Gleason 1988:65–66)

Finally, we have already looked at the key passages from Nielsen 2010 that claim that early generativists 'shunned academic journals' and 'deliberately kept their work private'. The next section demonstrates that such charges lack factual foundation.

[4] Ney also illustrated 'private knowledge' by the profuse use of personal introspective judgments of acceptability. The present discussion note has nothing to say about that issue.

4. The early generative grammarians were committed to publication. Chomsky's coming out to the world of linguistics was via his book *Syntactic structures* (Chomsky 1957). This work was about as mainstream as any publication could possibly be. Mouton was later to acquire a bad reputation for publishing too much of too low quality, but in 1957 it was riding high, having published Jakobson and Halle's *Fundamentals of language* (Jakobson & Halle 1956) the year before as the first title in its new 'Janua Linguarum' series. And if anything it was rushed to publication, based as it was on his lecture notes for a class at MIT that he had taught the year before (Chomsky 1975:3). Let us examine the publication record between 1957 and 1969 of four leading generative grammarians in that period: Noam Chomsky, Morris Halle, Robert B. Lees, and Paul Postal.[5]

In that period, Chomsky published six books, which is more than the average scholar writes in his or her lifetime. The publishers, aside from Mouton, were MIT Press, Harper & Row, and Harcourt Brace & World. I would hardly describe any of them as 'marginal'. As far as refereed journals are concerned, Chomsky published in *International Journal of American Linguistics* (twice), *American Documentation*, *Language* (three times), *Information and Control* (three times), *Proceedings of the Aristotelian Society*, *Word* (twice), *Journal of Linguistics*, *College English* (twice), *Syntheses* (twice), *The Listener*, *The Pedagogic Reporter*, and *Educational Review*. Twenty-five articles appeared in collections. What is more, he published one article in Hebrew, six in French, and three in Japanese, which would seem to indicate a desire to reach past a narrow anglophone audience.

Halle published two books between 1957 and 1969. His articles and reviews appeared in *Journal of the Acoustic Society of America* (four times), *Kratylos*, *Nuovo Cimento*, *International Journal of Slavic Linguistics and Poetics*, *Word* (twice), *Language* (twice), *Information and Control*, *Journal of Linguistics* (twice), *College English*, *International Social Science Journal*, *Slavic and East European Journal*, and *The Study of English*. He had nineteen articles in collections, and he published in Russian and French, as well as in English.

Lees had two books in that period, plus an edited book in Turkish (he also published in Russian three times). He contributed to the following journals: *Mechanical Translation*, *Nuclear Science and Engineering*, *Anthropological Linguistics* (twice), *Language* (five times), *Word* (twice), *Language Learning*, *American Speech*, *Zeitschrift für Phonetik*, *Gengo Kenkyu*, *General Linguistics*, *Linguistics* (twice), *Foundations of Language*, *Indogermanische Forschungen*, *Journal of the Conference on College Composition and Communication*, *Türk Dili Araştirmaları Yilliği-Belletin*, and *International Journal of American Linguistics*. He also had two articles in collections.

Between 1957 and 1969 Postal had three books and published in the journals *International Journal of American Linguistics* (four times, one article being reprinted in a Czech journal), *Harvard Education Review*, *Foundations of Language* (twice), *Language*, *International Social Science Journal*, and *American Anthropologist*. Six papers appeared in collections, and one was published in Russian.

The culture of publishing one's research results was effectively communicated to the students. The first three years that Ph.D.s were awarded at MIT were 1965, 1966, and 1967. Those receiving the degree in those years were Thomas Bever, Paul Chapin, James Fidelholtz, James Foley, Bruce Fraser, Jeffrey Gruber, Barbara Hall (Partee),

[5] I ignore nonlinguistic publications, such as Postal's two anthropological contributions and Chomsky's political writings.

James Harris, Paul Kiparsky, Sige-Yuki Kuroda, Terence Langendoen, Philip Lieberman, Theodore Lightner, Stanley Petrick, James McCawley, Peter Rosenbaum, John R. Ross, Sanford Schane, and Arnold Zwicky. In the 1960s all of them, with the exception of Chapin, Fidelholtz, and Lightner, published either books, refereed journal articles, or papers in easily accessible anthologies, and Chapin and Lightner published in the year 1970.[6] In most cases, these publications were based on their dissertations or were revised versions of the dissertations themselves.

If MIT linguists had been indifferent to publishing their research results, they would hardly have been the driving force behind the formation of a new journal in the late 1960s. Yet department members played a crucial role in launching *Linguistic Inquiry*:

> [Morris Halle] felt, I think, that it was 'difficult' to get articles in generative grammar published, not because of any discrimination against generative grammar, but rather because there was a glut of potential articles and a paucity of journal outlets; mainly, just two, *Language* and *Word*. He suggested to Curly Bowen that the MIT Press start a journal in linguistics and when Curly, naturally enough, wondered who would edit it, he suggested me. I'm sure Noam was involved in these discussions, but I have no specific memory of that being the case. So you can see that the founding of *LI* fits in very nicely with the thesis that the generativists were not inclined to hide their lights under a bushel. I met with Curly and agreed to edit the journal for 5 years. That was 42 years ago, going on 43. (Jay Keyser, p.c., January 1, 2013)

In sum, the evidence weighs heavily against the idea that the early generative grammarians 'shunned academic journals' and 'deliberately kept their work private'.

5. On the so-called 'underground literature'. Not everything written by a generative grammarian in the 1950s and 1960s appeared immediately (or with a short delay) in a mainstream journal or book. James McCawley popularized the idea that there was also a significant 'underground literature: the many papers that have been circulated in office-duplicator or in project reports but have not been given normal publication in journals or books' (McCawley 1976b:1). These writings have been discussed at great length in Nielsen 2010, which devotes a chapter to discussing 'three subtypes of underground literature' (Nielsen 2010:248) produced by generative grammarians between 1957 and 1968: 'manuscripts written in forms and styles appropriate for publication, but which were deliberately circulated privately and informally' (p. 248), the institutional or laboratory report, and 'manuscripts which were deliberately written in forms inappropriate for publication, and circulated privately' (p. 251).

In this section I discuss the literature in question, following Nielsen's categories. I argue that most of it was available to any interested linguist, thereby calling into question the appropriateness of the word 'underground'.

5.1. Polished manuscripts 'deliberately circulated privately'. Nielsen refers to four manuscripts 'deliberately circulated privately': Chomsky's *The logical structure of linguistic theory*, Lees's 'What are transformations?', Postal's *Some syntactic rules in Mohawk*, and Ross's *Constraints on variables in syntax*. They are discussed in the following four subsections.

The logical structure of linguistic theory. Nielsen 2010 singles out Chomsky's manuscript *The logical structure of linguistic theory* (*LSLT*) as the most important nonpublication in this category. One can dispute whether *LSLT* was 'written in forms and styles appropriate for publication', but one cannot dispute its importance. This 800-page manuscript was the product of Chomsky's years as a Junior Fellow at Harvard. *LSLT* laid the foundations for transformational-generative grammar and even today is cited for its

[6] I exclude publications in working papers and technical reports (see below, §5.2).

original insights about grammatical processes. Some of the formal mechanisms proposed in *LSLT* went into eclipse for decades, only to be resuscitated in recent years. A finished copy (though one that was to be revised over the next few years) was produced in 1955. However, the book would not see publication for another twenty years (Chomsky 1975 [1955]). The question is why it was not published in 1955 or soon afterward. There are several reasons. The most direct reason is that it was rejected:[7]

> After the revisions [to *LSLT*] described were completed, I submitted parts of the manuscript to the Technology Press of MIT for consideration for potential publication. It was rejected, with the not unreasonable observation that an unknown author taking a rather unconventional approach should submit articles based on this material to professional journals before planning to publish such a comprehensive and detailed manuscript as a book. (Chomsky 1975:3)

'Comprehensive and detailed' is an understatement. It is unlikely that more than a small handful of linguists active at the time could have understood its formalism, a fact that surely deterred Chomsky from revising it rapidly for publication. Moreover, as Chomsky noted, 'After I returned to MIT in the fall [of 1959], this [work on generative phonology] and other work took precedence, and I never did return to the revision of the remaining chapters' (Chomsky 1975:4). In any event, the great bulk of *LSLT* WAS IN FACT PUBLISHED AT THE TIME, some key sections appearing in *Syntactic structures* and others immediately thereafter in a series of articles published in the late 1950s and early 1960s: Chomsky & Miller 1958, 1963, Chomsky 1959, 1961a,b, 1962a,b, 1963, and Chomsky & Schützenberger 1963. It was really just the material dealing with phonology and (to a certain extent) morphology that was not published, and that because Chomsky had by the end of the 1950s drastically revised his notions of how these 'lower levels' of grammatical structure operated. One can fault Chomsky for not stressing sufficiently that if one wanted to know what was in *LSLT*, then all that one needed to do was to read his book and subsequent articles. But one cannot fault him for not making the material in *LSLT* available (this point was made in Bach 1965a:281).

One wonders in fact how very difficult it was to obtain a copy of *LSLT* in those days. Fred Householder recalls that he simply borrowed one from another colleague:

> … Ed Stankiewicz lent me the first three chapters of the *Logical Theory of Linguistic Structure* [sic] by Chomsky. That book really got me excited. It was around this time that Zellig Harris gave his Presidential Address (on transformations) at the LSA meetings in Chicago. (Householder 1980:199)

Harris was LSA president in 1955, the year that *LSLT* was completed. One might point out that Stankiewicz had no connection with MIT, nor did he ever become a generative grammarian. And consider the following recollection by H. Allan Gleason:

> In 1955 [Chomsky's] thesis was submitted. It was available at the Linguistic Institute that summer and was discussed. A few linguists found it very difficult; most found it quite impossible. A few thought some of the points were possibly interesting; most simply had no idea as to how it might relate to what they knew as linguistics. (Gleason 1988:60)

In a footnote to this passage Gleason wrote: 'Or, perhaps, it was his *The Logical Structure of Linguistic Theory*; my memory is not quite clear. In any case it was something

[7] Stephen Murray claimed that 'two publishers were interested in publishing *LSLT* in 1957' (Murray 1980:78). Murray later wrote that Chomsky had written him that he had 'never heard of the alleged offers to publish *LSLT*' and that he 'didn't submit book proposals anywhere' (Murray 1999:262). Murray counters that both North-Holland and Mouton offered to publish *LSLT* in the late 1950s and early 1960s. To make his case, Murray quotes on page 263 an aerogram from Chomsky to the Mouton editor Cornelis van Schooneveld stating that 'I have a tentative agreement with North Holland to publish it', as well as correspondence from van Schooneveld stating that Mouton wanted to publish the manuscript. One wishes that Murray had scanned Chomsky's aerogram into his article to settle the question once and for all. Unfortunately, he did not.

far more mathematical in its reasoning than anyone there had ever seen labeled as "linguistics" ' (Gleason 1988:60). Gleason was certainly correct in the footnote, since Chomsky's doctoral thesis (which was essentially a small proper subpart of *LSLT*) was never widely circulated, not even among linguists at MIT. At that institute, held at the University of Chicago, the teaching staff, aside from Gleason, consisted of Isidore Dyen (Yale University), William J. Gedney (American Council of Learned Societies), Charles F. Hockett (Cornell University), Fang-Kuei Li (University of Washington), Floyd G. Lounsbury (Yale University), Robert O. Swan (University of Pennsylvania), Donald E. Walker (Rice Institute), and twelve members of the Chicago faculty (*LSA Bulletin* 29, 1956).[8] The courses given there were attended by sixty-five students. If even just a small percentage of these individuals availed themselves of a copy of *LSLT*, it would have been subsequently transported from coast to coast in the United States. Chomsky in 1975 wrote: 'I have not kept count, but there must be several hundred copies [of *LSLT* in circulation]' (Chomsky 1975:3).

Robert b. lees's paper 'what are transformations?'. I had never heard of Lees's paper 'What are transformations?' (1976 [1960]) until it appeared in McCawley's anthology of 'underground' papers (McCawley 1976b; see §5.3 below), and this despite my having been Lees's own Ph.D. student in the 1960s. And in fact the paper had already been published in the Russian journal *Voprosy Jazykoznanija*. Even in the 1976 English translation, all of the examples are drawn from Russian. It is a mystery to me why McCawley (and hence Nielsen) would consider this to be an 'underground' publication. It does little more than review the ABCs of generative grammar in a way that could have been found in a dozen or more publications from the early 1960s.

Paul postal's ph.d. thesis *some syntactic rules in mohawk*. Postal submitted his Yale University dissertation, entitled *Some syntactic rules in Mohawk* and written under the direction of Floyd Lounsbury, in 1962. However, it was not published until 1979 (Postal 1979 [1962]). To help us understand why it was not published immediately, let us first recall that junior scholars did not generally rush to publish their dissertations, as they do today. We tend to forget that there was a time, long ago, when a tenure-track post at a university was not limited to those who had published their dissertations, two or three papers based on it, and had begun to explore new avenues of research. So we can fault Postal only if his work was of such importance that to not publish it would have been detrimental to the field. Was it of great importance? It is historically noteworthy in its being one of the first generative dissertations and the very first, I think, to deal primarily with an Amerindian language. But if the number of citations to a work is a good guide to its importance, then Postal's thesis was not in the first rank. It was referred to in passing in Chomsky 1964a and Chomsky 1965, but very rarely thereafter. For example, we find no reference to it in Ross 1967, nor in the first three major generative anthologies: Bach & Harms 1968, Reibel & Schane 1969, and Jacobs & Rosenbaum 1970.

As was the case with *LSLT*, everything of importance in Postal's thesis was published separately, in particular in Postal 1964a,b,c. Anybody who felt the desire to read the entire work could have done as I did and ordered a copy from University Microfilms.

John r. ross's ph.d. thesis *constraints on variables in syntax*. One cannot dispute the importance of Ross's 1967 MIT dissertation *Constraints on variables in syntax*. I

[8] There were actually three Institutes that summer, one at Chicago, one at the University of Michigan, and one at Georgetown University. I assume that Gleason was referring to the Chicago Institute, since he was on the teaching staff there.

know of no study that would allow me to confirm my intuition, but I am quite certain that it is the most cited Ph.D. thesis ever produced in the field of linguistics. Yet it was not published by a mainstream publisher until 1985 and then under the revised title *Infinite syntax!*. But that does not mean that it was not easily obtainable. The following year the Indiana University Linguistics Club (see below, §5.4) made it available to anybody in the world who might desire a copy and for a very moderate fee. Hence there is no sense in which Ross's thesis could be called an 'underground document'. Nielsen complains however that:

> [i]n John Robert Ross' *Constraints on Variables in Syntax*, for example, 32 documents in his 67-item bibliography—just under half of the items he references—are listed as 'unpublished paper', 'unpublished mimeograph', 'mimeographed', 'unpublished Ph.D. thesis', 'working paper', or are listed as appearing in technical project reports. He includes nine of his own works, none of which had been published: one unpublished paper, one unpublished mimeograph, his master's thesis, one unpublished ditto, two Harvard Computation Laboratory Reports, and three 'to appear' articles. (Nielsen 2010:258)[9]

Was it really the case that almost half of Ross's citations would have been unavailable to the average working linguist of the period? Absolutely not. Here is my breakdown of his sixty-seven references: cited as published or in press—thirty; published in 1967–1969—four; published in 1970 or later—seven; never published—seven;[10] published by the Indiana University Linguistics Club before 1970—three; in technical reports or departmental working paper volumes (see below, §5.2)—nine; theses (and therefore available from University Microfilms)—seven. By my count only fourteen references—the material published in 1970 or later or not published at all—would have been unavailable to the late-1960s linguist who was willing to put forward a small amount of effort. And of course, then as now, one could generally receive a copy of a paper by writing the author for one.[11]

5.2. The institutional and laboratory reports. Nielsen's second category of 'underground literature prevalent in the late 1950s and 1960s is the institutional, or laboratory, report. This includes, most prominently, the MIT Research Laboratory of Electronics *Quarterly Progress Reports* and the Harvard Computation Laboratory Reports' (Nielsen 2010:250). Let us examine them and then turn to working papers.

The RLE *Quarterly Progress Reports*. Several important linguistics papers were published in the 1960s in the RLE's *Quarterly Progress Reports* (*QPR*s), typically appearing in the same issue with papers on neurophysiology, communications biophysics, and cognitive information processing. The *QPR*s provided a rapid outlet for the dissemination of research results. Some articles were later published in mainstream journals, but others were not. The *QPR* papers were not in any sense 'underground publications'. Many university libraries subscribed to *QPR*s from their beginning in 1946 or started their collection in the next decade or two. In an informal survey, I found that twelve of the first sixteen university library catalogs I looked at showed *QPR*s going back to

[9] Dissertations (as compared to published volumes) have always been top-heavy with references to unpublished material. Consider a recent example, a highly regarded Stanford Ph.D. thesis on questions of lexical semantics (Koontz-Garboden 2007). The bibliography contains six references to unpublished manuscripts, sixteen to unpublished theses, six to working papers, four to presentations, and seven to talk handouts. The main difference between 1967 and 2007 is that in the latter year most of the references could be tracked down on the internet.

[10] One of the never-published papers was a proper subpart of Ross's thesis itself. I have no idea why he cited it.

[11] One could reply to P. H. Matthews (see above, §3) by providing a similar breakdown for the references in Jacobs & Rosenbaum 1970 as for those in Ross's thesis.

the 1960s or earlier, namely Alberta, Arizona, UC Berkeley, UC Davis, the Claremont Colleges, Cornell, Notre Dame, Oregon, San Diego State, Stanford, Texas, and Washington State.

The harvard computation laboratory reports. Between 1965 and 1974 the Computation Laboratory of Harvard produced a series of red-covered reports to the National Science Foundation under the rubric of 'Mathematical Linguistics and Automatic Translation'. They were generally referred to in the linguistics community as 'NSF-26', 'NSF-28', and so on. The first in the linguistics series, NSF-16, was George Lakoff's landmark Indiana dissertation *On the nature of syntactic irregularity* (Lakoff 1965). Most of the subsequent NSF reports were collections of papers written primarily by linguists at Harvard and MIT (Ross cites five of them in his thesis). They were available to anybody who asked for them. Susumu Kuno, who was the principal investigator of the NSF grant during most of this period, writes:

> In 1965, I started editing reports to NSF that contain papers on formal linguistics. NSF-16 (1965), which consisted of a dissertation by George Lakoff, who had joined my NSF project the previous year, was the first of such reports. My vague recollection is that the last volume of the series was NSF-31 (1974), *Adjectives and Adjective Complement Constructions in English* by Arlene Berman, but since my copy has been lost, I can't vouch for it. During 1965–1974, my vague recollection was that we had about 200 copies made of each report. We must have met any requests for copies as long as they were available, but I don't recall running out of copies, so around 200 copies per report must have been the extent of the circulation of the reports. (Susumu Kuno, p.c., October 3, 2012)

Two hundred copies does not sound like a lot, but one must consider the size of the field at the time. To put things into perspective, consider William Labov's *The social stratification of English in New York City* (Labov 1966), which was as important for variationist sociolinguistics as *Syntactic structures* was for syntactic theory. According to Allene Grognet, who was the editor for the book at the Center for Applied Linguistics, the initial press run was '500 copies, or 1000 at the very most' (p.c., October 12, 2012).

Working papers. Two of Ross's citations were to departmental working papers, one from the Ohio State University and one from the University of Pennsylvania. There was nothing 'underground' about them. Virtually every department of linguistics has put out a series of working papers at some point in its history. At first they tended to be free for the asking, then free with postage included, and then available only for a fee. Now, of course, they are mostly online. In the 1960s a letter or telephone call to OSU or Penn would have resulted in a free copy for anybody interested.

Ross also referred to two working papers from the MITRE Corporation, a research and development organization with close ties to MIT, located in Bedford, MA. Several MIT students in the early 1960s had summer research positions at MITRE. According to Bruce Fraser, who was one of those students:

> The MITRE paper Haj cites was available from MITRE as well as by contacting me directly. Same as if I worked at IBM today. I have heard it said that it was difficult to get MIT papers, but I've never believed it. We at MIT wanted to spread the word and looked for every opportunity to do so. Maybe people who have spread this rumor had trouble understanding the linguistics and used this claim as a convenient excuse. (Bruce Fraser, p.c., June 16, 2013)

5.3. The third type of 'underground literature' and McCawley 1976b. Nielsen writes:

> The third and final type of underground literature are manuscripts which were deliberately written in forms inappropriate for publication, and circulated privately. Witty, satirical, and often downright rude, this style of underground literature is associated most prominently with the generative semantics movement (circa 1968–1974). In the late 1960s, however, it straddled the border between regular transformational literature and generative semantics. (Nielsen 2010:251)

Oddly, Nielsen gives only two examples: Paul Postal's 'Linguistic anarchy notes' (Postal 1976 [1967]) and Jerry Morgan's 'Cryptic note II and WAGS III' (Morgan 1976 [1969]). She is certainly correct that they were 'inappropriate for publication', which perhaps goes a long way toward the explanation of why they were not at first published! But James McCawley did indeed publish them in his *Notes from the linguistic underground* (McCawley 1976b), and it is to this book that we now turn.

No publication by a generative grammarian has done more than McCawley's *Notes* to incorrectly convey the idea that a significant underground literature existed at the time. The book itself is a motley collection of twenty-two contributions, some of which actually had already been published, some that were put on the back burner by the author, given his or her belief that they were not particularly interesting, and some that never should have been written in the first place. The common thread is that McCawley had seen them and was intrigued by them, not that they had played an important role in the development of the theory. My somewhat blunt assessment can be confirmed by looking at the citation lists in six important publications from the early to mid 1970s: three by interpretive semanticists (Chomsky 1972, Jackendoff 1972, Emonds 1976) and three by generative semanticists (Postal 1971, Lakoff 1972 [1970], McCawley 1976a). Excluding McCawley 1976a, the remaining five works refer to only three out of the twenty-two papers in McCawley's *Notes* collection, namely to Harris 1976 [1965],[12] Lakoff 1976 [1969], and Karttunen 1976 [1969]. All three of these papers, whose interest is uncontestable, had been published by the Indiana University Linguistics Club before they appeared in *Notes*: in 1974, 1969, and 1971, respectively. McCawley himself refers only to eight of the twenty-two. We might be dealing with an 'underground' literature here, but not one that appears to have had any significant consequences for linguistic theorizing. Those who knew Jim McCawley well, and I count myself lucky to be in that category, recognized *Notes from the linguistic underground* as a reflection of his puckish anarchism, with his choice of contents to be taken with a grain of salt.

5.4. More informal publication outlets. By the mid to late 1960s it was clear to most MIT linguists that the time lag between finishing the draft of a paper and its appearance in print in a conventional journal was too long. To remedy this situation, in early 1967 a few members of the department set up the Program for the Exchange of Generative Studies (PEGS). The idea was to create a formal mechanism for the distribution of unpublished papers, in particular to help counteract the view that MIT linguists were myopic and inward-looking. Over the next year, as the number of participants grew, PEGS was taken over, at the request of its organizers, by the ERIC Clearinghouse for Linguistics at the Center for Applied Linguistics. Within the first three months of the takeover, 40,000 mimeographed copies were reproduced and mailed. However, '[f]aced with an expanding operation which it could not under the terms of its mandate handle for an extended period, the clearing-house turned over the PEGS program in 1969 to a newly established linguistic journal' (Roberts & Woyna 1972:17).[13]

[12] Florence Warshawsky Harris wrote the 1965 paper while a first-year graduate student at MIT. She did not complete the program, nor become a professional linguist. When I asked her why she did not publish the paper earlier, she replied: 'I just never thought about it. And nobody asked me until Jim McCawley did' (p.c., October 14, 2012).

[13] The question is: 'Which journal?'. I had assumed that it was *Linguistic Inquiry*, but Jay Keyser has no recollection of the PEGS program being turned over to *LI* (p.c., January 1, 2013). No other journal fills the bill, and neither Roberts nor Woyna is still here to tell us.

The slack was taken up by the Indiana University Linguistics Club (IULC), founded by the Indiana Department of Linguistics in 1968 'to support the activities of students in linguistics and related fields' (from the IULC webpage). While the IULC did not then, nor does it now, have an explicit generative orientation, the vast majority of its publications have presupposed generative grammar. The following is a complete list of IULC authors in the 1960s: Adrian Akmajian, Noam Chomsky, Susan Fischer, Ray Jackendoff, Lauri Karttunen, Carol Kiparsky, Paul Kiparsky, Charles Kisseberth, Andreas Koutsoudas, George Lakoff, D. Terence Langendoen, Roger Lass, Byron Marshall, James McCawley, J. R. Ross, Gerald Sanders, Carlota Smith, Robert Wall, and Wolfgang Wolck. Typically, a paper submitted to the IULC could be available for distribution at a very reasonable fee within a few months. As such, it provided an ideal mechanism for generative linguists to diffuse their ideas quickly and to a wide audience.

5.5. A LITTLE HISTORICAL PERSPECTIVE. There is a long history of complaints that some seemingly important piece of literature is unavailable. Consider an example from pregenerative days:

> In 1934, almost as an immediate response to Bloomfield's *Language* ([Bloomfield 1933]) Chao Yuen Ren had published a paper entitled 'The nonuniqueness of phonemic solutions of phonetic systems' in the *Bulletin of the Institute of History and Philology, Academia Sinica*, a journal that was not on the regular reading lists of many North American linguists ([Chao 1934]). For that matter, it was received in very few North American libraries.[14] At a later time some linguists—particularly graduate students—began to worry a little about some of the claims commonly made on behalf of linguistics—often by their teachers. Chao's paper began to attract some interest. Still, it was hard to obtain and few actually read it. I remember hearing of it—not once but repeatedly—from people who had heard about it from someone who had allegedly read it. It was said that it had been duplicated and that copies were circulating; I never saw one. (It was before the days of cheap and easy photocopying, and before the days of a highly active underground press in linguistics.) … Then in 1957 Joos published *Readings in Linguistics* ([Joos 1957]), and with this, Chao's paper became easily and generally available—bearing an imprimatur as a fundamental paper in the development of Bloomfieldianism. (Gleason 1988:56)

The later Bloomfieldians were not immune to holding onto important manuscripts for years before publishing them. Randy Harris (1993:271) notes that Zellig Harris's *Methods in structural linguistics* (Harris 1951) 'made the rounds for several years before publication, and Nida's *Synopsis* [*of English syntax* = Nida 1960 [1943]] was available only in thesis form until 1960 and cited personal contact with one linguist or another as the source for some idea'.

Throughout the 1960s, the principal rival to transformational-generative grammar, at least in North America, was stratificational grammar. Getting enough published information on the theory to render an informed judgment about it was painfully difficult. Emmon Bach complained: 'Lamb has presented various facets of his system before in lectures but aside from a semi-official publication of a pedagogical manual there has been little in print on which to base an appraisal of his ideas' (Bach 1965a:276–77). The situation was barely improved with the publication of Lamb's *Outline of stratificational grammar* the following year (Lamb 1966), after it had circulated in manuscript form for several years. This 100-page book was generally considered to be too sketchy and (at

[14] It was apparently the case that well into the 1960s North American libraries did not acquire the full range of European linguistic offerings, and vice versa. As noted in §3 above, E. M. Uhlenbeck complained that Chomsky 1962a and Chomsky 1962b were 'not accessible' (Uhlenbeck 1963:3). But both were in books published by major American university presses.

the same time) too impenetrable to serve as a counterfoil to Chomsky's *Syntactic structures* or *Aspects of the theory of syntax*.[15]

Finally, it should be noted that the paper that most would regard as the foundational document of linguistic pragmatics, H. P. Grice's 'Logic and conversation' (Grice 1975), circulated informally for eight years before seeing publication.

5.6. Summary. At first reading, Robert Stockwell's remark that 'if you weren't on the right mailing lists you could be seriously out-of-date in a few months' (Stockwell 1998:238) sounds quite damning. But we have to raise the question of what it took to get on a mailing list. Stockwell himself recalls that the only requirement was 'sending back comments and questions' and that the MIT linguists were 'unfailingly generous' in sharing their work (quoted in Nielsen 2010:256–57). James Ney did write that the generativist 'underground' literature was 'generally unobtainable' (Ney 1975:143), but he gives no indication of what he did to try to obtain it. I have never heard of a single case where a MIT-based linguist refused to send a copy of his or her work to anyone who asked for one, regardless of their theoretical orientation.

Paul Chapin, a 1967 MIT Ph.D., finds that:

> [t]he claim that MIT restricted distribution of working papers in those days, though, is laughable. The complaint at the time was the opposite, that people were being bombarded by a constant stream of papers from MIT faculty and students, more than they had the time or the interest to read. (Paul Chapin, p.c., October 1, 2012)

In sum, with a minimal amount of effort, any linguist in the world could have had access to the vast majority of the writings of the vast majority of generative grammarians.

6. Further examples of MIT outreach. The rapid publication of their research results was the most important means by which the early generative grammarians diffused the new theory, but it was not the only means. Scholarly publications were supplemented by textbooks (§6.1), participation at the LSA and other professional meetings (§6.2), teaching at Linguistic Institutes (§6.3), and hosting at MIT interested visitors from around the world (§6.4). Section 6.5 discusses the adoption of the theory by non-MIT linguists in 1960s as well as the impact of the first class of MIT Ph.D.s finding employment at universities all over the United States.

6.1. Textbooks. The first textbook in generative grammar appeared even before the first class of MIT linguists received their Ph.D.s, namely Emmon Bach's *An introduction to transformational grammars* (Bach 1964). It was an instant success, all of its reviews with which I am familiar giving a glowing account (see Hale 1965, Montgomery 1965, Peng 1965). What is especially noteworthy from our point of view is that Bach had no affiliation, then or earlier, with the linguistics program at MIT. Nevertheless, he 'especially' thanks Noam Chomsky and Paul Postal 'for comments and suggestions' (Bach 1964:vi). Quite obviously, his being an outsider to MIT did not prevent faculty members there from taking a keen interest in his work. Despite that fact, Nielsen could write:

> The market for linguistics textbooks in 1960s America was largely uniform, with one exception: at MIT only authors with close links to that institution were looked upon favorably. While Bach's book was widely used at universities across the country, it was 'officially ignored' at MIT—a result of its author having never been part of the MIT establishment. (Nielsen 2010:215)

[15] Peter Reich recalls (p.c., October 17, 2012) that 'most SG people found that their papers were rejected by mainstream journals'. Be that as it may, at the time there were plenty of 'non-mainstream' journals and other publishing outlets that Lamb and others could have turned to. Leafing through the linguistics journals of the 1960s reveals literally hundreds of nongenerative and antigenerative publications.

A footnote at this point states 'Bach, *Interview*'. However, its scope is not clear. Are the quotes around 'officially ignored' meant to imply that these were Bach's own words? Or were they Nielsen's scare quotes? In any event, I doubt that the failure to use Bach's textbook was a reflection of his not being an establishment figure. I was a guest at MIT a few years later (academic year 1968–69), and I do not remember ANY class there using a textbook. As David Perlmutter recalls: 'The syntax courses had no basic curriculum; for the most part, each professor talked about what he was currently working on, with little regard for what the students knew or understood. In this, I think, they were basically copying Noam's teaching style, since that is what he did in his courses' (p.c., February 5, 2013). Morris Halle (2011) states that the great 'innovation in teaching methods' at MIT was that 'teaching was mainly conversations between people who are interested in the [same] problems'. Graduate students all had their own offices. They were told that 'if you want to learn something, you'd better hang around the office and you might learn something that nobody is going to teach you in your class'.

That same year saw the publication of another textbook, Paul Roberts's *English syntax: Alternate edition* (Roberts 1964). Roberts's book was geared toward the applied linguistics market as much as Bach's was toward the market for books introducing theoretical linguistics. Roberts noted in the acknowledgments:

> Professor Chomsky cooperated with me closely in the preparation of the text. He read the first two versions, and we discussed them at great length, after which considerable revisions were undertaken. Problems of terminology were solved by agreement between us. (Roberts 1964:vii)

Chomsky, whose passion for applied linguistics has never been thought to be excessive, even contributed an introduction to the book (Chomsky 1964b).

6.2. LSA AND OTHER MEETINGS. The outreach by MIT linguists is manifested in their participation at the summer and annual meetings of the Linguistic Society of America (LSA) in the late 1950s and early 1960s. Chomsky was absent from the LSA program in most of those years, though the slack was taken up by others in his department. At the winter meeting in Chicago at the end of 1957—just months after the publication of *Syntactic structures*—Applegate, Halle, Lees, and Matthews gave talks. All four of them, along with Edward Klima, presented their work at LSA meetings in the following few years. But the most extraordinary fact is the number of MIT students who presented at the meetings. Such is in part a consequence of the fact that the Ph.D. program had an informal requirement that in the course of their graduate studies students should submit an abstract to the LSA (James McCawley, p.c., July 1982). In 1963 and 1964 over half of the graduate students at MIT gave LSA talks: Thomas Bever, Bruce Fraser, Barbara Hall, Paul Kiparsky, Terence Langendoen, James McCawley, Stanley Petrick, Peter Rosenbaum, John R. Ross, Sanford Schane, and Arnold Zwicky. What is especially noteworthy about this fact is that prior to the early 1960s, it was primarily established figures in the field who presented at the annual LSA meeting. The first group of students at MIT initiated a tradition of student presenters that continues to this day.[16]

Chomsky, despite his absence from LSA meetings, accepted a number of speaking invitations at conferences and institutes. In 1958 he gave a Forum Lecture at the LSA Linguistic Institute at the University of Michigan, making him at twenty-nine years of

[16] Archibald Hill wrote that before 1961, 'papers [at LSA meetings] by students had been at least extremely rare, if not unknown' (Hill 1991:101). However, Julia Falk has pointed out to me (p.c., October 19, 2013) that Hill exaggerates here. As she noted, the *LSA Bulletin* 21, 1948, reports that at the 1947 LSA summer meeting a resolution was passed commending the Summer Institute of Linguistics for 'its impressive series of publications' and for 'the papers presented by its students at this meeting' (p. 4).

age perhaps the youngest scholar ever to be accorded that particular honor. Two of his conference papers are legendary: one at the Third Texas Conference on Problems of Linguistic Analysis in English in 1958 (published as Chomsky 1962b)[17] and the other as one of four plenary speakers at the Ninth International Congress of Linguists in 1962 (published as Chomsky 1964a and elsewhere). Commentators generally agree that these presentations and the resultant publications were instrumental in broadcasting the theory of transformational-generative grammar to the outside world of linguists. The efforts extended to nonlinguists as well. To give just one example, in 1960 Chomsky and Halle were on the program of a symposium in New York entitled 'The Structure of Language and its Mathematical Aspects', which was sponsored by the American Mathematical Society. The publications Chomsky 1961a and Halle 1961 resulted from this symposium.

Again, their zeal for presenting their work orally as well as in written form indicates that the first linguists at MIT were anything but inward looking.

6.3. LINGUISTIC INSTITUTES. Since 1928 the LSA has sponsored Linguistic Institutes, held during the summer at some university campus. Before the 1970s, at a time when linguistics departments were rather rare in the United States, they were especially important in allowing students and faculty who might have been rather isolated at their own institutions to learn theory and methodology from some of the leading scholars in the field. They were held yearly in the 1960s. The teaching staff at each Institute is composed of local faculty members, supplemented by distinguished visitors from other institutions, all subject to the approval of the LSA's executive committee.[18] One does not 'volunteer' to teach at an Institute, so the absence of MIT faculty from the list of teachers during the first few years after 1957 should not be taken to imply a lack of willingness to teach. The University of Texas hosted the Institutes in 1960 and 1961. At the first, Lees, who was by then working at IBM, 'took part in a seminar on current linguistic theory' (*LSA Bulletin* 34, 1961, p. 22). The first full course in generative grammar was offered at the 1961 Institute by Emmon Bach. Archibald Hill, who was both secretary-treasurer of the LSA and director of the two Texas Institutes, recalled:

> Academically, these were the years in which the full impact of transformational analysis was for the first time felt by the linguistic community, and the Institute did its best to see that transformational analysts were fully heard. Indeed, the campus joke of those years was that linguists were divided into transformers, resistors, and transistors. (Hill 1964:11)

Generative grammar has been represented in the course offerings at every succeeding Institute, except for the ones held in 1962 at the University of Washington and 1967 at the University of Michigan, and even at the latter one Morris Halle and Charles Fillmore delivered invited Forum Lectures.

[17] Chomsky cannot be blamed for the four-year publishing delay. In fact, Archibald Hill, who organized four yearly conferences in the 1950s, did not succeed in publishing the proceedings of the first and second conferences (1956 and 1957) until 1962 either. Chomsky was invited back to the fourth conference in 1959 to talk on generative phonology: 'The proceedings never reached publication, and the early Chomskyans for the most part believed that the conference organizers had suppressed them. Chomsky evidently tried unsuccessfully to get Hill to release his 1959 paper for Fodor and Katz's important anthology, *The structure of language* (1964), which only added fuel to the complaints' (Harris 1993:272).

[18] In recent years the LSA executive committee has exerted fairly strict control over the Institutes, monitoring the content of the courses offered and the roster of visiting faculty members. Julia Falk has suggested to me (p.c., October 21, 2013) that in earlier decades the host institution was able to act more independently from the LSA in the running of the Institutes. This change, which was probably gradual, would be an interesting topic for further research.

6.4. Visitors to mit. An important part of the attempt on the part of MIT linguists to spread the theory of generative grammar to as wide an audience as possible was to host a steady stream of visitors, partly for the exchange of ideas that such entailed and partly in the hope that they would return to their home institutions to implant knowledge of the new theory among their colleagues and students. Three foreign linguists were visiting professors at MIT in the 1960s: the Indo-Europeanist Jerzy Kuryłowicz from Krakow (in 1962–63 and 1964–65), the classicist and stylistics specialist Peter Colaclides from Athens (1964–65), and the Sanskritist J. Frits Staal from Amsterdam (1966–67). According to Paul Kiparsky:

> 'Outsiders' like Kuryłowicz, and some years later Frits Staal, were invited to teach courses. We were skeptical in some cases, but not hostile, and their ideas ultimately had an impact. Kuryłowicz was pushing markedness, and some us thought it was an important idea, and years later it was adopted in Ch. 9 of [*The sound pattern of English*]. Another year he lectured on IE accent and ablaut. Both his courses meant a lot to me, and he wrote them up into books that are considered classics. Staal's course on Panini changed my life. (Paul Kiparsky, p.c., December 2, 2012)

There were three different levels of 'visitorship' at MIT below the professorial level in the 1960s. The highest in terms of recognition had the title 'Visiting Scientist'. It is easy to track down their names, though not always their external affiliations or linguistic interests, because they were listed above every linguistics article published in a *QPR*. Among those who arrived from abroad were M. J. Chayen and Neil Smith from Great Britain, Anna Wierzbicka from Poland, Jean-Claude Milner and Nicolas Ruwet from France, and Ray Cattell from Australia. Unfortunately, no records survive of those who, like me in 1968–69, had the title 'Guest of the Research Laboratory of Electronics' or of those who simply spent a few weeks or months at MIT attending classes and interacting with faculty and students, but without an official title. My estimate is that there were at least a dozen individuals each year in the second and third categories.

The recollections of the Visiting Scientists about their time spent at MIT in this period are almost entirely glowing. Neil Smith writes:

> MIT was a revelation. There was huge enthusiasm, appallingly hard work, and remarkable talent. I found myself again a neophyte, but being a post-doctoral 'Visitor' I was spared the ignominy of having to turn in term papers proving my inadequacy. The worst embarrassment was discovering that the nice man I'd tried to explain 'generative grammar' to at a welcoming reception was Paul Kiparsky. I had gone to MIT because of Chomsky, but when I arrived, he was away. Fortunately, Morris Halle took me under his wing, and in due course I became a phonologist. The riches on offer were remarkable: courses by Roman Jakobson on language and poetics … (Smith 2002:266)

Below are the personal reminiscences of three other Visiting Scientists from that period:

> My year as an NSF post-doc at MIT was the most important year of my professional life … I didn't feel at all like an 'outsider' at MIT. The MIT people were the most thoroughly professional academic colleagues I ever encountered, before or since my time there. Neither the faculty nor the graduate students treated me with disdain for what I didn't know (which was plenty); rather, they were interested in and respected me for what I did know, and were very helpful in pointing me to sources where I could learn more about my weak areas. (Donald Freeman, p.c., October 22, 2012)

> I was very much welcomed by the students … I was 'correctly' received by the faculty. But I understood rapidly that Halle considered that I did not belong there. That said, he was right and he gave me a chance. I needed one or two months to understand how everything functioned … One could say that I succeeded, because I published two papers in the [*QPR*] … I had two meetings with Morris Halle about my phonology articles. At that point I understood that his reservations at the beginning had disappeared. (Jean-Claude Milner, p.c., October 19, 2012; my translation from the French)

> It blew my mind to be sitting in lectures by Chomsky, Halle and others, and I still regard it as the most important year in my life, as far as intellectual development was concerned. (Ray Cattell, p.c., March 20, 2013)

One Visiting Scientist, however, had an unpleasant experience. For Anna Wierzbicka, the memory of the year she spent at MIT (1966–67) was 'painful' (p.c., October 26, 2012). What distinguished her from the others is that she arrived committed to an alternative theory of grammar based on Andrzej Bogusławski's conception of universal semantic primes (see Bogusławski 1970, Wierzbicka 1972, and much later work). She 'did not expect that linguists at MIT would be necessarily receptive to [this theory]'. At the beginning of the year she tried to convince John R. Ross and George Lakoff that deep structure was both semantic and universal. 'Both Haj and George listened to me politely but told me that Bogusławski and I were too radical and that they wouldn't want to go that far themselves'. Wierzbicka continued: 'You can imagine my shock when one day [they] announced [in their jointly taught class] that they were abandoning Chomsky's position on the relationship between syntax and semantics, and rejecting a syntactic "deep structure" in favour of a deeper, universal, semantic deep structure. … They didn't refer to our conversations at all'.

6.5. Generative grammarians outside of mit. It seems implausible to me that success could have been achieved so rapidly without non-MIT linguists actively contributing to the theory. Certainly then, 'outsiders' had enough access to the relevant published (and unpublished) materials to enable them to write convincingly on the theory. By way of confirmation, in 1965 William Orr Dingwall published a book entitled *Transformational generative grammar: A bibliography* (Dingwall 1965) with 962 entries. Many of these entries were works that criticized generative grammar, and some, it must be said, had no apparent relationship to generative grammar at all. However, subtracting these, there were still hundreds of generative publications extant in 1965. The vast majority of them were written by scholars who had no connection with MIT, thereby illustrating the success of MIT outreach.

Language, the journal of the LSA, was, then as now, the most widely distributed linguistics journal in the world. Consider how many full articles were published in *Language* between 1957 and 1969 that either presupposed or argued for a generative approach to the topic in question.

- Vol. 36, 1960: Lees, Stockwell
- Vol. 37, 1961: Smith
- Vol. 38, 1962: Bach
- Vol. 39, 1963: Lees & Klima, Katz & Fodor
- Vol. 40, 1964: Klima, Zeps, Smith, Katz, O'Neil
- Vol. 41, 1965: Foley, Saporta, Gleitman, Closs, Langacker, Fromkin, Klein
- Vol. 42, 1966: Langendoen, Harms, Schane
- Vol. 43, 1967: Chomsky, Stanley, Bach, Foley, Kiparsky, Voyles, Malone, Doherty & Schwartz, Gruber
- Vol. 44, 1968: McKay, Langacker, Weinstock, Hasegawa, Kuroda, Cressey, Huntley, Wang, Schane, Voyles, Schwartz, Anshen & Schreiber
- Vol. 45, 1969: Contreras, Dixon, Rigsby & Silverstein, J. Harris, Langacker (a), R. Lakoff, Labov, Fasold, Huddleston, Langacker (b), Cairns

One is struck by the number of papers, especially in the later years, by linguists who had not been members of the MIT department or long-term visitors to it. Clearly, they were able to access the generative work that they needed well enough to pass review in a highly competitive journal.

And it was not just the journal *Language* in which generative-related articles appeared. The first volume of *Journal of Linguistics*, published in the United Kingdom and the organ of the Linguistics Association of Great Britain, appeared in 1965. Four of the six main articles dealt with generative grammar, either supporting it (by Chomsky & Halle, James Thorne) or criticizing it (by Householder, Matthews). Thorne, Householder, and Matthews, none of whom had direct MIT connections, were able to inform themselves sufficiently about the ins and outs of the new theory.

It is uncontroversial, I think, that the most important journal article in generative syntax published between 1957 and 1965 was Charles Fillmore's 'The position of embedding transformations in a grammar' (Fillmore 1963). This article laid the basis for the transformational cycle and the successor mechanisms that are still assumed to be correct by the great majority of generative syntacticians (for discussion, see Freidin 1999).[19] Fillmore had never been to MIT. He remarked to me (p.c., sometime in the 1980s) how 'nurturing' the MIT faculty had been to him in his research project, singling out Paul Postal, who is cited in the acknowledgments to that paper, as being especially helpful. As I was writing this paper, Fillmore was unavailable for confirmation of his verbal comment, but Paul Postal remarked in an email (overly modestly, I suspect): 'I can't actually remember reading and commenting on Chuck's work, but it would not surprise me if I did and I certainly know of nothing which suggests he is mistaken or making anything up' (Paul Postal, p.c., July 25, 2012).

As stressed in Newmeyer 1986:41–43, an important factor in the success of the theory is the happy fact that all of the first Ph.D.s in linguistics from MIT found employment at major universities. The late 1960s saw the biggest expansion of the American university system in history, so there were sufficient funds to create departments of linguistics where none had existed before: 'It did not matter that Hockett and Hall were at Cornell, Trager and Smith at Buffalo, or Harris and Hiż at Pennsylvania. New departments could always be founded to serve as academic bases for generative grammar from the very beginning' (Newmeyer 1986:43). But Nielsen implies incorrectly that generative-oriented departments were founded mainly by MIT linguists or their students:

> In the 1960s, transformational theory was brought to the University of California at San Diego by Edward Klima, to the University of Chicago by James McCawley, to the University of Illinois by Robert Lees, to Indiana University by Andreas Koutsoudas, to the University of Massachusetts by Robert Binnick, to Ohio State University by Arnold Zwicky, to the University of Texas at Austin by Stanley Peters, to the University of Washington by Frederick Newmeyer, and, of course, to UCLA by Robert Stockwell. Many of these young transformationalist-oriented programs quickly built productive graduate programs, training large numbers of students in transformational theory. (Nielsen 2010:281–82)

Most of these statements are incorrect. Leonard Newmark and David Reibel, both early enthusiasts of generative grammar, were already at San Diego when the first MIT linguist, Sanford Schane (not Edward Klima), was hired in 1965. The theory was hardly 'brought' to Indiana by Andreas Koutsoudas, since he was already on the faculty when he adopted it. The same can be said about Fred Householder, a long-time Indiana faculty member, who was quite enthusiastic about generative grammar for a while and even directed George Lakoff's 1965 Ph.D. dissertation. The Massachusetts department was founded by Donald Freeman, who had had a postdoc at MIT (see above) and was the one who hired Binnick and several other generativists. Charles Fillmore at Ohio State had independently adopted generative grammar in the early 1960s before he hired Terence Langendoen from MIT in 1965 (Arnold Zwicky did not arrive there until 1969). Emmon Bach and Robert Harms, neither MIT-trained linguists, had established

[19] The first proposal for a syntactic cycle appears in Chomsky, Halle, & Lukoff 1956, where it is applied to the handling of sentence stress. This paper had no influence on future syntactic theorizing, however.

a generative presence at Texas well before Peters joined the faculty. When I was hired at Washington in 1969, Sol Saporta and Heles Contreras, neither of whom had ever been to MIT, had created a generative-oriented department there. Finally, Stockwell (along with Paul Schachter and Victoria Fromkin) turned the UCLA program into a largely generative one before the hiring of its first MIT Ph.D., Barbara Partee, in 1965. The fact that they had had no affiliation with MIT shows how simple it was to master the theory without direct MIT guidance. None of Nielsen's mistakes taken individually are particularly significant or even historiographically very interesting. But they need to be addressed because their cumulative effect is to reinforce the idea that there was once a 'decade of private knowledge'.

7. Some explanations for the spread of the 'private knowledge' idea. When I suggested recently to some 1960s-era MIT linguists that they were often perceived as members of an inward-looking clique, the reaction was incredulity. Here are two typical comments:

> So it is kind of vague in my mind who a putative clique of MIT people dismissive of outsiders might have included. It is true there was a lot of circulating of those horrible purple papers, and no doubt the circle to whom they were sent was not huge. But that was probably due essentially to the physical difficulties of producing and mailing them more than any desire to keep them exclusive. … So in sum, while I am obviously terribly vague on details, the idea that MIT people formed a sort of closed clique strikes me as entirely wrong. If that had been the case, the doctrines would hardly have so successfully spread and influenced people. What is no doubt true is that lots of linguists external to the MIT world which developed resented the increasing influence it had and the way its ideas were spread by young people like me and others in sometimes offensive and ill-considered ways. (Paul Postal, p.c., July 25, 2012)

> [The idea that we] tried to keep [our ideas limited to a small circle] strikes me as flat wrong. We were as hungry for recognition as anyone; if we could have blogged this stuff, we would have. It's just that these things were, most of them, rough drafts (many expanded into, or incorporated into, regular publications). (Arnold Zwicky, p.c., October 6, 2012)

And yet, the perception persists of an off-putting MIT isolationism. Why would that be? There are three principal reasons, I believe. First, the rapid pace of change in the field meant that not everybody outside of MIT could access the most recent work as rapidly as those on the inside could (§7.1). Second, the public behavior of some early generativists was extremely aggressive at times, and when not aggressive, it could be very insular (§7.2). Third and closely related, the self-identity of the students at MIT in the 1960s was very much wrapped up in the idea that they, and they alone, represented the future of linguistics (§7.3).

7.1. The consequences of a rapidly evolving discipline. The 1960s, and in particular the earlier part of that decade, was a time when the theory was evolving at an extremely rapid pace. As Thomas Bever observed,

> during this period virtually EVERY grad student paper introduced something novel into the field; we were all busy working out this brand new paradigm and everyone could make almost immediate contributions. That made the period intoxicating for the students, but frustrating for people who were not right there on the spot. (Thomas Bever, p.c., October 31, 2012)

Naturally, generative linguists cited each others' unpublished work. It would have been unethical not to cite some paper, whether published or not, for an original idea. So given the higher percentage of 'original ideas' then than there are now, the absolute percentage of references to unpublished work might well have been higher fifty years ago than it is today. That certainly could have helped to fuel the idea of 'private knowledge'. But there is no reason to think that the time lag between the completion of a paper and its formal publication was any longer then than it is now. It is just that the rapid tempo of theoretical change at the time made it more inevitable that work not yet sent off to press would be cited.

7.2. Aggressive and insular public behavior. In the last sentence of his quote in §7, Paul Postal puts his finger on one of the reasons that the early generativists were often seen as members of an inward-looking clique. In a nutshell, more than a few were badly behaved in public. Well before 1960, Lees's behavior at meetings led him to be seen as the hatchet man of generative grammar:[20]

> A very direct man, [Lees] employed a style calculated to shock and enrage which he now describes (with characteristic bluntness) as 'getting up at meetings and calling people *stupid*'. These tactics made him a legend among the transformationalists, but they did not endear him to the other side; Householder cautiously begins a review of Lees's *Grammar of English Nominalizations* with the remark that Lees 'is noted as a redoubtable scholarly feuder and cutter-down to size' (Householder 1962:326), probably the mildest terms used by his opponents. (Harris 1993:72)

By the early 1960s Paul Postal had eclipsed Lees in his degree of rhetorical vituperativeness:

> Postal was even less loved by the Bloomfieldians. Like Lees, he is warm and genial in personal settings, and quite tolerant of opposing viewpoints. But his reputation for intellectual savagery is well-deserved, rooted firmly in his public demeanor at conferences, especially in the early years. The stories are legion, most of which follow the same scenario. Postal sits through some anonymous, relatively innocuous, descriptive paper cataloguing the phonemic system of a little-known language. He stands up, begins with a blast like 'this paper has absolutely nothing to do with the study of human languages', and proceeds to offer a barrage of arguments detailing its worthlessness—often making upwards of a dozen distinct counter-arguments against both the specific data used and the framework it is couched in. (Harris 1993:72)

And consider the following:

> An example of rare disapproval of excessively vehement discussion followed the Joos paper [at the 1963 annual LSA meeting], which was a sketch, with a handout, of p. 76 of *The English Verb: Form and Meanings* (Joos 1964). An over-enthusiastic devotee of the latest type of grammatical theory [Paul Postal] attacked the paper as essentially ignorant; both the audience and the presiding officer disapproved, and it was ruled that the discussant's remarks were off the record. In consequence, his name does not appear at its proper place, p. 24, in *Bulletin* 37. (Hill 1991:122)

The style that advocates of the new theory often assumed in public was not limited only to those linguists trained at MIT. It even reproduced itself on the otherwise laid-back West Coast. As Victoria Fromkin recalls:

> [T]he early years following the publication of *Syntactic Structures* were exciting ones; the 'revolution' had begun. The weekly linguistics seminars at the Rand Corporation in Santa Monica more resembled the storming of the Winter Palace than scholarly discussions. (Fromkin 1991:79)

One assumes that she was referring to herself and Robert Stockwell as being the two principal stormers. Neither had ever been known to mince words.

Even when the behavior of the generativists at public meetings was not openly aggressive, and it was typically not, it was sometimes reflective of an in-group mentality that was quite off-putting to more than one participant. Comments like 'If you had read Paul's (or George's or Dave's, or whosever) paper on such-and-such, then your analysis would have been better' were not uncommon. Needless to say, the presenter would not be likely to know which Paul (or George or Dave) was being referred to, much less the

[20] To call Lees's personality 'abrasive' does not do it sufficient justice. Morris Halle told me that Lees had the same style when addressing administrators at MIT: 'If a day went by for Lees without his insulting somebody in a position of power, then he would consider that day a failure' (Morris Halle, p.c., December 17, 1978).

content of his paper. Or just as bad, 'a current MIT grad student would start [his or her conference paper] with something more or less equivalent to, "I presuppose the version of [the theory] presented in Chomsky's lectures this semester" ' (Barbara Partee, p.c., October 31, 2012). Good luck if you were not at the lectures. And recall H. Allan Gleason's remark (§3) that:

> an objection [to a generativist paper at an LSA meeting] would be answered by the simple assertion that the matter had been taken care of and warranted no further discussion. This would be supported by the citing of a paper in the underground press—a paper whose existence the objector had no way of knowing about, and which he would for some years have no opportunity to see. (Gleason 1988:65–66)

It is not at all clear to me whether the progress of the theory was advanced or retarded by the rhetorical style of some of its advocates. It was certainly off-putting to many, but at the same time, especially for students, it created a mystique around members of the MIT group that quite a few young people (like myself at the time) found very engaging. One thing is clear: it helped to set the early generative grammarians apart as a distinct group with a distinct agenda, and thereby contributed to the idea that they were possessors of 'private knowledge' denied to the linguistic public at large.

7.3. MIT STUDENT MENTALITY. The first students in the MIT linguistics program in the early 1960s were a breed apart. They had to be. It took courage, prescience, and a spirit of adventure for a high-achieving college senior to seek out as a mentor a relatively unknown junior scholar based in an institution known almost exclusively for science and engineering.[21] As David Perlmutter, a 1968 MIT Ph.D., has written:

> The graduate program in linguistics, begun in 1961, tended to attract students with an iconoclastic bent at a time when students who wanted to play it safe saw the upstart program at MIT as a risky bet. They went to Cornell to study with Hockett, to Yale to study with Bloch, to Berkeley to study Amerindian linguistics, or to other programs that promised a more secure career. (Perlmutter 2010:xix)

Not surprisingly, then, many MIT students felt imbued with a sense of destiny and of their own importance. It would be up to them, and them alone, to remake the field of linguistics by diffusing the theory of transformational grammar to the rest of the world.[22] In Barbara Partee's words: 'We all felt like pioneers in an exciting new venture' (Partee 2005:4). Such a feeling was marvelous, needless to say, in creating an esprit de corps and the self-confidence necessary to propagate the theory. Another positive effect was that it extended to the hearty welcoming of visitors from outside MIT, at least those who arrived there for instruction in the local brand of linguistics (see above, §6.4). Anybody who was officially part of the MIT coterie was automatically regarded as a colleague. But on the negative side, it led MIT students (though, interestingly, not MIT faculty) to be skeptical that anybody could have truly mastered the theory who had not spent hours and hours probing its intricacy within the walls of MIT's Building 20. Emmon Bach felt this condescending attitude very keenly:

> Early encounters with various MIT students … invariably had this sort of 'where did you come from' and 'how did you find out' cast about them. I availed myself of Fillmore's rejoinder: 'Well, I can read!' The last comment of this sort was from my colleague Joan [Bresnan] at UMass much later, when she paid me the supreme compliment of telling me she had thought I had an MIT degree for the longest time! I think Barbara [Partee] went off to UCLA with a certain missionary fervor about her. (Emmon Bach, p.c., July 25, 2012)

[21] Morris Halle (2011) tells a story, perhaps apocryphal, of the reaction of MIT President Howard Johnson when told that MIT had just been named as having the best doctoral program in linguistics in the United States. Johnson replied: 'We have a linguistics program?'.

[22] And to a significant degree they succeeded. As of this writing, seven MIT linguistics Ph.D.s from the 1960s (and four more from the 1970s) have been elected president of the LSA.

Robin Lakoff, who was a Ph.D. student at Harvard in the early and mid 1960s, recalls the behavior of members of the audience at MIT when unsympathetic visitors from the outside world came to speak:[23]

> I remember well the times that non-transformationalists would speak at MIT, in those early years when the field still saw itself as fighting for survival in a hostile world. Rather than attempting to charm, conciliate, find points of connection, the circle at MIT regularly went for blood. Points were made by obvious public demolition; the question or counterexample that brought the offender to his knees [was] repeated for weeks or months afterwards with relish. (Lakoff 1989:967–68)

When I first read this passage, I assumed that Lakoff was referring primarily to MIT students since, aside from Paul Postal, the core linguistics faculty members were generally polite (albeit forceful) in professional interactions. It was typically the students who 'went for blood'. Lakoff confirmed my assumption:

> Most of the hostility towards non-generative visiting speakers was done by grad students (and postdocs), who vied with one another to be both theoretically and personally obnoxious to anyone not of the MIT community who came to give a talk. … The students aren't really to blame. They were encouraged by their teachers and the 'bad guys' perspective,[24] and MIT was generally both a closed-in and a highly competitive place. (Robin Lakoff, p.c., October 26, 2012)

Barbara Partee, an MIT Ph.D. student in the early 1960s, confirms Lakoff's assessment of student behavior in that period:[25]

> This note is to back up Robin Lakoff's impression. I was kind of shocked when I saw what happened when the faculty invited non-generative visiting speakers—I have vivid memories of [one MIT faculty member] making audible comments and snickering to whoever was sitting next to him or behind him, in ways the speaker must have been able to see and hear. That was the model we were exposed to. I didn't like it—that wasn't the way I had been brought up. I readily learned about being critical, but I hope I didn't learn to be rude. I remember when Sydney Lamb came and gave a talk about his theory of Stratificational Grammar. I had read one of his articles in preparation, I think, and thought (naively) that I could offer him some counter-arguments that would lead him to abandon that theory. So I made an appointment to talk with him and gave him my arguments. I don't remember much about the conversation (but he didn't abandon his theory). What I do remember is my classmates teasing me mercilessly for several days thereafter as a 'Lamb-lover' just because I had made an appointment to talk to him. (Barbara Partee, p.c., October 28, 2012)

[23] Lakoff goes on to write: 'But by 1964, certainly, the battle was won. No more opponents came riding into Cambridge eager to joust with the champion' (Lakoff 1989:968).

[24] The class at MIT covering nongenerative approaches was universally known as 'The Bad Guys Course'. Chomsky writes:

> In the early '60s, I was working on topics in history of linguistics/philosophy that are discussed in *Current Issues*, *Aspects*, *Cartesian Linguistics*, *Language and Mind*. And as usual taught a course on what I was working on. It became pretty clear that students weren't interested, so I handed it over to junior faculty to run it as they and the students liked. They changed it to a course on contemporary critique of generative grammar, which, as you know, was widespread and quite passionate, and other approaches; and it soon came to be called by them the 'bad guys' course. I didn't like that. Morris didn't either. But it was their baby, and we didn't interfere. (Noam Chomsky, p.c., October 28, 2012)

[25] By the end of that decade, and probably before, the older establishment figures in the field were giving as much as they were taking. James Harris, a 1967 MIT Ph.D., recalls: 'At a talk at Cornell (in the late '60s, I think), I was treated to the most viciously nasty unprovoked attack, by a mature looking guy, I've ever witnessed. I didn't know who this jerk was, and I responded in kind (to applause from the audience). Turned out that the assailant was Charles Hockett' (p.c., November 9, 2012). Harris goes on: 'In Berkeley at around the same time, Henry Lee Smith (of Trager-Smith English phonology fame) confided to me that he didn't know "what the hell those MIT guys think they're doing, but it certainly isn't phonology." Smith was sure of this because he and Trager were among the wise men who had "founded modern phonology" (his words). So much for the kindness of strangers (neither of whom was a visiting speaker at MIT)'.

For Paul Kiparsky, however, 'there was no habitual hostile treatment of people from outside of MIT who came to give talks'. Rather,

> people were just excited about engaging different ideas. For example, when Gilbert Harman's 'Generative grammars without transformation rules: A defense of phrase structure' came out in *Language* 1963, an emergency meeting of the department was called almost immediately and we went through the whole thing in detail until we concluded to our great relief that it was a 'notational variant' of transformational grammar (whether rightly so, I can't tell). (Paul Kiparsky, p.c., December 3, 2012)

Kiparsky also disputes Lakoff's assertion that MIT was in any way 'closed-in':

> As for 'closed-in', to me it felt the exact opposite. Actually it was the most wide open place I've been at before or since. Building 20 housed exciting research such as Jerry Lettvin's, who was very accessible and a hoot to talk to. A number of us sat in on Putnam's courses, as well as psychology courses (Lukas Teuber, Davis Howes, who ran an aphasia project with Geschwind, Eric Lenneberg at Harvard). I went to lots of Indo-European courses at Harvard and interacted with their students. I was very much encouraged to do so by Morris Halle. … As I recall, some students attended math courses, those of Hartley Rogers, among others. (Paul Kiparsky, p.c., December 2, 2012)

Thomas Bever, a classmate of both Partee and Kiparsky, did agree that MIT students could be confrontational at times. However, for him their behavior was purely defensive:

> As to hostility to 'outsiders'. It is of course Robbie's prerogative to express how she felt about treatment of 'outsiders'. But I think that for many of us it was not the 'outsider-ness' that was at issue. During the period 1961–1965 there was a serious intellectual conflict between MITniks and the Linguistics Establishment. Many senior linguists (Hockett, Joos, Gibson, Lehmann, etc.) suddenly found themselves surrounded by a lot of young self-assured critics, with out-of-the-blue arguments that their basic assumptions were wrong and 'unscientific'. LSA meetings involved real strategic pre-planning, discussing ahead who would criticize whom at each meeting. We all felt the presence of a mission. And for our part, we all felt somewhat beleaguered by conservative inertia in the field. (Thomas Bever, p.c., October 28, 2012)

MIT students, either because they wanted to or because they felt that they had to, erected a rhetorical wall between themselves and much of the surrounding community of linguists. One can easily see how such attitudes and behavior contributed to the view that the early generativists shared among themselves some degree of 'private knowledge'.

8. Conclusion. This discussion note has taken on aspects of the early days of the theory of generative grammar, that is the late 1950s and the 1960s. We have seen how MIT faculty and students went about diffusing their ideas to a broader audience. They used every means at their disposal at the time: publishing single-authored books, journal articles, anthology chapters, and technical reports; aiding the writing of textbooks; giving conference talks; teaching at LSA Institutes; and hosting numerous visitors to MIT. All of these measures contributed to the rapid success of the theory. To read some of the critical literature, however, the early MIT linguists formed an elitist in-group, talking only to each other by means of inaccessible 'underground' publications and thereby erecting a barrier between themselves and the outside world of linguistics. We have seen that their tactics for reaching out to the linguistics community as a whole overshadowed any elitist behavior. And in particular, there was no significant 'underground' literature to obstruct the acceptance of the new theory.

REFERENCES

Anshen, Frank, and Peter A. Schreiber. 1968. A focus transformation of Modern Standard Arabic. *Language* 44.792–97.

Bach, Emmon. 1962. The order of elements in a transformational grammar of German. *Language* 38.263–69.

Bach, Emmon. 1964. *An introduction to transformational grammars*. New York: Holt, Rinehart, and Winston.

Bach, Emmon. 1965a. Review of *Report of the Fifteenth Annual (First International) Round Table Meeting on Linguistics and Language Sciences*, ed. by C. I. J. M. Stuart. *Word* 21.273–85.

Bach, Emmon. 1965b. Structural linguistics and the philosophy of science. *Diogenes* 51. 111–28.

Bach, Emmon. 1967. *Have* and *be* in English syntax. *Language* 43.462–85.

Bach, Emmon, and Robert Harms (eds.) 1968. *Universals in linguistic theory*. New York: Holt, Rinehart, and Winston.

Bloomfield, Leonard. 1933. *Language*. New York: Holt.

Bogusławski, Andrzej. 1970. On semantic primitives and meaningfulness. *Sign, language, culture*, ed. by Algirdas Julien Greimas, 143–52. The Hague: Mouton.

Cairns, Charles E. 1969. Markedness, neutralization, and universal redundancy rules. *Language* 45.863–85.

Chao, Yuen-Ren. 1934. The nonuniqueness of phonemic solutions of phonetic systems. *Bulletin of the Institute of History and Philology, Academia Sinica* 4.363–97.

Chomsky, Noam. 1957. *Syntactic structures*. The Hague: Mouton.

Chomsky, Noam. 1959. On certain formal properties of grammars. *Information and Control* 2.137–67. [Reprinted in *Readings in mathematical psychology*, vol. 2, ed. by R. Duncan Luce, Robert R. Bush, and Eugene Galanter, 122–55. New York: Wiley & Sons, 1965.]

Chomsky, Noam. 1961a. On the notion 'rule of grammar'. *Proceedings of Symposia in Applied Mathematics 12: Structure of language and its mathematical aspects*, ed. by Roman Jakobson, 6–24. Providence, RI: American Mathematical Society. [Reprinted in Fodor & Katz, 119–36.]

Chomsky, Noam. 1961b. Some methodological remarks on generative grammar. *Word* 17.219–39.

Chomsky, Noam. 1962a. Explanatory models in linguistics. *Logic, methodology, and philosophy of science*, ed. by Ernest Nagel, Patrick Suppes, and Alfred Tarski, 528–50. Stanford, CA: Stanford University Press.

Chomsky, Noam. 1962b. A transformational approach to syntax. *Proceedings of the Third Texas Conference on Problems of Linguistic Analysis in English*, ed. by Archibald A. Hill, 124–58. Austin: University of Texas Press.

Chomsky, Noam. 1963. Formal properties of grammars. *Handbook of mathematical psychology*, vol. 2, ed. by R. Duncan Luce, Robert R. Bush, and Eugene Galanter, 323–418. New York: Wiley.

Chomsky, Noam. 1964a. *Current issues in linguistic theory*. The Hague: Mouton.

Chomsky, Noam. 1964b. Introduction. In Roberts, ix–xv.

Chomsky, Noam. 1965. *Aspects of the theory of syntax*. Cambridge, MA: MIT Press.

Chomsky, Noam. 1967. Some general properties of phonological rules. *Language* 43.102–28.

Chomsky, Noam. 1972. *Studies on semantics in generative grammar*. The Hague: Mouton.

Chomsky, Noam. 1975 [1955]. *The logical structure of linguistic theory*. Chicago: University of Chicago Press.

Chomsky, Noam. 1975. Introduction. In Chomsky 1975 [1955], 1–53.

Chomsky, Noam. 1982. *The generative enterprise: A discussion with Riny Huybregts and Henk van Riemsdijk*. Dordrecht: Foris.

Chomsky, Noam. 2011. Lecture 4. Paper presented at 50 Years of Linguistics at MIT: A Scientific Reunion, MIT, Cambridge, MA.

Chomsky, Noam; Morris Halle; and Fred Lukoff. 1956. On accent and juncture in English. *For Roman Jakobson: Essays on the occasion of his sixtieth birthday*, ed. by Morris Halle, Horace G. Lunt, and Hugh MacLean, 65–80. The Hague: Mouton.

Chomsky, Noam, and George Miller. 1958. Finite state languages. *Information and Control* 1.91–112. [Reprinted in *Readings in mathematical psychology*, vol. 2, ed. by R. Duncan Luce, Robert R. Bush, and Eugene Galanter, 156–71. New York: Wiley & Sons, 1965.]

CHOMSKY, NOAM, and GEORGE MILLER. 1963. Introduction to the formal analysis of natural languages. *Handbook of mathematical psychology*, vol. 2, ed. by R. Duncan Luce, Robert R. Bush, and Eugene Galanter, 269–322. New York: Wiley.

CHOMSKY, NOAM, and MARCEL PAUL SCHÜTZENBERGER. 1963. The algebraic theory of context-free languages. *Computer programming and formal systems* (Studies in logic and the foundations of mathematics 35), ed. by Paul Braffort and David Hirschberg, 118–61. Amsterdam: North-Holland.

CLOSS, ELIZABETH. 1965. Diachronic syntax and generative grammar. *Language* 41.402–15.

CONTRERAS, HELES. 1969. Simplicity, descriptive adequacy, and binary features. *Language* 45.1–8.

CRESSEY, WILLIAM W. 1968. Relative adverbs in Spanish: A transformational analysis. *Language* 44.487–500.

DINGWALL, WILLIAM O. 1965. *Transformational generative grammar: A bibliography*. Washington, DC: Center for Applied Linguistics.

DIXON, R. M. W. 1969. Relative clauses and possessive phrases in two Australian languages. *Language* 45.35–44.

DOHERTY, PAUL C., and ARTHUR SCHWARTZ. 1967. The syntax of the compared adjective in English. *Language* 43.903–36.

EMONDS, JOSEPH E. 1976. *A transformational approach to English syntax*. New York: Academic Press.

FASOLD, RALPH W. 1969. Tense and the form *be* in Black English. *Language* 45.763–76.

FILLMORE, CHARLES J. 1963. The position of embedding transformations in a grammar. *Word* 19.208–31.

FODOR, JERRY A., and JERROLD J. KATZ (eds.) 1964. *The structure of language: Readings in the philosophy of language*. Englewood Cliffs, NJ: Prentice-Hall.

FOLEY, JAMES. 1965. Prothesis in the Latin verb *sum*. *Language* 41.59–64.

FOLEY, JAMES. 1967. Spanish plural formation. *Language* 43.486–93.

FREIDIN, ROBERT. 1999. Cyclicity and minimalism. *Working minimalism*, ed. by Samuel D. Epstein and Norbert Hornstein, 95–126. Cambridge, MA: MIT Press.

FROMKIN, VICTORIA A. 1965. On system-structure phonology. *Language* 41.601–9.

FROMKIN, VICTORIA A. 1991. Language and brain: Redefining the goals and methodology of linguistics. *The Chomskyan turn: Generative linguistics, philosophy, mathematics, and psychology*, ed. by Asa Kasher, 78–103. Oxford: Blackwell.

GLEASON, H. ALLAN. 1988. Theories in conflict: North American linguistics in the fifties and sixties. Toronto: University of Toronto, MS.

GLEITMAN, LILA R. 1965. Coordinating conjunctions in English. *Language* 41.260–93.

GRICE, H. PAUL. 1975. Logic and conversation. *Syntax and semantics, vol. 3: Speech acts*, ed. by Peter Cole and Jerry L. Morgan, 41–58. New York: Academic Press.

GRUBER, JEFFREY S. 1967. Look and see. *Language* 43.937–47.

HALE, KENNETH L. 1965. Review of Bach 1964. *International Journal of American Linguistics* 31.265–70.

HALLE, MORRIS. 1961. On the role of simplicity in linguistic descriptions. *Proceedings of Symposia in Applied Mathematics 12: Structure of language and its mathematical aspects*, ed. by Roman Jakobson, 89–94. Providence, RI: American Mathematical Society.

HALLE, MORRIS. 2011. Lecture 10. Paper presented at 50 Years of Linguistics at MIT: A Scientific Reunion, MIT, Cambridge, MA.

HARMS, ROBERT T. 1966. The measurement of phonological economy. *Language* 42.602–11.

HARRIS, FLORENCE WARSHAWSKY. 1976 [1965]. Reflexivization. In McCawley 1976b, 63–83.

HARRIS, FLORENCE WARSHAWSKY, and JAMES HARRIS. 1974. The development of the linguistics program at the Massachusetts Institute of Technology. Online: http://ling50.mit.edu/harris-development.

HARRIS, JAMES W. 1969. Sound change in Spanish and the theory of markedness. *Language* 45.538–52.

HARRIS, RANDY A. 1993. *The linguistics wars*. Oxford: Oxford University Press.

Harris, Zellig S. 1951. *Methods in structural linguistics*. Chicago: University of Chicago Press.

Hasegawa, Kinsuke. 1968. The passive construction in English. *Language* 44.230–43.

Hill, Archibald A. 1964. History of the Linguistic Institute. *ACLS Newsletter* 15.1–12.

Hill, Archibald A. 1991. The Linguistic Society of America and North American linguistics, 1950–1968. *Historiographia Linguistica* 18.49–152.

Householder, Fred W. 1962. Review of *Grammar of English nominalizations*, by Robert B. Lees. *Word* 18.326–53.

Householder, Fred W. 1971. *Linguistic speculations*. Cambridge: Cambridge University Press.

Householder, Fred W. 1980. A sketch of how I came to be in linguistics. *First person singular: Papers from the Conference on an Oral Archive for the History of American Linguistics*, ed. by Boyd H. Davis and Raymond K. O'Cain, 193–99. Amsterdam: John Benjamins.

Huddleston, Rodney. 1969. Some observations on tense and deixis in English. *Language* 45.777–806.

Huntley, D. G. 1968. Two cases of analogical feature substitution in Slavic. *Language* 44.501–6.

Hymes, Dell. 1964. Directions in (ethno-)linguistic theory. *Transcultural studies in cognition* (*American Anthropologist* 66.3, part 2), ed. by A. Kimball Romney and Roy Goodwin D'Andrade, 6–56. Washington, DC: American Anthropological Association.

Jackendoff, Ray. 1972. *Semantic interpretation in generative grammar*. Cambridge, MA: MIT Press.

Jacobs, Roderick A., and Peter S. Rosenbaum (eds.) 1970. *Readings in English transformational grammar*. Waltham, MA: Ginn.

Jakobson, Roman, and Morris Halle. 1956. *Fundamentals of language*. The Hague: Mouton.

Joos, Martin (ed.) 1957. *Readings in linguistics: The development of descriptive linguistics in America since 1925*. New York: American Council of Learned Societies.

Joos, Martin. 1964. *The English verb: Form and meanings*. Madison: University of Wisconsin Press.

Karttunen, Lauri. 1976 [1969]. Discourse referents. In McCawley 1976b, 363–85. [Distributed by the Indiana University Linguistics Club in 1971.]

Katz, Jerrold J. 1964. Mentalism in linguistics. *Language* 40.124–37.

Katz, Jerrold J., and Jerry A. Fodor. 1963. The structure of a semantic theory. *Language* 39.170–210.

Kiparsky, Paul. 1967. Sonorant clusters in Greek. *Language* 43.619–35.

Klein, Sheldon. 1965. Control of style with a generative grammar. *Language* 41.619–31.

Klima, Eward S. 1964. Relatedness between grammatical systems. *Language* 40.1–20.

Koontz-Garboden, Andrew. 2007. *States, changes of state, and the monotonicity hypothesis*. Stanford, CA: Stanford University dissertation.

Kuroda, Sige-Yuki. 1968. English relativization and certain related problems. *Language* 44.244–66.

Labov, William. 1966. *The social stratification of English in New York City*. Washington, DC: Center for Applied Linguistics.

Labov, William. 1969. Contraction, deletion, and inherent variability of the English copula. *Language* 45.715–62.

Lakoff, George. 1965. *On the nature of syntactic irregularity*. Bloomington: Indiana University dissertation. [Published as *Report NSF-16*, 1965, and as *Irregularity in syntax*, New York: Holt, 1970.]

Lakoff, George. 1972 [1970]. Linguistics and natural logic. *The semantics of natural language*, ed. by David Davidson and Gilbert Harmon, 545–665. Dordrecht: Reidel.

Lakoff, George. 1976 [1969]. Pronouns and reference. In McCawley 1976b, 275–335. [Distributed by the Indiana University Linguistics Club in 1969.]

Lakoff, Robin T. 1969. Some reasons why there can't be any *some–any* rule. *Language* 45.608–15.

Lakoff, Robin T. 1989. The way we were: Or, the real actual truth about generative semantics: A memoir. *Journal of Pragmatics* 13.939–88.

Lamb, Sydney M. 1966. *Outline of stratificational grammar*. Washington, DC: Georgetown University Press.
Langacker, Ronald W. 1965. French interrogatives: A transformational description. *Language* 41.587–600.
Langacker, Ronald W. 1968. Observations on French possessives. *Language* 44.51–75.
Langacker, Ronald W. 1969a. Mirror image rules I: Syntax. *Language* 45.575–98.
Langacker, Ronald W. 1969b. Mirror image rules II: Lexicon and phonology. *Language* 45.844–62.
Langendoen, D. Terence. 1966. A restriction on Grassmann's law in Greek. *Language* 42.7–9.
Lees, Robert B. 1960. A multiply ambiguous adjectival construction in English. *Language* 36.207–21.
Lees, Robert B. 1976 [1960]. What are transformations? In McCawley 1976b, 27–41.
Lees, Robert B., and Edward S. Klima. 1963. Rules for English pronominalization. *Language* 39.17–28.
Levin, Samuel R. 1965. Langue and parole in American linguistics. *Foundations of Language* 1.83–94.
Malone, Joseph L. 1967. A transformational re-examination of English questions. *Language* 43.686–702.
Matthews, P. H. 1972. Review of Jacobs & Rosenbaum 1970. *Journal of Linguistics* 8.125–36.
McCawley, James D. 1976a. *Grammar and meaning*. New York: Academic Press.
McCawley, James D. (ed.) 1976b. *Syntax and semantics, vol. 7: Notes from the linguistic underground*. New York: Academic Press.
McKay, John C. 1968. Some generative rules for German time adverbials. *Language* 44.25–50.
Montgomery, Christine. 1965. Review of Bach 1964. *Language* 41.632–40.
Morgan, Jerry L. 1976 [1969]. Cryptic note II and WAGS III. In McCawley 1976b, 337–45.
Murray, Stephen O. 1980. Gatekeepers and the 'Chomskyan revolution'. *Journal of the History of the Behavioral Sciences* 16.73–88.
Murray, Stephen O. 1999. How *The logical structure of linguistic theory* didn't get published during the 1950s or 1960s. *The emergence of the modern language sciences: Studies on the transition from historical-comparative to structural linguistics in honour of E. F. K. Koerner*, ed. by Sheila Embleton, John E. Joseph, and Hans-Josef Niederehe, 261–66. Amsterdam: John Benjamins.
Newmeyer, Frederick J. 1986. *Linguistic theory in America*. 2nd edn. New York: Academic Press.
Ney, James W. 1975. The decade of private knowledge: Linguistics from the early 60s to the early 70s. *Historiographia Linguistica* 2.143–56.
Nida, Eugene. 1960 [1943]. *A synopsis of English syntax*. Norman, OK: Summer Institute of Linguistics.
Nielsen, Janet. 2010. *Private knowledge, public tensions: Theory commitment in postwar American linguistics*. Toronto: University of Toronto dissertation.
O'Neil, Wayne A. 1964. Faroese vowel morphophonemics. *Language* 40.366–71.
Partee, Barbara. 2005. Reflections of a formal semanticist as of Feb. 2005. Amherst: University of Massachusetts, ms.
Peng, Fred C. C. 1965. Review of Bach 1964. *Lingua* 13.291–304.
Perlmutter, David M. 2010. My path in linguistics. *Hypothesis A/hypothesis B: Linguistic explorations in honor of David M. Perlmutter*, ed. by Donna B. Gerdts, John C. Moore, and Maria Polinsky, xvii–xxxvii. Cambridge, MA: MIT Press.
Postal, Paul M. 1964a. *Constituent structure: A study of contemporary models of syntactic description*. Bloomington: Indiana University Research Center in Anthropology, Folklore, and Linguistics.
Postal, Paul M. 1964b. Limitations of phrase structure grammars. In Fodor & Katz, 137–51.
Postal, Paul M. 1964c. Mohawk prefix generation. *Proceedings of the Ninth International Congress of Linguists*, ed. by Horace G. Lunt, 346–57. The Hague: Mouton.

Postal, Paul M. 1971. *Cross-over phenomena*. New York: Holt, Rinehart, and Winston.
Postal, Paul M. 1976 [1967]. Linguistic anarchy notes. In McCawley 1976b, 201–26.
Postal, Paul M. 1979 [1962]. *Some syntactic rules of Mohawk*. New York: Garland.
Reibel, David, and Sanford Schane (eds.) 1969. *Modern studies in English*. Englewood Cliffs, NJ: Prentice-Hall.
Rigsby, Bruce, and Michael Silverstein. 1969. Nez Perce vowels and Proto-Sahaptian vowel harmony. *Language* 45.45–59.
Roberts, A. Hood, and Adam G. Woyna. 1972. *Experiment in fast dissemination of research in selected fields in linguistics*. Arlington, VA: Center for Applied Linguistics.
Roberts, Paul. 1964. *English syntax: Alternate edition*. New York: Harcourt, Brace and World.
Ross, John R. 1967. *Constraints on variables in syntax*. Cambridge, MA: MIT dissertation. [Published as *Infinite syntax!*, Norwood, NJ: Ablex, 1985.]
Saporta, Sol. 1965. Ordered rules, dialect differences, and historical processes. *Language* 41.218–24.
Schane, Sanford A. 1966. The morphophonemics of the French verb. *Language* 42.746–58.
Schane, Sanford A. 1968. On the non-uniqueness of phonological representations. *Language* 44.709–16.
Schwartz, Arthur. 1968. Derivative functions in syntax. *Language* 44.747–83.
Smith, Carlota S. 1961. A class of complex modifiers in English. *Language* 37.342–65.
Smith, Carlota S. 1964. Determiners and relative clauses in a generative grammer of English. *Language* 40.37–52.
Smith, Neil. 2002. Personal history. *Linguistics in Britain: Personal histories*, ed. by Keith Brown and Vivien Law, 262–73. Oxford: Blackwell.
Stanley, Richard. 1967. Redundancy rules in phonology. *Language* 43.393–436.
Stockwell, Robert P. 1960. The place of intonation in a generative grammar of English. *Language* 36.360–67.
Stockwell, Robert P. 1998. From English philology to linguistics and back again. *First person singular III: Autobiographies by North American scholars in the language sciences*, ed. by E. F. K. Koerner, 227–45. Amsterdam: John Benjamins.
Thorne, James P. 1965. Review of Postal 1964a. *Journal of Linguistics* 1.73–76.
Uhlenbeck, E. M. 1963. An appraisal of transformation theory. *Lingua* 12.1–18.
Voyles, Joseph B. 1967. Simplicity, ordered rules, and the first sound shift. *Language* 43.636–60.
Voyles, Joseph B. 1968. Gothic and Germanic. *Language* 44.720–46.
Wang, William S-Y. 1968. Vowel features, paired variables, and the English vowel shift. *Language* 44.695–708.
Weinstock, John. 1968. Grimm's law in distinctive features. *Language* 44.224–29.
Wierzbicka, Anna. 1972. *Semantic primitives*. Frankfurt: Athenäum.
Zeps, Valdis J. 1964. Optional rules in the formation of the Old Church Slavonic aorist. *Language* 40.33–36.

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