

MORE ABOUT URIEL WEINREICH'S FOUR RIDDLES

by

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Twenty years have now passed since Uriel Weinreich died of cancer at barely forty years of age (March 30, 1967). A child of Vilna, he came to America with his family before the War, a breath ahead of the Nazis, eventually becoming Professor of Yiddish Language, Literature and Culture on the Atran Chair at Columbia University. His father, Max Weinreich, a philologist and major historian of Yiddish language and culture, was a light of YIVO (*Yidisher visnshaflikher institut*, "Jewish Scientific Institute," now Institute for Jewish Research), the institution which more than any other is responsible not only for the very existence of Yiddish scholarship but for the establishment of a tradition of respect for Yiddish and its history.

Yiddish has always suffered under an inferiority complex. The tendentiousness of the arguments varied: Yiddish isn't a language at all, Yiddish is bad German, Yiddish has no grammar, Jews should speak Hebrew or the language of the country, not Yiddish. One has heard it all so many times. The simple perception that Yiddish was a *language*, a language that could be studied like German, Latin, or Spanish, a *something* that could be a subject of serious scholarly research, did not become clear until well into the first quarter of the twentieth century, and not widely accepted until later. Men like Max Weinreich, Solomon Birnbaum, Judah Joffe, and Yudel Mark were the old guard, Uriel Weinreich the new. Brilliant, solidly grounded in modern linguistics, raised in the Yiddish language and steeped in Yiddish culture, diplomatic and calm, he had precisely the qualities required to bridge successfully the past and the future of Yiddish scholarship.

Very few academics have accomplished as much as he did, let alone in a professional lifespan cut so short. To the linguistic profession he is known primarily for his work in general linguistics, in particular in the areas of bilingualism and semantics. Most contemporary linguists over the age of fifty would enter him on their list of top ten linguists of the century, though most quite likely would have only the vaguest notion of the *size* of his Yiddish connection. The book *Languages in Contact* (1953) brought his steady hand to a research area--what happens to languages when they are spoken next to each other that forever struggles to escape the linguistic guild's silent suspicion of inexactitude, inexpertness. "Empirical Foundations for a Theory of Language Change," written jointly with William Labov and Marvin I. Herzog (Weinreich et al 1968), made it impossible for us to continue to think about language change as we had before. *College Yiddish* has been the standard textbook since it appeared first in 1949. *The Field of Yiddish* (1952) set a standard for modern Yiddish linguistics that to this day makes it difficult for inferior work to flourish. Yiddish linguistics would be a much different and much poorer thing had it not had the advantages of Uriel Weinreich's attention.

One's respect for him, both for the man and the work, does not diminish with the passing of time. Of how many of us will it be possible to say that? I have always found it profitable to go to his work, to his writings, and to marvel at how well his scholarship stands the test of time. Sometimes I have disagreed with Weinreich's solution to a linguistic puzzle. Such disagreement would have mattered as little to him as it does to me. Endowed with great charm and decency and simple human warmth, he was an extraordinary man with extraordinary virtues.

Two of these virtues were the clarity and intellectual honesty with which he staked out a position. We linguists, worse luck, are not always like that. With Weinreich you always knew what he was saying, and if you disagreed, at least you knew what it was you were disagreeing with. I could use virtually any of his writings to illustrate the point; the one I will use to do so is his article "Four Riddles in Bilingual Dialectology" which appeared in 1963 (Weinreich 1963). Recall what this article was about. (While it is impossible to discuss this article without some recourse to the technical concepts of linguistics and to some jargon of the trade, the non-linguist will not miss much by skipping over the specialized parts of the following presentation.)

He took a look at four features of Yiddish dialect structure, each of which was unusual, unexpected from some point of view or other. The four were vowel length, word-final voicing (i.e. sounds like *b d g* at the ends of words instead of *p t k*), /h/, and confusion of sibilants (*s*-like sounds). He then placed the Yiddish situation against the coterritorial language situation, i.e. he compared the structure of the Yiddish spoken in Poland with the structure of the Polish language, Yiddish in Lithuania with the Lithuanian language, and so on. And what he found was that nothing made much sense. Where, for example, Yiddish had vowel length, the coterritorial languages (Polish, Lithuanian, Rumanian, and the like) sometimes did and sometimes didn't; and where Yiddish didn't have vowel length, the coterritorial languages sometimes did and sometimes didn't. No pattern, no predictability. Similarly for word-final voicing, the presence of absence of /h/, and the loss of the hissing-hushing distinction in sibilants (*sabesdiker losn* as it is called in Lithuanian Yiddish; standard Yiddish *shabesdiker loshn*-language used on the Sabbath). The mix of match and no-match, presented by Weinreich as a kind of map overlay, looks even more like a jigsaw puzzle than does Eastern Europe itself.

The "riddles" referred to in the title of his paper grow out of this jigsaw puzzle. The presence or absence of a linguistic feature in an Eastern European language is simply not a good predictor of whether the Yiddish spoken in the same territory has or lacks that feature. The fit is bad, the match unsatisfactory.

Behind this view of things lies a theory that invokes *external contact* between languages as the primary initiator of linguistic change. The position is articulated by Weinreich with characteristic clarity and vigor:

Imagine a language family A B C D in which language D alone possesses a certain feature *f*. We consider the feature an innovation in D. How did it arise? Was it stimulated by some earlier structural peculiarities of language D? The idea is attractive; the possibility of explaining changes in a language by reference to the structure of the language itself is one of the significant achievements of linguistics in the past few decades. But suppose it turns out further that language D has been used by a population which also speaks another, unrelated language, Q, and that language Q (perhaps like its sister languages, R S T) has long possessed feature *f*. Most linguists, no matter how impressed with the achievements of diachronic structuralism [the study of historical linguistics in its modern, i.e. "structural," guise] so far, would probably still feel relief at the chance to attribute the rise of feature *f* in language D to the contact of D with Q. For the linguist concerned with the integral history of a language, the imitation of outside models remains, by and large, a more plausible cause of change than the pressures within the system. (Weinreich 1963:335).

I favor Weinreich's bias for attributing the cause of most linguistic change to external influence--a bias most mainstream theoreticians do not share. If language A has something unusual about it, say a strange sound or an unexpected case ending, how do we explain the thing? Well, if language A has long lived cheek by jowl with a language B which also has the peculiarity, then surely it is obvious why A acquired that feature: it *borrowed* it. In so easy a case, why reach for some strained "theoretical" explanation? (Having both seen and proffered myself so many such "strained" but theoretically solid explanations of linguistic changes, I constantly recall to myself George Orwell's celebrated *mot*: one must be an intellectual to believe such a thing; no ordinary man could be such a fool.) Whatever one's commitment to a theory, as a historical linguist trying to figure out language change you spell *relief*, Weinreich's word, L-A-N-G-U-A-G-E C-O-N-T-A-C-T. But the preference for attributing change to language contact is only half the story.

Basically there are two reasons why languages change; linguists label these causalities "internal" and "external." (It is not beside the point here to allude to the "hard" and "soft" polarity in other social-science disciplines, though the correlation between internal:external in linguistics is not quite the same thing as hard:soft in sociology.) Internally conditioned changes are those that occur by virtue of the structure of the language itself and because of general processes of historical development; they are, in a sense, evolutionary. Modern historical linguistics has mostly concerned itself with internal causation. Externally conditioned

changes are those that are brought about by contact with other languages or other dialects. Such changes would not have occurred on their own; they are due to outside "intervention." If I may cite an analogy from outside linguistics, a snake has lost the use of its legs through evolutionary development--this is a change brought about by "internal" motivation (in the linguist's sense). A paraplegic has lost use of his legs through trauma, usually an injury caused by some extrinsic agency--this is change induced by "external" means, as a linguist might say. (I owe the analogy to Dr. Neil Jacobs.)

I should have thought it self-evident that the *real* history of a language, what Weinreich in the above quotation called the "integral history of a language," is precisely the mix and muddle between internal and external causation. Internal causation: sound change, analogy, simplification, reduction of markedness (i.e. the intrinsic complexity of sound), tendencies towards symmetry, even what Postal (1968:283) described as "the general tendency of human cultural products to undergo 'nonfunctional' stylistic change." (Why do dress lengths rise and fall? Well, whatever the reasons, they aren't "functional.") External causation: language contact and dialect mixture--one language rubbing off on another one.

My own view is exactly that of the distinguished Romance scholar Yakov Malkiel: "While it is well known that students of explicative historical linguistics [by which Malkiel means, basically, the modern 'structural' approach to the study of language change] tend to lean either in the direction of 'substratum theories' (external influence) or in the direction of 'structural modifications' (internal influence), little attention has been paid to the wisdom of positing, under certain conditions, the agency of complex, as against simple, causation, which might bridge the resultant gap" (Malkiel 1968:27). Malkiel's point is, as it were, that while every linguist is free to worship in the church of his choice, he has nothing to lose and much to gain from attending worship elsewhere once in a while.

Linguistics, like all social sciences, follows intellectual fashion, though we don't like to talk about it. In the mid-nineteenth century "external" explanations of language change based on race, climate, "national character," and so on were popular. African languages changed, so went the argument, because Africans were incapable of articulating certain sounds; Grimm's Law, the set of sound changes setting off Germanic against Indo-European and arguably the most well-known sound changes of them all, was attributed to shortness of breath attendant upon life in the Alpine regions. The great Jakob Grimm himself thought these sound changes were an expression of the exuberance and impetuosity of the early Germanic tribes. (One somehow cannot quite refrain from adding a *sic!* here.) Such amusing pseudoexplanations now seem quaint and incredible to us; they fell of the weight of their own absurdity into disrepute (in other words, they violate the cultural and linguistic relativism on which modern linguistics is based), and the Neogrammarians--late nineteenth century precursors of structuralism--with their desiccated and unforgiving "give-me-the-facts-nothing-but-the-facts" positivistic attitudes gave the pendulum a vigorous shove back toward internal explanations. The strength of this swing left external explanations with a residual taint of the nineteenth century *Volksgeist* that they still labor, with indifferent success, to escape.

Yiddish historical linguistics has ridden out the same intellectual pendulum swings. The origins of respectable Yiddish linguistic scholarship reside intellectually in the late nineteenth century (Saincan, Landau, Gerzon, Sapir), so that it is no wonder that, initially, internal explanations were favored. But, as Wexler (1981:103) correctly points out: "In most cases, a model of monogenesis [essentially 'internal' explanations of development, in my sense] that derives a Jewish language from a single non-Jewish cognate source is blatantly inadequate." And, further: "Since the late 1920's, a few scholars such as M[ax] Weinreich and S[olomon] Birnbaum, reacting against the constraining model of monogenesis, have urged that the distinctness of Jewish languages be sought in creative fusion of heterogeneous linguistic sources at the very earliest stages of these languages" (p. 104). "Fusion" characterizes languages such as Yiddish in which one can conveniently speak of a distinct, identifiable "German component," a "Hebrew--Aramaic component," and so on. The notion, which has enjoyed a certain vogue in recent years, is intrinsically "external."

And I agree with Wexler up to a point, just as I agreed with Uriel Weinreich up to a point. My disagreement with both has to do with methodological sequencing, with priority and efficiency. The correct methodology, as I see it, is to press internal explanations to the limit--to the limit of course, not past it. When

we can go further--meaning when the theory fails, *that* is when we start looking around for external explanations, i.e. for a language-contact explanation. This is generally accepted linguistic methodology; it is correct linguistic methodology; it is the reason nobody believes any more that sound changes get started because people have trouble breathing in high altitudes.

So let's take another look at the four riddles. The synchronic (i.e. contemporary) match between Yiddish and its linguistic neighbors in Eastern Europe is very poor; this Weinreich makes completely clear. Nota bene the qualifier *synchronic*. Weinreich, confronting the puzzle, seeks a way out via diachrony (i.e. history). He points out correctly that a routine confrontation of the synchronic structure of one language with the synchronic structure of another language sharing the same territory is an engaging but ultimately frivolous kind of game. It doesn't prove anything. Just because the Yiddish of today has one thing and the Polish of today something else means nothing. It could well have been the case that Polish centuries ago had the feature in question and that Yiddish adopted it then and never let it go, while Polish in the meantime has gotten rid of it. In other words--this is Weinreich's solution to the four riddles--synchrony is not enough; we have to relate the structural developments in Yiddish dialects to the history of Jewish settlement and the nature of language contacts in Central and Eastern Europe from centuries ago.

So far so good, no disagreement here. But it works even better, in my opinion, to explore internal explanations to the limit and exhaust their appropriateness before turning toward Jewish-Gentile linguistic contacts to see what they may help explain. *That* is the right way to get to the integral history of the language.

I have dealt with two of the four riddles along these Malkielian lines in two articles: "The History of Final Devoicing in Yiddish" in *Field of Yiddish IV* (1980), and "Two of Weinreich's Four Riddles Revisited" in a paper I gave in December, 1986 in Oxford (King MS). In regard to word-final voicing my view (King 1980) is that it arose as a consequence of apocope (loss of final unstressed *-e*) and opacity (resultant ambiguity of the devoicing rule) in Yiddish even before the Jews had left Germany. Word-final voicing remained in all varieties of Eastern Yiddish except in Poland, where word-final devoicing was reintroduced under the influence of Polish (which itself innovated devoicing at the time) in the fifteenth century. The explanation is partly internal (one notices how loaded it is with narrowly linguistic concepts such as apocope and opacity) and partly external (Polish influence in the fifteenth century--influence that gains in credibility because of attested close social and economic contacts *at that time* between Jews and Gentiles in Poland up and down the social scale). Neither a pristine internal explanation nor an exclusively external meets the complexity of the case.

Likewise, my explanation (King MS) of the loss of vowel length in Northeastern and Southeastern Yiddish (and not Central Yiddish) is internal in part, external in part. I propose that loss of vowel length and the rise of word-final voicing are causally related, and that lengthening in open syllables and the replacement of long vowels by diphthongs play a role in the loss of vowel length by eroding the functional importance and independent status of vowel length. These are all internal factors. They are not enough. Consequently I found myself required to relate the loss of vowel length to differential contact situations involving Polish, colonial German, Lithuanian, and Belorussian. Again, internal *and* external causalities must be invoked.

In regard to the other two riddles, loss of /h/ and *sabesdiker losn*, I think Weinreich's explanations, which are external all the way, are correct. I see no satisfactory internal explanations.

The point of my paper is to argue for common sense (and ecumenism) in approaching the history of the Yiddish language, and in approaching historical change in general. Internal explanations are good; internal explanations past reason are bad. Fusion--the absorption into a language of complete chunks of another language, as Hebrew has been absorbed into Yiddish--is good as part of an explanation for historical linguistic change; fusion alone as an explanation is bad; fusion as a substitute for thought is bad. External explanations are good; external explanations before exhausting the possibility of internal ones are bad. Too much monogenesis--the attribution of the development of a language to a single source and causality--is "blatantly inadequate" (Wexler's phrase); so is too little. Linguistic extremism, no matter what it is in defense of, is rarely a virtue, almost always a vice. Eschew reductionism.

I once wrote a book about historical linguistics (*Historical Linguistics and Generative Grammar*) that had a lot of things to say about internal explanations and very little to say about external explanations of change. I have often been taken to task by reviewers for tunnel vision. The book was one-sided, impatient with opposing views, no doubt about it--it was after all written late in the 1960's. But I don't feel ashamed of my ministrations; the purpose of the book after all was to explore what generative grammar tells us about historical change. And generative grammar cannot in principle tell us *anything* about externally induced change; *no* theory of synchronic grammar can in principle tell us anything about external change.

Historical linguistics is more than the replacement of one synchronic grammar by another. It is borrowing *and* apocope, substratum *and* opacity. It is internal *and* external causation, and it is in the misty regions between the two extremes that the *real* history of a language, what Uriel Weinreich called the "integral history of a language," is to be sought and found. Let us, with Yakov Malkiel, pay attention to the wisdom of positing complex as opposed to simple causation. That way surely is the right direction for the historical linguist to be heading, that surely gets us closer to a *real* understanding of why languages change. This heterodoxy, this generous openness to the richness of the mix among causalities of change, was part and parcel of Uriel Weinreich's approach to historical linguistic change; it will, I venture to predict, endure as his most lasting contribution to linguistic theory.

REFERENCES

- ¹ King, Robert D. 1980. "The History of Final Devoicing in Yiddish." *The Field of Yiddish* 4.371, ed. by Marvin I. Herzog, Barbara Kirshenblatt-Gimblett, Dan Miron, and Ruth Wisse. Philadelphia: Institute for the Study of Human Issues.
- ² King, Robert D. MS. "Two of Weinreich's Four Riddles Revisited." Second Annual Oxford Winter Symposium ("Dialects of the Yiddish Language"), Oxford Centre for Hebrew Studies, 14-16 December, 1986.
- ³ Malkiel, Yakov. 1968. "The Inflectional Paradigm as an Occasional Determinant of Sound Change." *Directions for Historical Linguistics*, ed. by Winfred P. Lehmann and Yakov Malkiel, 21-65. Austin, Texas and London: The University of Texas Press.
- ⁴ Postal, Paul M. 1968. *Aspects of Phonological Theory*. New York: Harper & Row.
- ⁵ Weinreich, Uriel. 1963. "Four Riddles in Bilingual Dialectology." *American Contributions to the Fifth International Congress of Linguists*, 335-359. The Hague: Mouton.
- ⁶ Weinreich, Uriel, William Labov, and Marvin I. Herzog. 1968. "Empirical Foundations for a Theory of Language Change." *Directions for Historical Linguistics*, ed. by Winfred P. Lehmann and Yakov Malkiel, 95-195. Austin, Texas and London: The University of Texas Press.
- ⁷ Wexler, Paul. 1981. "Jewish Interlinguistics." *Language* 57.99-149.