

Review: *Zellig Harris: From American linguistics to socialist Zionism*, by Robert F. Barsky<sup>1,2</sup>

Biography is a perilous craft. If one is intimate with the subject person, the milieu, the issues, one almost ineluctably is partisan; if not, one necessarily is to that degree an ignorant outsider. Barsky, very much the outsider, grapples audaciously with the perils of ignorance. His declared method is to let quoted materials speak for themselves. “Given that he worked and circulated in quite different realms, sometimes simultaneously,” he says, “there are competing interpretations of his actions and his legacy; but rather than trying to resolve these differences, I will present them in as much detail as possible, and allow the dialectical and dialogical processes to work their own magic” (xi). And again (84) “No single narrative could adequately account for his ideas, his approach, his world; and so I have worked with the assumption that the only way to adequately render a life in words is with a plethora of voices speaking from his past, in harmony and contradiction”. Perhaps it is as well that he forbears (mostly) from overinterpretation. This entrusts a great deal of the work of integration to the reader. Are we up to it? And is he in fact as neutral as he claims?

The book has three parts: Part I, The man; Part II, The Language Work; Part III, The Politics. These are at best areas of wished-for emphasis rather than strict categories, however, for the material, or at least B’s assemblage of it, is too unruly, so that all three themes crop up in each section. Given the author’s previous two essays at biography, it can hardly surprise us that a twelve-page appendix is devoted to an interview with Noam Chomsky.

We have known from obituary notices that Zellig Harris was born in Ukraine October 23, 1909 and that when he was four years old his family fled violent antisemitism and emigrated to Philadelphia. We learn now that he was naturalized an American citizen in 1921 at the age of 12 and that the year after that, in 1922, the Bar Mitzvah boy was sent to live in Palestine. We are not told whether his siblings were also sent to Palestine. His daughter Tami Harris tells us (73):

He studied at the *herzeliya* gymnasium and worked to support himself. [He probably] lived in a youth hostel with some twenty kids in a room. At the age of seventeen, his aunt and uncle moved to Tel Aviv, and he finally had a home and the means to study more and work less. His uncle, Yehuda Kaufman Ibn-Schmuel, was a well-known writer and philosopher. His cousin, who was eight years old at the time, was Bruria Kaufman, who later became his wife....

This experience might have engendered a kind of Weltschmerz. Once, Tami, in a touching moment, confided to him her feeling that

we all have a void inside us, even people who have had a relatively good childhood. We try in different ways to fill it up—through work, sex, hobbies, family, drugs—but we are doomed to fail. Our only chance is to realize this, and accept the inevitable pain. He looked at me with tears in his eyes and said, “Those are the most intelligent words I have ever heard.” (75)

But this is not to say that he was a gloomy or somber person, he “had a ready laugh and could hardly ever be said to be ‘in the dumps’” (35). Jerry Cantor, a fellow kibbutz member, says (211) H’s “very gentle and helping personality was always much appreciated” and “his modesty was legendary.” Harold Orlans[ky], who knew him in educational work with Avukah, the student Zionist organization, said he was “entirely friendly and kind ... didn’t put on airs, he didn’t have a sense of superiority, he was even tempered, even under provocation, and he was, in all things of interest to him, very informative.”

But B also has found testimony that H was cold, distant (34), and dogmatic (317). How do we reconcile this disparity? B, as noted, leaves such questions for us to answer. Let us see what we can find.

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<sup>2</sup> A shortened version was published as “Zellig Harris: science, language, and radical transformation of society”. *Historiographia Linguistica* 38 (3): 355–366. (doi:10.1075/hl.38.3.05nev). A much shorter review was published in 2013 in the *Journal of Modern Jewish Studies* 12.3:591–593 (DOI:10.1080/14725886.2013.864533)

An obvious key ingredient is a critical stance toward the enviroing culture, and a skepticism about the efficacy and fitness of extant social arrangements—a stance which H attributed to Sapir in his monumental 1951 review of the *Selected Writings*.<sup>3</sup> This attitude of reasoned detachment is perfectly understandable in intellectual families who had immigrated to the United States from disastrous circumstances in Ukraine (6–16), who had a lively awareness of the antisemitism and the potential for fascism in their adopted country, and who experienced and saw around them the fragility of conventional social and economic reliances even before the Great Depression. This orientation cannot but have suffused and informed the lively salon-like conversations for which the Harris household was famous (17–18).

Underlying this is a deeply felt commitment to contribute to the common stock of human knowledge and understanding. H had intellectual abilities that were evident to all, and surely also to himself. But rather than using his capacities for personal advantage, he clearly felt an obligation to be useful to his fellow creatures. Jerry Cantor once sent me scanned images of some analytical work, an extension of discourse analysis, that H had done for him as a friend and neighbor in the kibbutz. Cantor was responsible for technical documentation of some complex systems. There was a great deal of redundancy, i.e. repetition across documents, and a great deal of technical detail. He saw that if he could somehow factor these documents into reusable chunks, and if those chunks could be managed and reassembled in various combinations, he could achieve much better results with huge increases of efficiency and reduction of cost. H voluntarily undertook that factorization for him, the kind of thing that is now done with XML, but before even the development of SGML. And in all that he did H was a very hard worker. His daughter, Tami, tells us (74) “He would write until drifting off to sleep, and the line slanted down. He startled and kept on writing until it happened again. On the third time he would stop and go to sleep, not before.”

As Tami Harris observed (72), “people who starve seldom have the luxury of coming up with revolutionary ideas.” Few individuals are able to make their best contributions in a society that has great disparity of wealth and privilege, for the wealthy few are occupied with their entertainments and mutual status relations, and the majority are economically bound to labor in support of commercial purposes whose only relation to such contributions is to seek ways to gain from them once they are proven. When H was just 20 years old, he wrote (193) that “no individual can be as rich, as many sided as an organization of such” and “a nation is “a harmonious coordinating of peculiar abilities and tendencies, of reactions and orientation of life, of an attitude to the past and the future, of all the mores and morals of a group ... the bearer of its culture, bounded by it, and like it, to be taken in reference to the whole world.” But to the extent that the unique capacities of individuals to contribute in their society are thwarted, that society is diminished no less than are those individuals.

Within this generic incentive to work for more humane and equable social arrangements there is specific concern for the future of Jews. Hence, Zionism: a concerted effort to establish a center where Jewish culture could develop and make its unique contribution among the cultures of the world without the perpetual corrosive influence of struggle as a minority. With the rise of Nazism and fascism the need to provide a home for those fleeing persecution became even more acute, fueled by the painful recollection of pogroms only a generation away in family memory. And yet many young people were strangely indifferent to such issues.

On this basis, H worked to understand the dynamics of social institutions, how they change, and how individuals by informing and educating others can foster and influence such change. From this work; from the necessity of pursuing these questions in time taken from his academic career, first as a Semitist and then in the new field of linguistics; and from certain incentives to keeping a low public profile—Hoover’s FBI was undoubtedly watchful (though they disclosed nothing to B’s FOIA query), and H had

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<sup>3</sup> An essential entrée to understanding Sapir, and for understanding H as well. There is no evidence that B has read it.

reason to write (107) of “the brutality of American universities, and the indifference of most of them to scientific work”<sup>4</sup>—from all this it followed necessarily that the political work was carried out quietly, within trusted circles, and that H focused his attention on what Glazer (and B after him) thought of as a ‘vanguard’ of those who were similarly motivated and who were able to grasp the issues and contribute to the work. Those who were not could not be given the attention of which they doubtless considered themselves worthy. We should emphasize that the inner circle of Avukah leadership, with its more radical politics and its penchant for secrecy in the face of red-baiting and government investigations, was in New York, centered around Melman, not in H’s Philadelphia. We will look at this in more detail later.

No such dynamic existed in linguistics. The attentive reader may note that the negative assessments—e.g. that H was cold, dogmatic, and intolerant—are quoted from those who knew H only through Avukah, and significantly from those in Avukah who rejected the intensive educational work of H’s so-called ‘vanguard’, or who did not enter into it at all.

H’s sister-in-law, Susannah Harris, tells us (67):

Zellig’s mind went in many directions, he could do anything. But as time went by, people felt detached from what Zellig believed, and from the way he looked at things. He was a very different individual, very, very different. And it was very hard to match up to him, in any way. Eventually, people distanced themselves from him, they had to; they went on with their ordinary lives, and not this highfalutin existence.

From this statement that others drew away into their mundane concerns, B concludes, inexplicably, that H “became increasingly aloof”. Again, B says (35) “In some ways, E.F.Konrad Koerner’s biography ... notes the unanimity of this view” that H was cold, dogmatic, pedantic. In the cited location (Koerner 1993) we find a two-paragraph introduction to his comprehensive Harris bibliography. It is difficult to see this as a ‘biography’, and even more difficult to find any such corroboration in Koerner’s statement that “all seem to agree that he was a very private man who would have thought any tribute to him inappropriate”, the only statement there that says anything about his character.

This preference for a negative interpretation is pervasive. B says (xli) “In order to validate some of the statements recorded here, I have asked the same questions of different interviewees, and I have tried to confirm statements with different people ....” Ted Live, H’s nephew, says (18) H was “an unassuming, friendly relative ... helpful, he certainly didn’t strike me as being a particularly difficult person”—that last clause apparently in response to a leading question. B quotes Senta Plötz (155) writing in 1972 that “Harris did not create a school or admit disciples; instead he was tolerant toward work done outside his framework [...]”. In an endnote on this paragraph, B says “Not everyone I have interviewed agrees with this perspective, and even from this chapter it is clear that Harris had some disciples who would receive considerable attention for a period of time, and would then be dropped...he did not readily bear dissent in situations such as Avukah or with close coworkers.” But nowhere in that chapter or elsewhere in the book are any ‘dropped disciples’ in linguistics named. I know of none,<sup>5</sup> and with Chomsky it was the other way around. This quite apart from the fact that H did not cultivate ‘disciples’.<sup>6</sup>

A consideration of H’s principled view of science opens the question to what extent science is a matter of reporting replicable findings, and to what extent it is a matter of agreements within a community of scientists. These are obviously inextricably related, the question is a matter of their sequence and relative weight.<sup>7</sup> In a political controversy with a right-leaning student Zionist organization, H said (229):

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<sup>4</sup> This was during his efforts to find an academic home for Roman Jakobsen.

<sup>5</sup> After this review was published, Lila Gleitman told me this was her experience. The occasion seems to have been her proposed dissertation presenting joint work as her own, at a time when the consequences of Chomsky’s having done the same were difficult to ignore. This was published two years later in 1965: Coordinating Conjunctions in English, *Language* 41.2:260-293. She did get a Linguistics PhD from Penn in 1967, under Henry Hiz.

<sup>6</sup> This is more characteristic of the branchings and altercations of the ‘Generative Enterprise’.

What [they] fail to understand, and what they characterize as “a confused attempt to do away with positive factors within Jewish life,” is that this is a simple presentation of scientific method, which abstractly would read (1) begin with no preconception as to the character of the material to be studied; (2) deal only with observable data; (3) draw conclusions consistent with observed data.

Political advocacy, to the extent that it flouts (1), as it routinely does, is at best tendentiously selective of (2) and thus incapable of following through with (3). B is puzzled why the decades of work on the “Frame of Reference” project led to no publication other than H’s posthumous *The transformation of capitalist society* (Harris 1997). He does not connect this question with his own rather astute answer (81): “he was in search of verifiable results, and perhaps he never satisfied that requirement when it came to work on social issues.” He follows this immediately with the tendentious non-sequitur “This scientism lends an air of seriousness to everything that Harris published, such that it is difficult to imagine him in social interactions.” But H’s reticence and slowness to publish<sup>8</sup> are patently opposite to scientism, which is typified by a quick display of spurious certainties.

Chomsky’s conviction that an a priori theory of language is a prerequisite to doing linguistics is in direct contradiction to H’s commitment to (1), and indeed did lend support to the vicious academic politics of Generativist linguistics the 1960s. B makes much of the ‘insularity’ and ‘increased isolation’ of H’s work beginning in that period. He quotes Matthews’ obituary (162):

The paper in 1965 was also, sadly, his last contribution to *Language*. I have not thought it my business to inquire into the circumstances; but from then on, for whatever reason, a journal for which he had written so much for a quarter of a century, and so much of such influence and importance, published him no more.

B continues, “This is a point of interest upon which a number of informants speculated in discussions with me”, but he quotes no such speculation, giving as an example only Kittredge saying, in essence, that he knew nothing. We must ask the reason for H’s “lack of engagement in theoretical debates” (Kittredge’s phrase) beginning during the 1960s; let us now do so, as B does not.

Harris’s daughter Tami says (72) that her parents, Zellig and Bruria, were “inspiring in their humility”, and again (77):

He, as a person, or an ego (spiritually speaking, not psychologically) was not there. He was like a hose. You turn the tap on, and water comes out the other end. The hose has no ego, the hose needs no recognition, the hose needs nothing, it’s just a hose. The hose is a conduit through which knowledge, wisdom, talents, or human values can come to life. “Shoot the messenger,” they say. My father was the messenger, totally indifferent to being shot. Actually, these metaphors are not precise either. A hose or a messenger would not avoid recognition or acknowledgement the way my father did.

Whatever there may have been of native temperament in this modesty or humility,<sup>9</sup> it appears as well that it was a matter of principle. Naomi Sager writes, in her contribution to vol. 2 of *The Legacy of Zellig Harris* (Nevin & Johnson 2002):

His theoretical results were the product of prodigious amounts of work on the data of language, in which the economy of description was a major criterion. He kept the introduction of constructs to the minimum necessary to bring together the elements of description into a system. His own role,

<sup>7</sup> For discussion, see Nevin, Bruce (2010), “Noam and Zellig”, in D. A. Kibbee (ed.) *Chomskyan (R)evolutions*, Amsterdam: John Benjamins.

<sup>8</sup> As B remarks (124), “To read Harris’s descriptions of his own work is to have the sense that he would undertake analyses that in some cases lasted many years, and would publish them only when he felt they were ready”. There is of course abundant independent evidence of this in the primary sources to which B seldom refers.

<sup>9</sup> Given this prominent trait, it is striking how often B uses the incongruous phrase “Harris’s ambitions”.

he said, was simply to be the agent in bringing data in relation to data. [...] It was not false modesty that made Harris downplay his particular role in bringing about results, so much as a fundamental belief in the objectivity of the methods employed. Language could only be described in terms of the placings of words next to words. There was nothing else, no external metalanguage. The question was how these placings worked themselves into a vehicle for carrying the ‘semantic burden’ of language.

As a result, he knew that the findings that he reported could stand on their own merit as a lasting contribution to science, just as surely as can a report on the chemical properties of a new compound, or the identification of a new species. Polemical argumentation, what might be called a gladiatorial conception of science,<sup>10</sup> was beside the point.

In this, undue attention to the *person* of the scientific worker was misplaced. In 1969, Harris firmly put a stop to a Festschrift that was being planned for him.<sup>11</sup> In a letter to the organizer, former student W. C. Watt (b. 1932), he wrote about this as a

matter in which I have human rights. ... Such a publication would be a deep personal affront to me and to my sense of values. I have managed to live this long with the principle that scientists can be people who do the best work they can for the sake of knowledge and of its human value. Any special—and unavoidably invidious—recognition of their work, such as honors, prizes, and Festschriften, is abhorrent to me, and would violate what I feel is a human right and dignity.... Many years ago, during Bloomfield’s lifetime, I had to get a similar project stopped for Bloomfield’s sake, and I am sorry now I have to do it for myself. I am sure, however, that you will understand me, and will respect my principles even if they may seem excessive.

In a following letter, he wrote (76) that “it is good sometimes to air one’s feelings about the culture we live in (I don’t mean only ours, or only now—the others are even worse).” This is the only occasion I have ever heard of him taking umbrage<sup>12</sup> in a personal matter, and though I was there at the time and working fairly closely with him, I heard nothing of it. He had an aversion to the guru relationship that some young Avukah followers accorded him (27), with its inherent cult of personality. This may account for there being only a very few photographs of him, all candid snapshots, no portraits or posed pictures.

This principle, and the subordinating of self to a larger purpose, links the politics and the linguistics. In 1936, the year he obtained his Ph.D. at Penn with a comprehensive grammar of Phoenician that is still useful today, an article in the *Avukah Bulletin* about H’s retirement from the Presidency of that Zionist student organization quotes him saying “this is a movement, and individuals don’t count.”

So, again, what happened in the 1960s? H’s daughter, Tami Harris, says (79):

At some point ... Chomsky turned against him ... and was publishing their joint research, without even mentioning my father’s contribution. For around twenty years my father’s career was seriously restricted by Chomsky, mostly in the U.S. ... I heard the whole story when I was fifteen years old. I asked my father why he didn’t somehow correct it. He looked at me as though I were asking the dumbest question imaginable, and, very typical of his thinking, he replied, “naah, I have work to do”.<sup>13</sup>

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<sup>10</sup>Michael Gottfried’s phrase.

<sup>11</sup>We are told that “many of those who contributed to Nevin and Johnson’s *The Legacy of Zellig Harris* also wrote for [the] Festschrift” proposed by Watt. An inference that their intended contributions for the 1969 project found their way into the 2002 collection would be wrong. Aside from that, of the 31 who Watt lists, only five contributed to *Legacy* (Lila Gleitman, Aravind K. Joshi, Leigh Lisker, Naomi Sager, and Carlota S. Smith). Perhaps that is “many.”

<sup>12</sup>Watt takes exception to this word (p.c.): “I read his letter as firm, perhaps slightly annoyed.”

<sup>13</sup>Similarly, Henry Hiz recalled in his obituary, “Once, it was said in his presence that a linguist had written something that he would not like. He cut it short: ‘Let us do our work.’”

B calls this a “line of thinking among family members,” apparently as grounds for not taking it very seriously. One would think that these notably intelligent and perceptive people, who knew both men well, would have some insight even in matters about which H himself was reticent. A natural inquiry following up this strong assertion by H’s daughter might have been, is this plausible? How could Chomsky have restricted H’s career? By what means?<sup>14</sup> For reasons that we can only guess, B does not pursue such questions.

Here, B’s ignorance of linguistics becomes crucial. As has been well documented by Hymes & Fought, Murray, McCawley, Koerner, and others, the influx of Federal dollars during the Vietnam War coincided with the aggressive marketing of Generative Grammar as a package—to buy any of it, you had to buy all of it, in a kind of conversion experience. With that money, new departments were created and existing ones expanded, and administrators hired faculty trained in the sexy new approach with the grand claims and the flashy publicity because (as McCawley wrote in 1976)

it was to their advantage to speculate their newfound monetary capital on it, since if the new theory was going to become influential, a department would have to offer instruction in it if the department was to attract students in numbers that were in keeping with its newfound riches. (152–153)

Linguists like Bernard Bloch assumed (151–152) that Chomsky was elaborating on H’s ongoing work, but in fact he had replaced H’s transformations, a set-theoretic and algebraic property of language, with logical operations on abstract base structures—Carnap’s ‘rules of formation’ and ‘rules of transformation’.<sup>15</sup> Those who had bought the Generative Grammar package could see H’s transformations only in its terms, and H’s subsequent developments beyond transformations they could not rightly apprehend at all, and still cannot. The distinction between the property of language and the logical metalanguage that sort of represents it (while imposing its own characteristics) is still rarely recognized. For example, also in 1976, Katz, Langendoen, and Bever wrote (131) that

contrary to popular belief, transformations come into modern linguistics, not with Chomsky, but with Zellig Harris’s rules relating sentence forms. These are genuine transformations, since they are structure-dependent mappings of phrase markers onto phrase markers.

A Generativist will not see anything wrong with that characterization. (The allusion to “popular belief” reflects how marketing was taken as history.) B quotes reviewers who are plainly unable to see relevance or value in H’s publications and grant proposals, understanding his work to be no more than ‘taxonomic linguistics’ or ‘early transformational grammar’, or some other outdated model that had been labeled obsolete in the polemics endemic to the Generative enterprise.<sup>16</sup> That these straw men have nothing to do with H has been amply documented in works from which B actually quotes (Hymes & Fought, Murray,

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<sup>14</sup>Relevant here is the story that an essay by H on the form of information in science was rejected by the editors of *Science* because he did not cite Chomsky.

<sup>15</sup>In the 1975 Introduction to *LSLT* (p. 43), Chomsky said “In *LSLT*, transformations are understood in a very different sense; it probably would have been preferable to select a different terminology instead of adapting Harris’s in this rather different context.”

<sup>16</sup>B does not mention Paul Postal’s influential 1964 *Constituent structure: A study of contemporary models of syntactic description* (*IJAL* 30.3), which finds every extant proposal for syntactic description, including H’s 1946 class-expansion analysis and his 1963 string analysis, to be just another instance of Chomsky’s Phrase Structure Grammar. Postal sets aside H’s semantic methodology (the substitution procedures). He acknowledges that these approaches both avoid an inflation of what Joshi calls a pseudo-hierarchy (but which he does not see as problematic) and that they capture the head-of relation in endocentric constructions (which PSG could not until X-bar notation was introduced some years later, a remediation that controverts his case). He ignores that H himself agreed with his major thesis, saying that these forms of analysis were incomplete, and that transformations were necessary to advance from a grammar of word-class combinations toward a grammar of word combinations, which was reached with operator grammar. There is a large subsequent literature bearing on this, which cannot be discussed here. The present relevance is that the polemical erection of straw men was typical of this period.

Nevin, Matthews), but he simply does not understand the issues. Nor does he try. “However insular his work was in an American context,” he says (162), “it is remarkable how many of Harris’s later writings were to appear in Europe ... and not, as hitherto almost exclusively, in America.” What is remarkable, instead, is the studied ignorance of his work in America—and how incurious B is about that.

After mention of some of the early work at Penn with machine parsing of language, and (irrelevantly) the first sound spectrograph in a linguistics department (due to Leigh Lisker’s work at Haskins), B quotes Chomsky’s *Language and Mind* at length (157–158) as a “broad-ranging assessment”:

In the United States at least, there is little trace today of the illusions of the early postwar years. If we consider the current status of structural linguistic methodology, stimulus-response psycholinguistics ..., or probabilistic or automata-theoretic models for language use, we find that in each case ... a careful analysis has shown that insofar as the system of concepts and principles that was advanced can be made precise, it can be demonstrated to be inadequate in a fundamental way. The kinds of structures that are realizable in terms of these theories are simply not those that must be postulated to underlie the use of language, if empirical conditions of adequacy are to be satisfied.

And so forth. The quotation continues for another half page, after which B says “The distinctions between Harris’s ambitions and Chomsky’s approach are herein made explicit”.

Yet in his defense B is not alone in assuming that H’s linguistic work at the time of this publication (1968) could be reduced to “structural linguistic methodology” or had anything whatsoever to do with “stimulus-response psycholinguistics or probabilistic or automata-theoretic models for language use”. The publisher’s reviewers of B’s ms., whoever they may have been, also would naturally have accepted this, because in 1968 and still today “everyone knows” that in utterances like this (and there are many), Chomsky was referring to and differentiating himself from Harris.<sup>17</sup>

“[Chomsky’s] work on the system of knowledge and belief, referred to variously as a mentalist approach,” continues B, “is in sharp contrast to the study of output, upon which in Chomsky’s view so much of Harris’s work focuses.” An endnote here elaborates

Goldsmith suggested in personal correspondence (8/07/09) that what is being studied is not the “output,” but rather the “less rich thing that it has excreted.”

Now, this quotation puzzled me mightily, particularly its use of the word “output” so I asked John Goldsmith to clarify what he meant; he was equally puzzled, and passed the question on to B. It turns out, this was a marginal comment on a copy of the ms. that B had asked him to review. Here is what Goldsmith wrote, actually recommending against using that word:

“Output” is the wrong metaphor in action. It suggests that there is a big thing there which deserves to be studied, but [that] what is being studied is instead the less rich thing that it has excreted. That’s not Harris’s view (and many linguists agree with him); it is Chomsky’s.

So even taking this book as a useful montage of quotations, *caveat lector.*, this garbling of quotations may not be unique, and despite that B says (172) “it is not my objective to adjudicate in these technical matters” of linguistic theory, there does seem to be some unheralded side-taking.

Had H distracted himself from his work to respond to this stuff, he might have said “Noam’s not talking about anything that I’m doing. and I’m not yet ready to say anything about a general theory of language, how it came to be, how it is learned and used;<sup>18</sup> but he is welcome to try.” As Maurice Gross put it (163),

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<sup>17</sup> Chomsky has told me that even his critique (with Halle) of ‘taxonomic phonemics’ did not refer to H’s *Methods*, and that he is unfamiliar with H’s later work and has never understood it. He has also told me that he has no responsibility for inferences his students and followers may have made and then transmitted unimpaired to their students.

The theoretical views that Harris develops in [*Mathematical Structures of Language*, 1968], although based on the same empirical material [as Chomsky's work], are completely new, and the relationship with what is called mathematical structure in Chomsky's framework is quite indirect. In fact, Harris's approach has many facets. While Chomsky made one main hypothesis about language structure, namely, that it is describable by means of certain types of formal systems (in the sense of mathematical logic), Harris looks at various phenomena as being of distinct natures; as such, they require distinct treatments. Thus, for Harris, linguistic theory does not have to be unified for the time being.

Whence also H's well-known comment in (1965) about the folly of pitting one "tool of linguistics" against another. H was on to something, an important contribution of lasting value. He had no interest in fame or a personal following—or rather, as his daughter says, he had an aversion to such distractions. For those who want these things, very public disputation serves a purpose. Why should he bother himself arguing with them?

Context is missing in a number of places. For example, Orleans is quoted (82) saying "Sapir was considered far more brilliant among many." More brilliant than *hoi polloi* of the field? Considered by many to be more brilliant than Harris? H congratulates Goetze "on making the point about "German nominations" for "honorary memberships in the Oriental Society" (94). One wonders just what point was made. Opposition to fascism? Welcoming of German Jewish scholars? We want B to do this bit of research and tell us, but despite patent relevance to issues brought to prominence in this book, he drops it on the floor.

B seems not to be aware of issues that have been raised in the historiography of linguistics. For example, in several publications E.F.K. Koerner has wondered whether H's references to Bloomfield's *Menomini morphophonemics* were in the ms. of *Methods* in 1946, or if they were added after Chomsky had written his M.A. thesis on Hebrew morphonemics. In a letter to Bloch in 1949, acknowledging the suggestion of a publisher associated with the LSA (Ronald Press), H writes (127):

I simply lost interest in my *Methods* ms after writing it. There are other things bothering my little head, and I practically never remember the existence of the ms. Of course, such books are items of status; but I have such a deep disrespect for the status structure of our society (or of any other) that I just don't react. I'd much rather live and find things out. So the delay in the appearance of the book has no personal meaning for me.

In an undated letter (probably written just after the above, since in it H is floating the idea of approaching commercial publishers as against an "informal understanding" to publish under the auspices of Bloch as editor of *Language*), he says the ms. was "revised by me for the (n+1)th time last spring." In March 1950, he asks for advice about commercial publishers, then "Incidentally, although I couldn't go back and review the book, there are some improvements I want to put in as a part of my work during the last few years." A letter of December that year notes a commitment from the University of Chicago Press. These letters are equivocal on the matter, to be sure, but strongly suggestive that the revisions of which H wrote in March were minor tinkering. Certainly, H's term "descriptive order" is straight from Bloomfield (canonically, in his book *Language*, p. 213), and his treatment of morphophonemes (pp. 237–242), like Bloomfield's, depends upon the innovation of ordered rules. We do not know what other indications there may be in correspondence that B has examined without awareness of this question. And of course this is just one of myriad historiographic questions upon which an informed researcher might find light shed in the archives that B has examined. May such researchers be encouraged to look!

We have noted B's strategy of juxtaposing sometimes contradictory testimony in a montage. Whee he actually does make some attempts at integration in transitional passages, the results are mixed. On page 60, a paragraph beginning with a topic of "new communication systems" includes a lengthy quotation

<sup>18</sup>All of which he undertook in his last publications, beginning with the Bampton Lectures at Columbia in 1986 and culminating in *A Theory of Language and Information* (1991).

from IBM about the need to produce more efficient electronic parts (“rectifiers ... switching devices”), the relevance of which seems to be that there was impressive but incomprehensible (to B) technical stuff going on, then

Zellig Harris’s scientific linguistics provided the tools that would be required for the purposes of machine translation, decoding, content analysis, and so forth; and his political work, aimed at providing a more adequate description of capitalist society and a science-inspired method for its transformation, was similarly ambitious and all encompassing.

And then suddenly we are talking about H’s teaching style as a “master-apprenticeship relationship that was aimed at fostering a vanguard of like-minded students and colleagues”.<sup>19</sup> Some *segué!*

Here, we must pause to affirm that H did no such thing. If he “fostered a vanguard of like-minded students and colleagues”, where is the resulting ‘Harrisian school of linguistics’? Where are the accoutrements of polemics, popularizations, and PR that such a ‘school’ would produce?

The result of B’s montage strategy is choppy. I have commented on some incongruities and non-sequiturs. Missed opportunities to make connections are more common. For example, quotations on pp. 134-135 (also p. 39) discuss semantics and the use of meaning in H’s analysis of language. This has been elaborated in a number of works that B cites, but does not mention here. He does quote a valuable letter from Bloomfield to Pike (134) refuting the notion that

I, or rather a whole group of language students of which I am one, pay no attention to meaning or neglect it, or even that we undertake to study language without meaning, simply as meaningless sounds.... It is ... something which, if allowed to develop, will injure the progress of our science by setting up a fictitious contrast between students who consider meaning and students who neglect it. The latter class, so far as I know, does not exist.

(This is relevant because of the familiar canard that H was a neo-Bloomfieldian continuing a rejection of meaning and mentalism because of commitments to positivism and behaviorism, etc.) Then at the beginning of the section on “The reception of *Methods*” (139), B makes no reference to that prior discussion when he observes that most reviewers of *Methods* opined that H “eliminates meaning as a criterion of linguistic analysis”. Later in the book, B dwells on his favored interpretation that negative reviews indicate H’s isolation from or even withdrawal from the field beginning in the 1960s, because it suits his narrative arc, but this earlier parallel passes without comment.

Similarly, quoting Seuren’s remark that *Methods* is “among the dullest [books] ever published”, B volunteers the opinion (135) “Especially compared to the wonderful array of recent works that have popularized interest in the relationship between language and the mind ... much of the Harris corpus is dry and painful to peruse”. One might remark (though B does not) that articles in journals and texts of fields such as chemistry, physics, biology, or immunology are almost always quite boring except to those who want to know “what gives” in the subject matter itself. But the point is valid that, as we noted earlier, there is no parallel to the PR activity surrounding the ‘Generative Enterprise’. Later, quoting Hiz (179), we read that H’s 1989 and 1991 books on language and information “are written in a clear, simple manner, almost without formulas”. Each of these characterizations, B could have confirmed for himself. Is stylistic change not worth remarking? What does it signify about H and his relation to his work and to those who he anticipated might benefit from it? The contrast simply stands without comment.

B does work hard to make a connection between H and Einstein. “[D]espite their many differences,” he writes in a typical passage (105), “it is interesting to recall some of the overlapping concerns between Einstein’s and Harris’s work, especially given their connections through Zionism, Avukah, and Bruria

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<sup>19</sup>At this point, endnote 12 directs us to Watt’s obituary, presumably for comment at some undisclosed location in it about H’s teaching style. Confusingly, the same endnote number 12 occurs in the next section on the same page, on a quotation from Konrad Koerner’s 1993 Harris bibliography (the so-called ‘biographby’), in which Koerner refers to “tributes ... on the Linguist electronic bulletin board” as a source of information about H’s teaching style.

Kaufmann”, who was Einstein’s mathematical assistant at Princeton. Certainly the two scientists had Zionism in common. Three pages are devoted to Einstein’s move from Germany to the United states. “I am not at all eager to go to America”, wrote Einstein in 1921, “but am doing it only in the interest of Zionists, who must beg for dollars to build educational institutions in Jerusalem and for whom I act as high priest and decoy.” (197) On his return trip to the U.S. in 1929, he gave a speech that was broadcast from the ship, under the auspices of Avukah. (This speech by Einstein as quoted on p. 199 appears to be a different translation of the speech that is quoted on pp. 197–198 as one of five given between 1921 and 1933 in Berlin. B does not remark the redundancy.)<sup>20</sup> His actual immigration (which B does not note) was in 1933, when Hitler assumed power in Germany.

The attempt to draw an Einstein connection crops up in unlikely places. B introduces a quotation about “the structural method” from H’s 1940 review of Gray’s *Foundations of language* with the informative proposal that “Harris’s method revolves around phonemic analysis” (100), then:

This is where we find the quest to uncover underlying components in human language, an ambition which is in some ways interesting to consider alongside the Einsteinian quest to uncover relations between, for example, subatomic particles. This is not to say that Harris’s linguistics were [sic] inspired by Einstein—it would perhaps be more appropriate to consider constructivism and intuitionism in mathematics and some of the work by Gödel—but Einstein’s ideas were certainly in the air at that time, and there was for many people an allure in the idea of finding the workings of complex phenomena in the actions of unseen structures or particles.

It is doubtful that the reference to “constructivism and intuitionism in mathematics and some of the work by Gödel” reflects B’s mathematical prowess, it is cribbed straight from H’s 1990 survey of his work (published in English in 2002), which is cited later on the same page.<sup>21</sup> Never mind that H did not originate the phonemic principle, and that the analogy to inferred entities in physics applies more aptly to Chomsky’s penchant for abstract constructs.

B says “some further overlaps between Harris’s and Einstein’s approaches are suggested by a talk that Einstein gave on the relationship between words and thoughts” (104), but on reading his quotation from this brief essay the most that can be said is that both talked about language. By some undisclosed logic, Stephen Murray’s questioning of Generativist claims to a Kuhnian revolution (152) “recalls earlier mention of Einstein’s influence upon scientific research”. A letter opposing fascist ‘revisionism’ in America (208) is “a new link between Einstein and linguistics.” Thin tissue for an appeal to glamorous authority which is entirely unneeded.

There are disconcerting temporal discontinuities. We are reading about H and Melman in Avukah as undergraduates (21); suddenly (21–22) Melman has graduated and we are with him and Glazer in 1940; then abruptly (22) we are with H and Melman as undergraduates again. B interrupts an account of Chomsky’s undergraduate and MA work on Hebrew morphophonemics (129) with a tale relating to his PhD thesis, and then resumes with the MA. We read a 2002 quotation (151) of Stephen B. Johnson<sup>22</sup>

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<sup>20</sup> Here, too, we find the first mention of H’s involvement with the League for Jewish-Arab Rapprochement. There is some unremarked confusion as to the name of this organization. On p. 253 we are told that H was associated with the Council for Arab-Jewish Cooperation, which grew out of the left wing of Avukah (which by prior account surely means the so-called ‘vanguard’ in New York), and that it “was similar in its objectives to the League of Arab-Jewish Rapprochement”. A source is quoted (254) calling it the League for Jewish-Arab Rapprochement and Cooperation, and on the same page the Avukah newsletter calls it the League for Arab-Jewish Cooperation.

<sup>21</sup> This essay is correctly sourced in endnote 47 on p. 100, albeit without mention of the French translation by Daladier in the 1990 issue of *Langages* that is cited on p. 165. Farther down on p. 165, further quotations from the English original are sourced instead to a 1992 publication in French by Fuchs and LeGoff, students and colleagues of Maurice Gross.

<sup>22</sup> The co-editor of vol. 2 of *The Legacy of Zellig Harris*. The production editors at John Benjamins pulled from their author database the name Stephen M. Johnson, and it is this name which is mistakenly printed in the book and which lives on in citations

about developing computer formalisms for H's 1982 Operator Grammar, then abruptly and anachronistically "another effort in this direction relates to work on transformations" and we are reading a quote from a letter written by Bernard Bloch in 1959 about Chomsky's transformations.

The discontinuities are sometimes rather lengthy. Beginning a discussion of discourse analysis (154), B refers to H's "pioneering study *Discourse Analysis Reprints*" (a 1963 reprint of three TDAP papers from 1957), then to the rather more 'pioneering' "Discourse analysis" published in the first number of *Language* in 1952 (but omitting "Discourse analysis: A sample text" which appeared in the last number of *Language* in that same year of 1952).<sup>23</sup> Then begins a series of digressions on the Transformations and Discourse Analysis Project (termed "the Transformation and Discourse Analysis Papers series"), covering early computational linguistics, the introduction of the first sound spectrograph in a linguistics department, and jumping thence to the extended quotation from Chomsky's *Language and Mind* as noted earlier, then several Chomskyan negative reviews of string analysis—and finally we return to a discussion of *Discourse Analysis Reprints* six pages later (160).

As B advised us at the outset, he has made little use of primary sources in linguistics, relying instead largely on reviewers, including Chomsky's initial attack-dog Lees (159), Langendoen (131), and various reviewers of *Discourse Analysis Reprints* (160–161). He acknowledges that for them, H's work "recalls, rather than elaborates, upon theories from an earlier era" (170). In light of the practical results in sublanguage parsing by Sager, Hirschman, Johnson, and others, and the spectacular development of discourse analysis in *The form of information in science* (Harris et al. 1989), which B scarcely mentions (evidently he found no reviews of it), the negative comments of anonymous reviewers of grant proposals quoted here (173–179) clearly miss the point. G. Hell concludes that the relations found in discourse analysis are not formal and therefore "are impossible to use in computers." Tae-Yong Pak demands a mechanical discovery procedure, which (*pace* Chomsky) H never claimed to provide.<sup>24</sup> As H pointed out (168), "human judgment is not needed in the individual case" once a sublanguage grammar and lexicon are established. The work of creating the sublanguage grammar and lexicon requires human cognition; computers' use of them does not.

The comments on a 1983-1986 proposal for a project in sublanguage analysis of social science survey instruments, undertaken jointly with Paul Mattick, are somewhat unusual because there were "twenty-nine reviewers, from psychology, sociology, linguistics, economics, mathematics, engineering, life sciences, social sciences, and computer sciences." On the first round, strong support came from those familiar with H's prior work, and objections clearly came from those least familiar with linguistics in general and with the prior work on sublanguage analysis in particular. They allude to the work in a sublanguage of immunology (which was subsequently published in *The form of information in science*), assuming that since "the proposed methodology was perfected" on immunology texts it could not readily apply to other domains. They evidently did not take into account the adaptation to a variety of domains of, for example, Naomi Sager's Linguistic Strings Project at the same Courant Institute for Mathematical

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<sup>23</sup> The analyzed text is a section from a political article in *Commentary*. Michael Gottfried tells me (p.c.) "Zellig once referred to the Commentary gang as a 'crypto-fascist' group trying to turn Jews off from the left." Tom Ryckman was also present when this "popped out seemingly from nowhere, not so much as an expletive but rather like the Latin name of a rare species of butterfly, exact and objective." Compare Melman's view (40) that "Harris invented transformational grammar for essentially political reasons, as a tool to show that the very sentential structure in the writings of Sidney Hook, with whom he disagreed profoundly, would show their falsehood." This reflects their mutual terms of discussion more than it does the range of H's thinking, and is twice synecdochic, of course, taking transformations for discourse analysis, and taking the political application for the entirety that was later expressed as a theory of language and information.

<sup>24</sup> B quotes (130–131) Chomsky's analogy, in *Language and responsibility*, between a mechanical discovery procedure and a putative process for child language acquisition. The quote is from a 19-line paragraph on p. 115 which B does not compare with the 12-line paragraph on pp. 124–125 of the original interview in *Noam Chomsky: Dialogues avec Mitsou Ronat*. The former is of course said to be "based upon" the latter, so I impute no falsification, but Chomsky's inveterate reformulations are always informative.

Sciences where the proposed work was to be done, culminating there in the Medical Language Processor (MLP), a software system which produces a database of structured information formats from the records of physicians, medical technicians, and nurses in hospitals, rife with telegraphic expressions and incomplete sentences that would stump an ordinary parser. These reviewers felt that an analysis of survey questionnaires required a prior semantic representation for responses to surveys, and worried that there was “no way to consider the relationship between questions and answers” or to “distinguish between different ways of saying the same thing”. Of course, any linguist would be confident of question-answer relations and paraphrase, and since a semantic metalanguage for the domain (a sublanguage grammar and lexicon) is a result of the analysis it cannot be demanded as a prerequisite for it. They also objected to the cost, but on the second round, with the proposal narrative more effectively addressing its broad-based audience, the cost was accepted and the proposal was funded. In B’s narrative arc, however, this is further evidence of H’s isolation from the field.

The costs of publishing are brutal, especially for scholarly works. Even for a book aimed at a general readership, such as this one, editorial oversight is left more and more in the hands of authors, not all of whom are good researchers and editors. It is no slight on the MIT Press to identify editorial gaffes in this book. Here are some: “Edmund Sapir” (xii); “Haidemacks (Russian brigand banks)” (6); “Lisker never did work alongside ... Chomsky ... because once named to the faculty at Penn, and was therefore busy teaching...” (41); “concur with these matters on man of these matters” (43); “discrete about his emotions” (64); “our little Ras Shamra book” refers to Harris & Montgomery (1935), not to H’s *Grammar of the Phoenician Language* (89); “but ... he ascribed to rigorous and scientific methods” (93), presumably meaning “subscribed”; “contribution to the documenting analysis” (97); “the phoneme may be grouped” (100); “Kingwa Swahili” (103); “Boaz” (293 endnote 64); “to provide a ‘criteria of relevance’” (117); Sapir’s *Selected Writings* (1941) is confusedly merged with *Language, culture, and personality; essays in memory of Edward Sapir*, the collection of writings for a Festschrift that was published as a memorial in 1941 (118 and endnote 81); quotations cite McCawley (1976b), for which there is no bibliographical information (152–153); endnote 12 on Chomsky’s comment about DoD funding (153) refers us back to endnote 1, which is an Einstein reference; “his lifelong corpus” (166), evidently intending “oeuvre”; “Bruce Nevin disagrees” but B doesn’t say with what in Wheeler’s review he disagrees, and doesn’t note refutation there of claims by other reviewers, including those whom B cites here; “Brower” for “Brouwer” and “Godel” for “Gödel” (179); “work-occurrences” for “word-occurrences” (180); “Binlik” for Bialik (191); “the Sirens of Revisionism, who [sic] are as much a danger to our youth” (208), (B’s “[sic]” apparently supposing mechanical alarms rather than mythological temptresses); “the similarity of wartime situations move economies in this same direction” (244); “it was never affirmed it as” (endnote 4 p. 289); Koerner’s so-called ‘biography’, two paragraphs on p. 509 of the journal, is cited as being on p. 527, the start of the “Publications Received” section. H is identified (41) as “the Benjamin Franklin Professor of Linguistics” when Chomsky met him, though that chair was not conferred upon him until decades later, and it is not clear in what sense he “met” him then since “he had known him ... since childhood” (42). “Swiggers offers an excellent assessment of what Harris highlights in the preface to *The Collected Works of Edward Sapir*” (119) refers actually to Swiggers’s “Introduction to Zellig Harris’s Text”, that is, his introduction to H’s review of the 1951 *Selected Writings* as reprinted in vol. 1 (2008) of the comprehensive Sapir collection. Several URLs are incorrect: The Zellig Harris website has been relocated to [zelligharris.org](http://zelligharris.org); this affects e.g. references in endnotes 48 and 51 of Chapter 6 and perhaps elsewhere. The Zipf website (endnote 7 of Chapter 5) is now at [www.nslj-genetics.org/wli/zipf#](http://www.nslj-genetics.org/wli/zipf#)

B employs an old-fashioned citation style. There is no list of cited references, though the Harris bibliography published by Koerner in 1993 is appended (without attribution!). The first reference to a work is given in an endnote. Subsequent references are cited with only the author’s name and a page number, with occasionally *ibid.* for immediately consecutive endnotes. Sometimes there is no endnote, as when “Stanislawski writes” introduces a quote on p. 59.

Sometimes a reference is unclear. “It is important to consider Noam Chomsky in this regard” (27): in what regard? This is an odd way to introduce the section “Extended family relations”. If it refers to Meyer Rabban perceiving H as a “guru” when he became convinced to join Avukah, at the end of the preceding section, then B is attempting a strained link indeed to the theme of this section, “how tightly knit and connected the Harris family was” (29), and “how interconnected this group was” (28) around them. After this introductory sentence, the remainder of the section is about everybody in Avukah except Chomsky, who by his own account “came onto the scene when Avukah was already over” (307). Again, we read “Given that Harris had originally dreamed of being a physicist” (29), as though referring to prior information, but this is the sole mention of this youthful aspiration.<sup>25</sup> Oddly, B doesn’t use it to help build up a connection between H and Einstein. Provocatively, “his relationship . . . went beyond professional and entered the personal” (62); “Harris’s belief in more open relationships” (69); “Harris had another child” (72)—these allusions prior to their referent can only have been intended to titillate the reader. I suppose I am doing MIT Press a favor by saying that to satisfy any prurient interest the reader will have to obtain the book. Or they could read the Wikipedia page on Harris.

B refers to “a memorial service for Zellig Harris, held at the University of Pennsylvania [where] family members, former Avukah members, and colleagues came to speak on his behalf” (83). Whatever memorial service there may have been, B is confusing it with a very public event in a large auditorium at Penn in 1997, organized by Bill Evan and others to celebrate the posthumous publication of *The transformation of capitalist society*, combined with a plea for support of independent bookstores. Over Bill’s objection, Seymour Melman insisted on inviting Chomsky as the main speaker, for the sake of drawing a crowd. The recollections of Ted Live and Tami Harris, as B quotes them, accord well with mine. Chomsky spoke at great length about East Timor, mentioning Harris briefly only once and his book not at all. At the beginning of the Q&A, I asked him if he had anything to say along the lines of H’s book about how we might foster the seeds of a successor to capitalism, which (as the book affirms) after all had improved the standard of living of the great majority of people in spite of its problems. “Problems!” he replied, “Capitalism is a disaster”, and off he went at great length on all the things wrong about capitalism. I have long regretted not having the courage to follow my impulse to interrupt and say “OK, OK. A simple ‘no’ would suffice.” Instead, I yielded the microphone as he carried on and the next questioner waited. Tami Harris says “at the time we were furious. It was disrespectful, and even more so considering their common history and his betrayal.” At the celebratory dinner that evening, the Chomsky family sat apart in an alcove. I observed in particular that Zellig’s brother Tsvee was visibly furious.<sup>26</sup>

This event highlights an important difference between the two men in their approach to politics. Chomsky is a very public lightning rod, always an intense and eloquent, even prolix, critic of what is wrong. Harris was a very private man, economical and precise of expression, who sought always to understand social systems and how they are enacted by their human participants, so as to foster change for the better. Both have performed a valuable service, one far more visibly than the other.

“In 1980, Zellig Harris retired from his long career at Penn” (168) and continued his research and teaching uninterrupted at Columbia rather than staying on at Penn in emeritus status, as commonly happens. I have heard it asserted that he was forced out, but nothing more substantial than rumor. However that may be, the department thereupon changed radically. I had suspended my Ph.D. matriculation in 1970 to go do fieldwork on a nearly extinct language in the northeast corner of California. This, like my initial work on Sapir’s Yana texts, which led to it, was intended to challenge H’s model with data from a language that was utterly different from English. (H welcomed this. While I was gone, Daythal Kendall wrote an operator-grammar restatement of Sapir’s grammar of Takelma under H’s direction.) When I resumed the doctoral program at Penn in 1987, a department “tolerant toward work done outside [Harris’s] framework” was supplanted by one that required conformity to then standard

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<sup>25</sup> Watt tells me that H joined the National Academy of Sciences in the physics section.

<sup>26</sup> Chomsky appears to attribute Bruria’s anger at him “by the mid 1960s” to a change in her commitment to anarchism (311).

theory, and quite inimical to my interest in Harris's theory of language and information. The most hospitable response was from a visiting professor where I worked at BBN with whom I was taking a syntax course, who said I could write a paper on Harris's treatment of island phenomena "if I found it intellectually challenging." I was (and am) struck by the contrast between a prizing of intellectual challenge and an interest in finding out what is true and stating it as simply as possible. As H said in letters quoted as an epigraph to the book, "It is quite important for me to find out what gives" and "the only fun in science is finding out what was actually there."

The Frame of Reference project was a protracted<sup>27</sup> investigation into how societies change, grounded in the relationship of personality to culture. Philosophical and popular extrapolations from relativity in physics gave general currency to the term and concept of a frame of reference. As Walter Lippmann wrote (*Public Opinion*, 1922):

We are told about the world before we see it. We imagine most things before we experience them. and those preconceptions, unless education has made us acutely aware, govern deeply the whole process of perception. They mark out certain objects as familiar or strange, emphasizing the difference, so that the slightly familiar is seen as very familiar and the somewhat strange as sharply alien....

As James Atherton notes (<http://www.doceo.co.uk>), "frames ... are obligatory ... not only in the sense that sharing a similar frame is necessary in order to gain acceptance into a professional community; but also in the sense that once acquired, one cannot step out of them. Once one has learned to read, one cannot not read text." In order to "consider what is relevant to the possible elimination of class rule" (quoting now from the archived ms.) requires new terminology and concepts to supplant

... inadequate concepts (e.g. power; or the confusion of industrial and business managers); this is largely due to political usefulness (to capitalism) of certain confusions. For all these reasons (and in spite of the communicational disadvantages of introducing new terms) the concepts used here differ in many respects from the capitalist ones, and frequently also from the Marxist.... The new terms are no mere translations of known ones. They are selections of basic interpersonal relations or behavior whose position in the network of social relations permits an efficient description both of the current situation of capitalism and of the changing of society out of capitalism.

"[M]any people who knew him very well knew nothing about the FoR project, including Meyer Rabban, Chava Rapkin, Irene Schumer, and William Schumer", all of whom were connected to H through Avukah. "The process unfolded on the basis of an elaborate outline that provided a road map, so that each contributor would know where things were supposed to fit." B says the thousand-page ms. collection now in archives "was mostly written by Harris and by people of the generation before Murray Eden and Seymour Melman" (45). A recent biography of Paul Mattick tells us

Almost everyone in the Frame of Reference [group] was an academic, with participation fluctuating between six and twelve individuals. Insofar as they had an articulated purpose, it was to bypass Marx's theory-laden critique of capitalism while arriving at similar conclusions through an analysis of empirical data. Unpublished papers by Harris were the main focus of discussion.<sup>28</sup>

It would appear that B has not read the text so as to make a judgment for himself as to how 'opaque' it is. B's phrase "the generation before Murray Eden and Seymour Melman" is difficult to understand even in academic rather than biological terms, since of individuals he names Karush was born in 1914, Melman

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<sup>27</sup> B says "decades", Naomi Sager says "a decade +". In the ms material archived at Penn by Seymour Melman, there are three revisions of the complete table of contents, dated 1947, 1948, and 1949, and an annotation identifies a later page as "before 1951". It was submitted for publication to the Left Book Club, which existed from 1936 to 1948.

<sup>28</sup>Roth, Gary. 2014. *Marxism in a Lost Century: A Biography of Paul Mattick*. Leiden & Boston: Brill Academic Publishers; p. 221.

in 1917, and Eden in 1920. Then on the next page we are told that Eden and Melman were also contributors. We might infer that linguistic workers on discourse analysis of political writings (also largely unnamed) were contributors, certainly Sager was. Chomsky has said nothing about it. Sager tells me that it was very much a collaborative product, although H did most of the final writing-up.

*The transformation of capitalist society* is a distillation of parts of the FoR corpus. (The manuscript title was *Directing social change*; the publisher made the substitution posthumously.) Again, what B has to say about it seems to derive from what reviewers have told him. In this book, H directs our attention to areas of economic and social activity that are neglected by capitalism because they are insufficiently profitable, which therefore may become loci for emergence of a successor to capitalism, just as capitalism emerged in areas of feudal society that were of marginal interest to its ruling class. H identifies a number of such fallow areas, and explores a variety of ways in which these have been and could be seed points from which alternatives to capitalism could emerge. In this, there is no commitment to any particular -ism or political line, but rather an affirmation that what emerges from the inadequacies of capitalism may be unexpected and new. Disregarding this, what B reports (pursuant to the appended interview with Chomsky) is socialism and ESOPs.

I have not had much to say about Zionism. I am not Jewish, nor am I a student of the politics of immigrant populations. Barsky is both. Avukah (“torch” in Hebrew) was founded about 1925 when H was 16 years old and living in Palestine, and lasted until the New York leadership dissolved itself in 1943. H returned to the United States sometime between age 17, when B documents him still in Palestine, and age 19, when he wrote an article for the first issue of an Avukah periodical at the University of Pennsylvania. In it, he recounts how two members of the “Palestine Youth Commission” had come from Palestine to “influence organized and unorganized Jewish youth in the direction of Zionism” (187), and were dismayed at the apathy of “the human material with which they had to deal.” It is evident that he identified himself strongly with their mission, and given their “legendary hospitality” (17) I surmise that the Harrises probably hosted them. The next year, H and his siblings were members of an Avukah reading group hosted by their parents. H gave a talk to that group on “The Ancient Hebrews”, in which he proposed (193) that

a nation exists to teach, to teach its own peculiar contributions to world culture to its neighbors, whence it passes on. [... But] the Jewish principles of World-Unity (as in Monotheism) and Justice have not yet penetrated to the world (as the principles of Greece and Rome have) ... the Jews should, by massing their forces and by centralization, make their culture more essentially a culture, and more essentially individual. For various reasons, Palestine is the only place—and a perfect place—for that ... a playground, a laboratory of ideas [where] a newer, vital, individual Jewish culture, which, as a distinct unit of a perfect world whole, will give its new and vital and individual contributions to humanity.

He distinguished “born Zionists” who never question it, and those “made” Zionists by force of intellect “who, knowing and understanding Judaism, accept Zionism because they find it worth the accepting.” In a 1929 Avukah publication, Chaim Arlosoroff proposed that Avukah was the “reserve officers training camp of the Zionist movement”, comprising exceptions to the apathetic “many” who ignored the Nazi threat, a core group who would ensure that it would “never seek to become a mass movement or organization.” (194) Zionism would provide for Jews “a center where they will not be a minority.” (249–250).<sup>29</sup> It is relevant, too, that Avukah provided a form of employment during the Depression, and that H employed a number of Avukah members to do linguistic work, helping them to avoid the draft (a benefit also accorded Chomsky a decade later, at the start of the Korean war). This was seen as relevant and

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<sup>29</sup> R infers from this 1942 statement a change, from advocacy of Arab-Jewish parity and comity to advocacy of a Jewish majority. I suspect that the point, rather, was to end the centuries of status as a recognized and often targeted minority in foreign lands. Being numerically fewer than half does not entail being culturally or politically treated as a minority.

proper in the face of growing strains of fascism in the United States, a concern that has been amply borne out in subsequent years.

In 1942, rumors of Nazi deprivations were confirmed by the shocking account given by two women who had escaped from Treblinka. People gathered in an emotionally charged rally at Madison Square Garden in New York. Melman wept, recounting it. Irene Schumer said “official people were not saying anything. They already knew these things, the leadership knew, it wasn’t only the women from Treblinka, but they knew” (48). There was a move by many associated with Avukah to redefine its purpose to be the rescue of Jews. In Schumer’s opinion, “Zellig and the more radical elements ... didn’t wish to become co-opted to the allied cause, which would undermine the broader effort towards a radical overturning of society.”

Avukah was dissolved at its national meeting in June, 1943. There is general consensus among B’s informants that this was accomplished by the more radical group in New York. The New York leaders were Seymour Melman, Al Kahn, and Meyer Rabban, joined by Seymour Martin when Melman went in the army. Glazer attributes to “Seymour ‘the serpent’”<sup>30</sup> a “plot” among the core New York leadership to get themselves elected to the national offices. But Lipset recanted and revealed the plot to the larger New York contingent. (His ‘confession’ is said to have been motivated by his Trotskyite ideology urging democratic process.) Irene Schumer recounts the dissolution of the New York enclave in recriminations (later forgiven) in the midst of the summer camp which was the location of the national convention. B does not explain how this resulted in the dissolution of Avukah as a whole. After all, as Zev Schumer says, “these central people in NY didn’t have the membership, so they could be outvoted in plenary sessions.” Was the leadership of the New York group indeed essential to the survival of the organization? If B did inquire farther into this, he does not tell us. Irene Schumer remembers “that the organization moved to New England” and renamed itself “the Intercollegiate Zionist Organization, which dissolved within the year.”

B advances several theories about Avukah’s end. First, “Several Avukahites [Schumer and Glazer seem to be his principal informants] believe to this day that Harris and Melman decided in early 1943 to disband Avukah” in preference to abandoning their purpose—establishing a homeland for Jews shared with Palestinians—in order to focus instead on saving Jews in Europe. This seems to be B’s favored theory because of the role it assigns to H. Second, Irene Schumer believes (49) that it was because “they started taking people into the army, so everyone began to disappear, and it was a hard thing to hold the organization together”. In consequence the leadership was increasingly in the hands of herself and Lillian Schoolman, but, as she recounts, even the “girls” themselves did not consider themselves competent for this (though she is equivocal). Third, concern about red-baiting was a major factor; there was great consternation at the news, at a meeting at which Melman was present in army uniform, that Murray Wax had talked about the activities of the more radical New York leadership at a meeting of some unnamed group in Washington, and an immediate decision “without much discussion” to dissolve the group. Irene Schumer says

I understand that what the leadership wanted was small study groups, like cells, so that if the government came around you’d only know the people in your cell.... they had this notion that fascism was going to take over and people were going to be at risk for having these ideas.

Melman attributes the dissolution of Avukah to pressure from the parent Zionist Organization of America because Avukah was too radical for them; Avukah was financially dependent, receiving a dollar per member from the ZOA for the operation of the organization, \$2000 per year in the New York office (52), more than \$30,000 in 2010 dollars. Chava Rapkin thinks it was just a function of young people moving into their separate adult lives in which Zionism had a less important role. Probably all of these factors were instrumental. For example, the ZOA surely redirected its resources in response to news of death camps. Recognizing these challenges, H and the New York leadership evidently decided to abandon what

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<sup>30</sup> Referring to Melman, presumably, as Lipset was called ‘Marty’, but B does not disambiguate.

was becoming “a mass movement or organization” (194) in which the educational focus would be diffused, and to retrench to a hub-and-spokes network of more dedicated workers (Schumer’s ‘cells’).

B does not explicitly make the connection to H’s role as an organizing hub for the Frame of Reference work drawing on individuals and small groups who were not all in contact with each other. At the end of his life, H integrated parts of the FoR material in *The transformation of capitalist society*.<sup>31</sup> We do not know what else may lie waiting for a suitably perceptive eye in the archival FoR material.

B closes the book on a hopeful note (284):

To look back to Harris’s and Avukah’s work today, in light of the current crises, worldwide, could provide the means to reimagine our relationship to production and a template for creating cooperation rather than competition among people now living in a failing system that seemed so brutally entrenched.

To which one can only say the good Hebrew word “amen.” And in a nice parallel, the abundant fruits of Harris’s work in linguistics lie waiting for an open-minded generation of students.

This book, however chaotic, and despite its flaws and shortcomings (none is perfect), provides essential spadework for those who wish to investigate further and understand the results that H came to in these disparate but linked fields, work which mattered to him greatly; and from it one can glean a flickering sense of who this remarkable man was, which mattered to him not at all. In this, the book performs a vital service, and we owe to its author a debt of gratitude for yeoman’s work that no one else has stepped up to. In light of the remarkable difficulty getting an obituary for Zellig Harris’s prepared for *Language*,<sup>32</sup> it may have been necessary for an outsider to do the job. There is much to be mined from this montage of secondary and tertiary sources. Let us hope that Robert Barsky makes more of his source material available on line for the benefit of future researchers.

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<sup>31</sup> This is analogous to H as a scientific researcher and the Principal Investigator in the Transformations and Discourse Analysis Project integrating his work and that of graduate students, another connection that B does not make.

<sup>32</sup> We may never learn why the task of obituarist was delayed and passed from hand to hand. Watt tells me that the obituary he wrote for the National Academy of Sciences had a like history before it was assigned to him, and that no one professes to know why.