

The conservative turn in Linguistic Landscape Studies

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The conservative turn in Linguistic Landscape Studies

by

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The conservative turn in Linguistic Landscape Studies

Jan Blommaert

The very first issue of a new academic journal called *Linguistic Landscape* (vol 1/1-2, 2015, John Benjamins), unsurprisingly devoted to Linguistic Landscape Studies (LLS), contains much to be concerned about. I happen to be a member of the editorial board of that journal (but was not consulted during the preparation of that first issue), and what follows should best be seen as a friendly and collegial encouragement to seek improvement in work done in an emerging and potentially highly dynamic and productive field. At the same time, as we shall see, my comments are not entirely disinterested; part of what I have to say is about my own work and how it relates to what is presented there.

My main cause for concern is that the first issue of *Linguistic Landscape* presents the field of LLS as precisely the opposite of what I just wrote: it suggests an *established, mature* and *fully developed* field of scholarship - a *tradition* if you wish- for which a methodological "canon" can be proposed, to be emulated in future work. Future work, then, can and might be judged as either "in" the tradition, as "canonical" work, or conversely as a departure from it, a rupture, a deviation. Several papers in the first issue explicitly position the kind of Ethnographic Linguistic Landscape Analysis (ELLA) developed by myself and my colleagues in the latter sense (e.g. Blommaert 2013; Blommaert & Maly 2015).

The development of ELLA - to sketch my own point of view - was an attempt to *do more* with LLS than what was done in the "first wave" of studies (see the references in Blommaert & Maly 2015). This first wave, I insist, was marked by a *synchronic, static and quantitative approach to hypostatized "languages" in a given physical arena*. Old-school multilingualism-in-space, so to speak, in which "languages" - their presence as visual markers - were *a priori* seen as indicators not just of *present populations* (of "speakers") but also of *regimes* of multilingualism, readable from the relative (quantitative) preponderance of language A over languages B, C, ... n in a given space. The critique of this general (and widespread) theoretical stream has been abundant and is ancient by now; I need not reiterate it.

Against this approach, we pitted an *ethnographic* approach, in which signs as seen as *traces of multimodal communicative practices within a sociopolitically structured field which is historically configured*. Note, for clarity's sake, three major points here:

- *Ethnography is intrinsically historicizing*, because any form of effectively performed (and ethnographically monitored) communicative practice can only be made meaningful because of its (Bakhtinian) histories of production and uptake by nonrandomly positioned actors. Contrary to what is widely assumed, ethnographic research is the exact opposite of synchronic, snapshot-based inquiry. I have belabored this point extensively in a long stream of publications; one can check chapter 6 of Blommaert (2005), chapter 5 of (2010) and the entire *Grassroots Literacy* book (2008) for clarification.
- The theoretical backbone for the first point can be found in the neo-Whorfian, Hymesian and Silversteinian tradition of linguistic anthropology - not elsewhere, for its roots (like those of any intellectual enterprise) are not accidental nor freely exchangeable. What was said above about the meaningfulness of the sign is an exact empirical reformulation of that central concept in this tradition: indexicality. Failing to grasp that point, and its theoretical implications, renders dialogue about it rather pointless.
- In addition, the effort is driven by an *ethnographic understanding of social "structure" as dynamic, fragmented and essentially stochastic*, i.e. "chaotic": while the general vector of change can be determined, the actual outcomes of processes of change are relatively unpredictable, even if they appear "logical" post factum. Random aggregates of processes generate nonrandom outcomes, and *change is the "system" we observe*. Note that this is a departure from established Durkheimian-Parsonian understandings of "structure" as that which *dominates* the "large" ("macro") processes in social life. Practically speaking: "structure" can reside in the *exceptional*, the near-invisible, rather than in the dominant. The politics of a place is not readable in a self-evident way from the volumes of particular signs displayed in that place.

Those fundamental theoretical choices explain why, in ELLA, the work of political-historically astute scholars such as Gunther Kress, Christopher Stroud or Adam Jaworski, and ethnographically sophisticated ones such as the Scollons takes pride of place. It does not warrant too much explanation that these choices are *ontological, methodological* as well as *epistemological*. As for the latter: we assume that LLS, thus performed, might

bring something unique and valuable to higher levels of generalization about societies, their histories, dynamics and structures.

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Let me now sketch the way in which LLS is imagined as a tradition in the first issue of *Linguistic Landscape*. This is done in the opening position paper by Monica Barni and Carla Bagna, "The critical turn in LL: New methodologies and new items in LL". The point is quickly summarized. Barni and Bagna observe that since its inception by Landry and Bourhiss in the 1990s, LLS has seen an expansion, both in foci of interest - different spaces, different types of signs, different forms of multilingualism - and in methodological orientations. As for the latter, they welcome the "interdisciplinary" expansion of LLS which now comprises linguistic, sociolinguistic, sociological, political-analytical and semiotic approaches. And this, they emphasize, is *in itself* an "innate critical turn" - even if the question as to what this turn would precisely be critical *of* remains, intriguingly, unanswered. It is interesting that no mention is made of ELLA in the review provided by Barni & Bagna of this critical-turn-through-expansion, in spite of the explicitly critical intentions behind ELLA I described earlier.

This expansion of the field of LLS is also seen, in itself, as a firm refutation of the criticism leveled in Blommaert & Maly (2015) against the general trend of the "first wave", summarized above. For in the conclusions to the paper, suddenly our comments emerge, albeit in a curiously truncated form. And they are rejected as follows:

"The studies mentioned in the above paragraphs (and many others that could not be mentioned for reasons of space), contrary to the assertion by Blommaert & Maly, demonstrate that once LL was identified as a field of analysis, a number of perspectives opened up immediately. These made it clear above all that studying the LL does not mean limiting oneself to counting the languages present in it, but involves contextualizing the analysis, broadening it to encompass the actors who shape or use the landscape and the factors which have contributed to its formation over time (...). In addition, different investigative methodologies need to be used depending on the research objectives."

No mention is made of our main point of criticism: the absence of an ethnographic-historical approach which uses sociolinguistic objects as an infinitely sensitive tool to detect sociopolitical change at levels and in sites usually not judged relevant. Or to put it differently: the surrender of all ambitions to say something substantial about *society* in

ways not usually done by sociologists, anthropologists, historiographers or political scientists, in favor of a preference to stick within a safe, "canonized" synchrony grounded in a kind of instant sociology of the most conventional kind - to reiterate Glyn Williams' (1992) now classic critique of mainstream sociolinguistics. Obviously, the ontological, methodological and epistemological dimensions of our critique remain unaddressed, and I see no reason to consider them refuted to any degree. Barni and Bagna seem to believe that *saying* that certain options are not being taken (or even worse, *wishing* that they were not being taken) equals *effectively* not taking them. It is to be hoped that *Linguistic Landscape* does not take this intellectual effortlessness as its guideline.

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The issue, to be fair, does make an attempt towards scoping the expanding field of LLS as described by Barni and Bagna. There are admirable papers by, for instance, Adam Jaworski, David Malinowski and Amiena Peck & Christopher Stroud. All three of these papers are genuinely explorative, even idiosyncratic (which I welcome), and show the creative use of LLS techniques deployed in atypical fields. Jaworski's analysis of art in public spaces, for instance, offers a welcome broadening of the objects of LLS from strictly linguistic to more broadly semiotic ones, and directs us towards a sociosemiotics of public space not constrained by questions of multilingualism. Peck and Stroud, by focusing on "skinscapes", expose the simple but often overlooked fact that linguistic landscapes also, usually, include people moving through them, and that the bodies of these people are equally "readable" within such landscapes. And Malinowski's study emphasizes the value of linguistic landscapes as important learning environments, adding a superb potential for application to the fundamental-scientific interest of LLS.

But the paper by Barni and Bagna is immediately followed by a paper by Eliezer Ben-Rafael and Miriam Ben-Rafael, "Linguistic landscapes in an era of multiple globalizations". And this paper does exactly what we took issue with in our critique of "first wave" LLS. Ben-Rafael & Ben-Rafael compare three cities: Brussels, Berlin and Tel-Aviv. They count languages as occurring in public space there and focus on the quantitatively dominant ones as the ones informing us of "structural" aspects of the place. This quantitative and sociologically baffling approach is couched in the most superficial snapshot-analytical "background" on these three cities (and the actual sites of research therein). (1) And this mixture is next presented as something that should yield fundamental and insightful facts about the social effects of globalization.

Here, once more, we see how LLS practitioners believe that *change* can be analyzed by looking at everything that does *not* index it, in ways that preclude any advanced understanding of its dynamics. If the presence of this paper in the launching issue of *Linguistic Landscape* is to be understood in a programmatic sense, as an announcement of more to follow, it means, sadly, that the “first wave” of LLS will never really end.

It gets worse. In a truly perplexing text, Aneta Pavlenko and Alex Mullen discuss "Why diachronicity matters in the study of linguistic landscapes". I would of course nod approvingly, given my own take on the intrinsic historicity of any form of actually performed language and the amount of argument I brought to this precise issue. But Pavlenko and Mullen take exactly *my* work as an example of anti-historical argumentation in the field of LLS and beyond. Pavlenko also takes issue, elsewhere, with the way in which Ben Rampton and myself discuss superdiversity - e.g. Blommaert & Rampton (2011) - but manages to overlook, in the exercise, the rather fundamental fact that we see the role of the internet as crucially defining superdiversity as a historical moment. (I shall return to this issue shortly.) This obvious and elementary misrepresentation is repeated in the paper discussed here.(1)

I shall be forgiven for being a wee bit impatient in the face of facile critique based on an avoidance of almost everything I have to say on the topic. One can disagree about something, but that something needs to be precisely and fully represented; if not, one finds oneself facing a straw-man argument of dubious intentions and no weight. I shall focus, instead, of how this argument operates in the general concept of the launching issue of *Linguistic Landscape*. And I can be brief.

Pavlenko and Mullen discuss linguistic landscapes from classical antiquity, with forms of language now often called "languaging", as "proof" of the fact that superdiversity must have occurred in ancient history and that, consequently, contemporary claims about the newness of the phenomena of sociolinguistic superdiversity by people such as yours truly are grossly overstated. They commit three substantial errors while building their case.

- One: they base their argument on a *surface-formal inspection of linguistic forms* and disregard the *sociolinguistic conditions* under which such forms operate. While I, and others, have repeatedly and elaborately explained that not the forms of language - complex patterns of mixing and new forms of creolization,

for instance (see e.g. Rampton & Sharma 2011) - are "new" or "unique", but the sociolinguistic conditions under which they emerge, are distributed, and acquire social value in social life (see e.g. Blommaert 2015). Pavlenko and Mullen's assumption, thus, is that *linguistic* similarity equals *sociolinguistic* similarity, which is a simple denial of most of what sociolinguistics has been trying to establish over the past six decades.

- Two: Extrapolating from the first error, they assume *unbroken historical continuity* indexed through similarities of forms of language across time. Similarities in traces of communicative patterns over time would, absurdly, suggest similarities in social systems over centuries. I am eagerly awaiting evidence for that assumption.
- Three, they define *history as diachrony* - a very widespread error in our fields of study, and the topic of several of the critical writings of mine I mentioned earlier. Time *itself* is not the stuff of history: it is time filled with human social agency; this simple statement defines modern history as a science. It is the reduction of history to diachronic comparison that leads so often to that classical antihistorical error called *post hoc propter ergo hoc*: when we see things "reoccur", they must be related to their "previous" occurrence.

To the extent that this paper should represent the "historical branch" of LLS in the expanding scenery scoped by Barni and Bagna in this launching issue, the message is disconcerting, for it suggests that superficial formal diachronic comparison equals historical research. So doing, it re-emphasizes precisely what used to make much of so-called historical-comparative linguistics so useless and misleading for scholars of the history of societies. On this issue too, there is a rich critical literature which appears to be entirely overlooked (e.g. Irvine 2001).

As to the way in which ELLA is used to make this point: the paper by Pavlenko and Mullen should add to the "refutation" by Barni and Bagna of what ELLA claims to contribute to LLS. And both fail in their attempt. It is regrettable that ELLA, as a genuinely constructive (and critical, to be sure) attempt to "expand" LLS, is discussed in such a deeply cavalier and intellectually compromising way in this issue.

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The straw man built by Pavlenko and Mullen, as we have seen, revolves around a reduction of what people such as I have to say on superdiversity to just migration

patterns in physical space. While our argument (and the point at which we fundamentally deviate from, for instance, Steve Vertovec's view) has always been that it is the confluence between such new migration patterns and the presence of new technologies for communication and knowledge distribution, that has shaped a fundamentally new *sociolinguistic* environment. And this brings me to a final observation.

The internet is the largest and most complex social space on earth now. It is a profoundly multimodal space which engages hundreds of millions of individuals in communicative practices that did not exist before the 1990s and have entirely reordered what we understand by "repertoires", "knowledge of language" and "language usage". It also reshuffles the empirical character of what is *public* and what is *space* (or better: *timespace*) in ways that not just invite but demand profound theoretical re-imaginings - paradigm shifts, if you wish. There is nothing "virtual" to the internet - this very text on my blog is as real, as effective, and as published, as any traditional hard-copy journal article or book. In spite of all that, the "virtual" is either avoided in mainstream research or segregated into some specialized "e-studies" niche of scientific practice.

This is unfortunate, for the challenges offered by the historical transformation of the infrastructure for social interaction and community formation are *general* and *inevitable*. It is this awareness that underlies my sociolinguistic work, for the present episode of globalization cannot in any way be understood without a profound and integrated engagement with the electronic knowledge-and-communication engines that are *intrinsic* to it and exert a *systemic* influence. And it is precisely because of the fact that this historically unprecedented change in infrastructure is a *communicative* thing, that our fields of study may have a bit of an edge over others: no one can accurately comprehend the contemporary world without comprehending the new complex communicative modes and economies it generates.

Not a single paper in the launching issue of *Linguistic Landscape* presents research on the "scapes" we see on the internet. Signs in public space - the object of LLS - are conservatively, even stereotypically, defined as hard-copy signs in physical "offline" space, within clearly demarcated timespace configurations. I do not see an unlimited potential for theoretical and methodological expansion in that conventional domain, but I do see such potential when we *also* consider signs in the "virtual" public arenas in which all of us are presently profoundly socialized, and in which we spend large chunks

of our lives these days. It is the way in which the new modes of communication merge and interact with old ones, and so reshape existing communicative economies at all levels of social life and from metropolises to margins in the world, that should concern us.

The innate critical potential of LLS, I would suggest, needs to be searched there. For I repeat what I said earlier: we cannot understand contemporary society by dismissing its historically unique infrastructure as an elementary area of inquiry.

Notes

1. One example. The authors announce that they included, in their Brussels study, the linguistic landscapes of the “Flemish quarter” of Linkebeek. First, Linkebeek is not part of Brussels, but that is a detail. More interestingly, Linkebeek is administratively Flemish but cannot in any way be seen as a “Flemish-speaking” area, as its population is overwhelmingly, and even politically militant, Francophone. In fact, one of the typically nagging Belgian political issues of the past few years is the appointment of a Mayor in Linkebeek. By law, the Mayor should use Dutch as the working language, but the Linkebeek population elected by a landslide (and later, emphatically re-elected) an uncompromising Francophone politician as its Mayor, leading to the refusal of his confirmation by the Flemish government, and so on and so forth. It’s not a new thing: as a teenager growing up in Brussels in the 1970s, I attended numerous rallies protesting the “Frenchification” of Linkebeek. *C’est la Belgique*: an administrative identification problematically imposed upon a different demographic identification. So rather than “Flemish”, it would be safer to call Linkebeek “superdiverse”.
2. See for an example of this misrepresentation: https://www.angl.hu-berlin.de/news/conferences/Archive/2014/sloganizations-in-language-education-discourse/abstract_pavlenko_edit.pdf. Note that the paper by Pavlenko and Mullen is a revised version of a paper delivered at a workshop on "Linguistic Landscapes Ancient and Modern" held at All Souls College, Oxford University in the Summer of 2014, where I was present. I discussed these issues with the authors on that occasion.

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