

Volume 5

Number 2

December 2018

Multilingual Margins

A journal of multilingualism
from the periphery

Centre for Multilingualism and Diversities Research
University of the Western Cape

ISSN 2221-4216

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Omphile and his Soccer Ball: Colonialism, Methodology, Translanguaging Research

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In this paper, I am reviewing autoethnographic method in translanguaging research. I tell a story that is based on a casual and unplanned encounter with Omphile, a seven year old boy with whom I interacted using communicative practices that confirmed the suppositions of translanguaging theory but also challenged the methods that support empirical observations of translanguaging research—in equal measure. The paper signposts the promises that autoethnographic approaches hold for researching naturalistic human communication in ways that side step the language and methods of the positivist tradition. I argue that in the same way that contemporary sociolinguistics theorisations remind us about how communication is not limited to determinate languages or codes, research does not have to be limited to controlled, systematic scientific methods. The framework of autoethnography reviewed in this article is one example of a praxis that is anti-methodological and, thus in line with many of the anti-foundational premises of translanguaging theory.

Key words: translanguaging, research methodologies, autoethnography, experimental research designs, researcher-as-participant, linguistic systems, translingual practices, linguistic ideologies

Framing the problem

Scholars of sociolinguistics and allied disciplines have made quite commendable theoretical and conceptual progress when it comes to challenging linguistic normativity and those frameworks that have crystallised into some kind of traditional orthodoxy in language research. Such progress is attested by the burgeoning of theorisations around language as process, dating back to

the 1970s and 1980s work of Einar Haugen, Lachman Khubchandani, John J. Gumperz and Howard Giles. By the 1990s the cacophony of voices following this line of critique had grown, with Lachman Khubchandani (1997) proposing what he called “plurality of consciousness” and “communication ethos”, which are about consideration of how individual language users have “day-to-day, moment-to-moment successes that make language transactive, functional and

alive” (Khubchandani, 1997: 14). This was a call to shift the locus of enunciation and see language as an ongoing process of social transaction and not something that is located in an institution.

The critique of conventional understandings of language has continued to gather momentum in recent times with the emergence of quite contemporary theories such as ‘transidiomatic practice’ (Jacquemet, 2005), ‘polylinguaging’ (Jørgensen 2008, 2010), and ‘codemeshing’ (Canagarajah, 2011). ‘Translinguaging’ (García 2009; García & Li Wei 2013; García & Kleyn, 2016) and ‘metrolinguism’ (Pennycook & Otsuji, 2015) are the latest additions to the long list of contemporary sociolinguistic theorisations. These theories echo Blommaert, Leppänen, Pahta and Räisänen’s (2012: 18) advice on the need to start with our “feet on the ground from a strong awareness that the phenomenology of language in society has changed, has become more complex and less predictable than we thought was.” A crucial foundational premise shared by these theoretical frameworks is their call for unbounding language from its position as an object of study and situating it in the sociocultural complexity that surrounds speakers’ ‘real language use’ (Ndlovu 2015).

What also unites the majority of followers of this scholarly tradition—in a rather negative way—is their reliance on conventional research methodologies that are limited to controlled scientific experiments: oral interviews, surveys, focus groups, participant observations, and so on. This article argues that notwithstanding the theoretical and conceptual innovations that have been made, there is a gap that is yet to be filled in contemporary sociolinguistics research. This is about doing research using methodologies that are consistent with the anti-foundational stance of emerging theories such as translinguaging.

Current conventional scientific methods and the language they use have rarely been challenged or problematised. This invites several questions centring on the ways sociolinguists continue to be wedded to conventional methodologies in language research. If we recognise that the phenomenology of language is so complex and that the ways human beings communicate eschew any easy generalisations, why do we still do research using the same conventional methods that are used to investigate languages as ordered and enumerable objects?

How realistic is it for new philosophies of language to claim they are pushing scholarship forward in a new direction when their theoretical suppositions are supported by data generated through conventional research methods? How do we do ethnographic social science research in ways that allow us to capture the complex relations between society and communication resources? In other words, can we really claim to be theorising in unconventional ways when our methodologies remain conventional? I address these questions by narrating and analysing a story that is based on my casual and unplanned encounter with Omphile, a seven year old boy whose communicative practices prompted me to think more critically about widely used methods in social science research.

The style of presentation I use departs slightly from conventional academic narrative techniques in that it does not have the usual elements of a research essay such as research methods and procedures, research design, sampling techniques, and so on. This is because the article is a reflective piece that reports on a random unplanned observation of naturally-occurring communicative practices. The paper, is therefore, in line with the frameworks of autoethnography, which is “narrative research that entails a double narrative process, one that

includes the narratives generated by those participating in the research, and one that represents the voice of the researcher as narrator of those narratives” (Kratzis & Green 1997 cited in Méndez 2013: 280). Additionally, the central themes of the analysis align with debates around reflexivity in ethnomethodology (Watson 2005, Colombo 2003, Czyzewski 1994) and approaches of conversational analysis that elucidate basic aspects of human sociality that reside in talk (Mazeland 2006, Heritage 1995, Atkinson & Heritage 1984). I discuss autoethnography in greater detail in a later section with an eye on prospects and possibilities for enriching translanguaging research methodologies. I also make in the same section some passing remarks on the relevant theoretical and methodological insights of reflexivity and conversational analysis to support my argument that the things that we know so foundationally as ‘languages’ are not as straight forward as they are thought to be. But first I would like to narrate the story of my encounter with Omphile.

Encounter with translingual Omphile

In August 2016, I attended the *Third International Conference on Language and Literacy Education* that was organised and hosted by the Wits School of Education at Witwatersrand University in Johannesburg, South Africa. In the afternoon of Day Two there were three parallel sessions themed ‘Translanguaging Lesson Demonstrations’. Although I had a very keen interest in seeing what a translanguaging lesson would look like, I was underwhelmed by the so-called translanguaging lessons that were not different at all in form and content to the traditional bilingual or dual medium

of instruction demonstrations. The translanguaging lesson demonstrations reinforced a view of languages as fixed and bounded objects that are separate from each other—supposedly because the presenters’ misunderstood translanguaging pedagogy. Owing to my frustration over the ‘translanguaging lesson’ demonstrators’ limited understanding of what the theory and praxis of translanguaging is all about, I decided to slip out of the conference venue and took a short walk around the Parktown Campus of Wits University. I then sat on a chair on one of the campus courtyards, reading the conference program—reflecting on how some scholars were missing the crucial message of translanguaging theory. Little did I know that this was, in fact, going to be an opportunity for me to witness conversational practices that would prompt me to analyse contradictions between contemporary sociolinguistics theorisations and the methods used to collect data that support such theoretical positions.

While sitting on the chair, I saw this little boy coming from the other end of the campus kicking a soccer ball and seemingly unbothered by the few cars that drove past. As he got closer to me he slowed down his pace of walking and kicking the ball. He then stopped and greeted me using the honorific Setswana/Sepedi greeting ‘*Dumelang*’ (literally: plural form for ‘hello’) to which I replied using the singular form ‘*Dumela, ukae?*’ (literally: singular form for ‘hello, how are you?’).

The rest of our conversation, in multiple languages that we both moved in and out of, subconsciously, went as follows (M = me; O = Omphile):

M: (speaking in isiZulu) ‘*Ungubani igama lakho?*’ (What is your name?)

O: (with a little smile): ‘Omphile’.

M: (speaking in isiZulu) ‘*Kutheni udlala wedwa?*’ (Why are you playing alone?)

- O: (speaking in isiZulu) *'Anginaye ubhuti noma usisi. Ngihlala nogogo'* (I do not have any brother or sister that I can play with. I live with my grandmother).
- O: (this time around mixing isiZulu and Setswana expressions) *'Wena awunaye umosimane?* (Don't you have a son?)
- M: (mixing isiZulu and English expressions): *'Nginaye but umkhulu kakhulu, unaseventeen'* (I do have a son but he is big, he is seventeen).
- O: (looking at me with a sullen face and gasping a sigh of great disappointment) *'Ah! ngibe ngisithi mhlawumbe ungangami, ngifuna adlale nami'* (Oh, no! I thought he was of my age, I would have wanted him to play with me) .

Based on this last statement, I came to realise that my new-found friend, Omphile, really wanted someone to play with. So, I offered to kick the soccer ball with him to which he jumped with a lot of excitement and declared from start (again using a mixture of expressions from isiZulu, Setswana, Sepedi and English) that he was going to beat me. So, we quickly identified some temporary goal posts and started kicking the ball. After about five minutes of play he had scored three goals while I had zero. Each time he scored he would jump up and down in excitement, declaring in isiZulu, Sepedi, Setswana and English that he was a very good soccer player and that I was never going to beat him. It was, indeed, a lot of fun until after I temporarily caught up with him by scoring three goals, thus making it a draw.

We continued playing, with Omphile really determined to prove that he was unbeatable. He eventually scored two more goals, which was a very big win for him. I kept on trying hard but I could not catch up with him. So, in the end I gave up; he still wanted to keep on playing but I had to go back to

the conference venue. In order to bring the game to an end, I admitted that he was the winner and gave him a few coins as a way of conceding defeat. The soccer game eventually came to an end after 20 to 25 minutes of play.

So, what is the point of this story? It is not so much about me meeting a seven year old boy and playing soccer with him. Two things are of significance here: (i) the dynamic of our interaction, the ways in which we negotiated and deployed our respective linguistic systems; and (ii) the methodological implications of my empirical observations that were not based on pre-planned approaches of the scientific method. It is evident that the interaction I had with Omphile and the attendant language practices confirmed both the theoretical suppositions and empirical observations of previous sociolinguistics scholarship described in the first and second paragraphs of this article. The soccer game itself was in many ways a form of language; an integral part of the discourse and praxis of communication in naturally-occurring environments. As the literature on conversational analysis has posited, single acts are parts of larger, structurally organised entities, also known as sequences (Schegloff 2006). The most basic and quite important sequences consist of actions performed by one interactant, which invite particular types of further actions performed by another interactant; and so on. The actions can be vocal (as in question-answer, greeting-greeting, invitation-acceptance/declination) or performative (as in gestures or partaking in an activity of mutual interest) (Schegloff 2006). My interaction with Omphile consisted of all of these. Furthermore, consistent with the suppositions of reflexivity, the soccer game in particular was a constituent part of communicative practice I am describing here and, therefore, elaborates the circumstances of our interaction and

conversation while simultaneously being elaborated by them (Watson 2005: 7)

Another equally important point that is at the core of this paper is about how my empirical observations were made outside the orbit of mainstream social science methodologies – thus tying in with the tenets of the anti-conventional agenda. I did not go out with a pre-planned research idea built around a scientific experimental design that sought to address some pre-conceived research questions. Neither Omphile nor I did at any one point attempt to raise the question about which named language(s) each one of us could speak well as a way of establishing common ground (Goffman 1981; Enfield 2008) in our interaction. We did not even bother to find out whether there was any named language that we had in common. From the very start of our conversation, we tapped into our respective linguistic systems that emerged naturally and spontaneously during the course of our interaction. Although my knowledge of Setswana and Sepedi is quite limited, I did not alert Omphile to this when he passed a greeting in these languages. Neither did I ask him about his level of knowledge of both isiZulu and English, the other two languages that contributed to the linguistic systems that we used throughout the course of our interaction.

Another notable point is one about the blurring or porosity of language boundaries that was evident in my conversation with Omphile. We both crossed effortlessly – and even disregarded – the supposed language boundaries as we used linguistic resources available to us in rather seamless and fluid ways. This laid to rest notions of linguistic purism whereby languages are perceived as distinctly bounded entities that are to be used in particular ways. Though named languages are real and exist in societies that have coined names for them, “they do not necessarily overlap with the

linguistic systems of individual speakers” (García and Kleyn 2016: 10). This is precisely what we see in my conversation with Omphile. The linguistic usages and interactional processes between Omphile and me are a clear example of communicative translanguaging that does not necessarily follow pre-conceived boundaries of languages-with-names. But in what ways does the story of my encounter with Omphile confirm the theoretical suppositions of contemporary sociolinguistic theorisations? And what does this story tell us about how to do research on language and communication in ways that enable us to observe and report on those casual naturally-occurring conversational data that escape the attention of conventional scientific methods? I address these and related questions in the remaining parts of this article.

An appraisal of translanguaging and allied theories

Translanguaging is one of the most recent theories of language and communication that seek to contribute a more nuanced conceptualisation of how real people communicate in everyday real life. A common definition of translanguaging is one provided by García and Kleyn (2016: 14):

[T]ranslanguaging refers to the deployment of speakers’ full linguistic repertoire, which does not in any way correspond to the socially and politically defined boundaries of named languages.

García and Kleyn further suggest a bifurcated view of translanguaging – the *weak version* and the *strong version*. The weak version of translanguaging is one that supports named language boundaries, and yet calls for the softening of these boundaries. This view, which follows hard on the heels

of traditional sociolinguistic notions of 'code-switching', and 'code-mixing' is associated with the work of Suresh Canagarajah (2011) on 'code-meshing' and Jim Cummins' (1979 & 2007) 'interdependence hypothesis' and 'transfer theories'. On the other hand, the strong version of translanguaging, which I also subscribe to, posits that bilingual people do not speak languages, but rather use their repertoire of linguistic features selectively. Seen from a translanguaging perspective, 'language' is not something that a speaker simply 'has' but a repeated and expansive system of communicative practices in which he or she continuously engages (García 2009; García & Li Wei 2013; Canagarajah 2011; and Li Wei & Zhu Hua 2013). Translanguaging, thus, becomes a summary term that should be taken in the sense of "transcending" or going beyond the two or more named languages of bi-/multilinguals (García and Kleyn 2016: 10). In this regard, it converges with other quite contemporary scholarly conversations that promote and value language as local practice (Pennycook 2010); languages as creative linguistic practices (Otsuji & Pennycook 2010); languages as plurilingual multimodal communication resources (Piccardo 2013); and languages as communicative resources (Blommaert 2010). The main argument of these studies is that boundaries between languages are somewhat temporal, porous and irrelevant if we consider the dynamic, unpredictable and spontaneous ways by which people use language as a social practice (Ndlovu 2015). This body of work begins the movement away from didactic thinking about language and how human beings communicate. However, the key point here is that although translanguaging and similar theories challenge linguistic normativity and push the debate on language theorisation towards an anti-foundational direction, they still rely on the traditional scientific method of

data collection. Attempts to transcend conventional scientific methods in most translanguaging reports have remained somewhat tentative and parsimonious as most such studies continue to rely on focus groups, oral interviews, and ethnography (in the traditional sense of 'researcher as impartial observer').

As the relevant body of literature dating back to the early 1970s has clearly demonstrated, the idea of language as object is a modernist and colonial invention that does not capture the complex communicative practices of the majority of people around the world (see for example, Haugen 1972; Gumperz 1982; Giles 1984 and Khubchandani 1997). Here is how Einar Haugen, way back in 1972, expressed his frustration with mainstream sociolinguistics theorisations: "The concept of language as a rigid, monolithic structure is false, even if it has proved to be a useful fiction in the development of linguistics. It is the kind of simplification that is necessary at a certain stage of a science, but which can now be replaced by more sophisticated models" (Haugen 1972: 325). This line of argument has been pursued further in more recent times by scholars such as Sifre Makoni and Alastair Pennycook (2007), Jan Blommaert (2010), Lesley Milroy (2001) and many others.

These scholars argue that the emergence of modern linguistics as a social science at the dawn of the twentieth century was prompted by fundamental questions around the relationship of language, thought, cognition, and how human beings interact with one another and with their immediate environment. Some well-known pioneering thinkers such as Ferdinand de Saussure, Leonard Bloomfield, Noam Chomsky, and those who followed their tradition of linguistics tried to address these issues in their work. However, they did so from a segregationist/colonial perspective, which has come to be known as orthodox/

mainstream linguistics. Segregationists treat “language and languages as objects existing in their own right, independently of other varieties of communication” (Harris 1987: 131). Such insistence on the study of language structure rather than the study of linguistic communities or communities of practice is consistent with the approaches of colonial linguistics that sought to homogenise what were otherwise disparate communities to facilitate colonial domination and control (Makoni 1998; Brutt-Griffler 2006; Ndhlovu 2010; and Errington 2008).

Therefore, the rise of the translanguaging school of thought is a welcome development not because it is a novelty. Rather it has to be seen as symptomatic of homecoming by academics and education practitioners. It signals a re-awakening and a reconnection with the foundational questions of language in society – those basic questions around how human beings communicate. The renewed interest in understanding the complex communicative practices of plurilingual and translanguing individuals is essentially about bringing back to mainstream academic conversations an important issue that had been overlooked and marginalised following the rise of modernist theories of language that have erroneously come to be seen as if they were of a ‘natural kind’. So, essentially, translanguaging is about going back to basics.

The majority of scholars who have exercised their minds on the theory and praxis of translanguaging have done so in the context of educational linguistics – in language education classroom contexts, second language acquisition, bilingual education, TESOL education, and so on (see for example, García 2009; Canagarajah 2011 & 2013; Cummins 1979 & 2007; García and Li Wei 2014; Creese and Blackledge 2010; and Hornberger and Link, 2012). The flourishing of translanguaging theorisation

in educational settings is perfectly understandable given that the roots of this theory actually lie in Cen Williams’ (1994) doctoral thesis that explored opportunities presented by the presence of bilingual children in Welsh school classrooms (García & Kleyn 2016). Some other scholars have, however, theorised and tested the applied interests of translanguaging in out-of-classroom contexts. For example, Li Wei (2011) and Li Wei and Zhu Hua (2013) use the insights of translanguaging theory to investigate transnational identities and ideologies of Chinese university students in the UK. Li Wei and Zhu Hua (2013: 516) use narrative data and ethnographic observations of British-born Chinese students (whose parents came from China, Taiwan, Hong Kong and Singapore) to “explore issues such as their socio-cultural identification processes, the interactions between their linguistic and political ideologies, their multilingual practices and what they have learned from being part of this new [transnational] social space.” Li Wei and Zhu Hua (2013) conclude by highlighting the promises of translanguaging theory in the context of identity studies. They point out that the “translanguaging approach has the capacity to demonstrate how multilayered social, linguistic and community practices and reflections yield multiplicity in identity construction. The story of my encounter with Omphile described above adds another dimension to the theory and praxis of translanguaging, that of communicative translanguaging. What we see from the moments of interaction between Omphile and myself are instantiations of linguistic boundary crossing that take place in spontaneous and unplanned social encounters in the community. The communicative strand of translanguaging (as opposed to those found in educational and transnational identity formation arenas) is located within and mediated by a different set

of conversational circumstances that call for humility, empathy, accommodation and the need for intercalants to concede space for each other's linguistic systems.

However, notwithstanding these different contextual applications, all translanguaging theorists are united on one thing, which is this: in translanguaging, named languages do exist only insofar as they have social reality and not linguistic reality. There is very little, if anything at all, that is linguistic about named languages (García and Kleyn 2016). From a translanguaging perspective, the linguistic is located within the communicative systems of individual speakers who have the capacity to appropriately leverage their repertoires in ways that would enable them to perform according to social norms while simultaneously not being constrained by such norms. Thus, in spite of their differing contexts of applied interests, translanguaging theorists are united on the fact that linguistic resources or knowledge of multiple languages are part of a single language system that an individual uses to create meaning and accomplish goals (Daniel and Pacheco 2015).

So far so good – but a glaring problem still remains: to what extent has this body of quite contemporary scholarship pushed the boundaries of language research toward a new methodological direction that encourages the use of yet to be proven and anti-foundational methods? I address this question in the next section.

Colonialism and contending methodological issues

The tenuous foundation of logical positivism continues to exert an enormous influence in the social sciences (Baronov, 2004) and this includes sociolinguistics. It seems much of the burgeoning scholarship of this tradition

is yet to break free from the conventional scientific method. There is tendency to do very little or no analysis of the underlying assumptions and beliefs that form the ideological presuppositions of widely used systematic research tools of the positivist tradition such as the questionnaire, surveys, oral interviews, focus groups and participant observations. These are often treated as if they were ideologically neutral and objective yet, as we know, they emerged out of specific contextual and cultural conditions in the Global North. The content and *modus operandi* of conventional methods are predominantly shaped by colonial understandings of what constitutes valid and legitimate knowledge (Ndhlovu 2017). The universalising tendencies of the conventional scientific method are regularly imposed on all societies (including those in the Global South) without due regard to contextual particularities. A major problem with adopting these conventional scientific methods *holus bolus* is that they also shape the nature of our research questions, what we look for or overlook in our data sets and, ultimately, our answers to such questions.

Four geopolitical assumptions that underpin the architecture of conventional methods have been suggested in the relevant social science literature (Peet 1997; Nustad 2004 and Connell 2007). First is the claim to universality whereby the very idea of mainstream research methods involves talking about universals and generalisations as if the whole world was a homogenous continuum. The fatalistic assumption of this claim is that “all societies are knowable in the same way and from the same point of view” (Connell 2007: 44). The second assumption is that of reading from the centre – the construction of a social world read through the eyes of the metropole and not through an analysis of the metropole's action on the rest of

the world. What conventional scientific approaches overlook is the fact that the experiences of cultures and societies from other parts of the world cannot be fully understood through the use of methods that arose out of a colonial metropolitan reading of the world (Ndhlovu 2017). The third problem with conventional scientific methods is one about how they are underpinned by what Connell (2007) calls ‘gestures of exclusion’. This is about the total absence or marginalisation of methodologies and theoretical frameworks from the non-Western and formerly colonised world in metropolitan texts on research. In those exceptional instances where material culture and ideas from these other parts of the world are acknowledged, they are rarely considered as part of the mainstream dialogue on research theory and method. Riding on the back of colonial ethnography and social anthropological frameworks emphasising the modern/pre-modern distinction, the method of ‘science’ renders the cultures and thought processes from the Global South irrelevant and treats them as belonging to a world that has been surpassed (Connell 2007; Ndhlovu 2017). This leads us to the fourth contour, which has been termed ‘grand erasure’. The point here is that when empirical knowledge and theorisation about humanity more generally are seen as coming solely from metropolitan society (where the roots of conventional research methods lie), the immediate effect “is erasure of the experience[s] of the majority of human kind from the foundations of social thought” (Connell 2007: 46).

All of the above put to question the claims of objectivity and neutrality that are often said to be the hallmarks of most of these scientific methods. For this reason, some humanities and social science scholars from across a range of disciplines have consistently called for breaking free from the conventional scientific method.

They include scholars who work under the banners of Southern Theory (Connell 2007; and Comaroff & Comaroff 2011); Decolonial Epistemology (Mignolo 2002, 2011; Maldonado-Torres 2007; and many others); and Decolonising Indigenous Methodologies (Linda Tuhiwai Smith 2012; Bagele Chilisa 2011). Linda Smith (2012: i–xiv) in particular raises four pertinent points that undergird my line of argument:

- That we need to develop “counter-practices of research” relevant to the agenda of disrupting the current hegemonic rules of the research game.
- That we need to articulate research practices that arise out of the specificities of epistemology and methodology rooted in people’s cultural experiences.
- That stories of research, examples of projects, critical examination, and mindful reflection must be woven together to make meaningful and practical designs.
- That we need new ways of knowing and discovering, and new ways to think about research in order to demonstrate the possibilities of re-imagining research as an activity that can be pursued outside the narrow box of the scientific experimental design.

This is about integrating praxis, theory, action and reflection in ways that provoke revolutionary thinking about the roles of knowledge and knowledge production in social transformation. These methodological issues are not explicitly addressed in the frameworks of most contemporary sociolinguistic theories. I see this as a missed opportunity to integrate new and alternative methods more fully into language research. Therefore, I argue that in spite of their anti-conventional and anti-foundational stance, most researchers that have

embraced contemporary theories of language (such as translanguaging), still submit to the use of the ‘scientific method of enquiry.’

As I have already said in the introduction to this article, earlier and present generations of sociolinguists have made major advances in terms of generating new theoretical frameworks that challenge normativity and purism in language research. However, I do not think that it is good enough for us to simply come up with new conceptual frameworks that are not complemented by equally innovative methodological paradigms. I am guilty of this omission myself insofar as I have proffered new sociolinguistics theories such as ‘the language nesting model (Ndlovu 2013) and ‘ignored lingualism’ (Ndlovu 2015) that are not supported by fresh and anti-foundational methodologies. If we are indeed serious about pursuing this type of intellectual endeavour, we need to formulate counter-methods of scientific enquiry that are consistent with the anti-foundational premises of contemporary social science theories. In addition to the much broader humanities and social science scholarship cited above, some leading international applied linguists and sociolinguists such as Alastair Pennycook and Emi Otsuji (2010 & 2015); Li Wei (2011) and Li Wei and Zhu Hua (2013) have articulated with greater clarity the call for methodological innovations in language research. Taking after Heller’s (2011) notion of critical ethnographic sociolinguistics, Pennycook & Otsuji (2015: 20) posit that we need to study contextually (ethnographically) the social use of language (sociolinguistics) with an eye to understanding relations of social differentiation and inequality. In discussing the methods that underpin their theory of ‘metrolingualism’, Pennycook & Otsuji suggest the following about what we need to do in language research:

Ethnographic research [should] not only be about the gathering of data in specific contexts, the note-taking, the recording, the questioning, the observing, nor is it only about the writing, the attempts to capture what is going on, to describe the bustle of the market, the hectic work in the restaurant or kitchen, the interactions over lunch in a construction site. It is also about the conversations, the developing understandings as we sit and talk about the market gardens, watch conical hats in the fields and the plane flying overhead and try to make sense of all this. (Pennycook & Otsuji, 2015: 44).

Drawing on their ethnographic work in metropolitan areas in Australia and Japan, Pennycook and Otsuji describe how the methodologies they used have sought to capture the “throwtogetherness of linguistic resources – across space and through different interactions and observing how resources come and go in one place – in order to relate physical activities of work, the social and historical trajectories of participants, the organisation of space and the language resources at play in particular places” (p. 88). Along the same vein Li Wei (2011) pioneered the innovative method of *moment analysis* that is based on the idea that reflections of the critical moments often result in fundamental learning that enables individuals and groups to uncover or create knowledge from their own experiences for improving their future actions (p.1224). Lei Wei applied the method of *moment analysis* to a study that used a combination of observation of multilingual practices and metalanguage commentaries by three Chinese youths in Britain. He says “metalanguaging data can be collected through conversations, individual or group interviews, journals and autobiographies” (1225). Although

moment analysis seems to still retain some footprints of the conventional scientific method, it takes a rather different turn by focusing on ‘moments’ of interaction. Thus, the data collection processes and procedures of moment analysis do not necessarily follow the sequential, systematic, directed and controlled approaches of the conventional scientific tradition.

This is precisely the methodological direction that the story of my encounter with Omphile is taking us. In the next section build on and extend the methodological innovations of this previous body of work by reviewing the framework of autoethnography as a possible explanatory paradigm for my empirical observations in the story I narrated above. Though it has been applied widely in other social science disciplines, autoethnography is rarely used as a method in language research. I describe below the insights of this approach and spotlight the promises it holds for a more innovative methodology of doing language research in ways that are in line with the anti-foundational stance of contemporary sociolinguistics theories.

The case for autoethnography

The origins of autoethnography are traced to the 1980s, what Holt (2003: 18) calls the ‘crisis of representation’ period because this was a time when researchers were concerned about formalising qualitative research to be as ‘rigorous’ as quantitative research. It was also that point in history when qualitative researchers found themselves using diverse research strategies that were borrowed from the quantitative paradigm (Méndez 2013). Autoethnography, therefore, emerged as a response to this challenge and to increasing “calls to place greater

emphasis on the ways in which the ethnographer interacts with the culture being researched” (Holt, 2003: 18). Steven Pace (2012: 4) says the earliest uses of the term ‘autoethnography’ are found in a 1979 essay by cultural anthropologist Hayano who made a case for self-observation in traditional ethnographic research. In more recent times, the term ‘autoethnography’ has come to be associated with the work of Carolyn Ellis (2004, 2007 & 2009) and Arthur Bochner (1997, 2000, 2001 & 2002). By way of definition, Ellis & Bochner (2000: 739) say autoethnography is “an autobiographical genre of writing and research that displays multiple layers of consciousness, connecting the personal to the cultural.” In a later publication, Ellis, Adams & Bochner (2011) elaborate this definition further, noting that autoethnography expands and opens up a wider lens on the world in a manner that eschews rigid definitions of what constitutes meaningful and useful research. They posit that the autoethnographic approach “helps us understand how the kinds of people we claim, or are perceived to be, influence interpretations of what we study, how we study it and what we say about our topic” (Ellis, Adams & Bochner 2011: 2). In their *Handbook of Autoethnography*, Jones, Adams & Ellis (2013) provide an extended explanation of what autoethnography as method entails .

Autoethnography as method is about using unconventional ways of doing and presenting research. Some such non-conventional ways include the use of conversational styles of presentation that make the narration engaging and emotionally rich. As Ellis (2011: 3) further advises “‘Telling’ is a writing style that works with ‘showing’ in that it provides readers some distance from the events described so that they might think about the events in a more abstract way. Adding some ‘telling’ to a story that ‘shows’ is

an efficient way to convey information needed to appreciate what is going on, and a way to communicate information that does not necessitate the immediacy of dialogue and sensuous engagement.” What this essentially means is that autoethnography provides room for the researcher/writer to use first-person to tell a story. This is especially powerful when the writer tells in an intimate way a story he/she observed or an interaction he/she participated in. It is precisely for this reason that I see the story of my encounter with Omphile as a good example of autoethnographic praxis in language research. Some of the subtleties of my interaction with Omphile that I have presented in this article might have been missed were it not for the first-person narrative style that enabled me to ‘tell’ and ‘show’ my eyewitness account in my own words. The first-person narrative technique provided me with the opportunity to tell the story as I experienced it without waiting for others to express what I, as a researcher-participant really wanted to be known and understood (Richards, 2008). To summarise, it is worth quoting Anderson (2006: 388) who says “the definitive feature of autoethnography is this value-added quality of not only truthfully rendering the social world under investigation but also transcending that world through broader generalisation.”

The main contours of autoethnography that set it apart from the procedures of mainstream experimental research designs are six-fold.

- The author of an autoethnographic research report usually writes in the first person style, thus making himself or herself an integral part of the object of research.
- Writing autoethnographically allows for the researcher’s life to be studied along the lives of other participants in a reflexive connection. The researcher engages in analytic

reflexivity, demonstrating an awareness of the reciprocal influence between him/herself, the setting and other participants (Chang, 2008). It is here that autoethnography aligns with the views of scholars who follow the tradition of reflexivity, which expresses “the inextricability of ordinary descriptions (such as typifications of persons, actions or situations) from the circumstances they describe, [whereby] the description and the circumstances are reciprocally-elaborative” (Watson 2005: 7).

- The accessibility of an autoethnographic writing style helps position the reader as an involved participant in the dialogue, rather than as a passive receiver (Pace, 2012).
- Autoethnography enables the researcher to demonstrate commitment to theoretical analysis while simultaneously capturing (in an accessible style of writing) what is going on in individual lives or socio-cultural environments (Ellis, 2004).
- The richness of autoethnography is found in those realities that emerge from the interaction between the self and its own experiences that reflect the cultural and social context in which those events took place (Méndez, 2013: 284). On this point, the auto-ethnographic approach compares quite favourably with conversational analysis (CA), which “studies the organization of talk as situated, socially organized sets of practices ... as interactional structures that both shape the context in which they operate and enable its interactionally coordinated progression” (Mazeland 2006: 156). As in autoethnography, the main focus in CA is on systematic practices such as overlap positioning and overlap resolution, collaborative turn construction, and the role of gaze, gesture and body positioning

(Schegloff 2000, Lerner 1996, Mazeland 2006).

- The subjective interpretations that may arise from personal narratives oppose the positivist view of research which aims at presenting an 'objective' account of the truth. The personal and emotional involvement of the researcher in autoethnography thus counterbalances the rather distant and perceived 'objective' role of the researcher in a positivist stance (Méndez, 2013: 284).

Therefore, the distinct advantage of the method of autoethnography is that in addition to reporting about other participants, it also makes the researcher/narrator part of the research story. It engenders collaboration between the researcher-as-participant and other participants, thus levelling the power imbalances that characterise most conventional social science methods. In the context of my story with Omphile, autoethnography clearly doubles as a method for generating my empirical observations about our interaction, and as a framework for presenting the story and making sense out of it. The autoethnographic approach enabled me to construct a narrative that side steps the language of conventional ways of doing and thinking about research.

Like all other methods or conceptual frameworks, autoethnography has had its fair share of criticisms. Three such criticisms follow. First, autoethnography has been dismissed on perceptions of being insufficiently rigorous, theoretical, too aesthetic and emotional (Delamont 2009). Second, those scholars following the autoethnographic approach have been accused of doing too little fieldwork, observing too few cultural members, and not spending enough time with different others (Anderson 2006). The third criticism levelled against autoethnography

is about how the researcher uses personal experience, hence supposedly biased data that does not fulfil scholarly obligations of hypothesizing, analysing and theorising (Ellis 2009; and Madison 2006). I see these criticisms as biased in the sense that they evaluate the utility of autoethnography using standards of the scientific experimental method. Autoethnography does not subscribe to the procedures and processes of conventional approaches. It is a totally different methodology that seeks to inaugurate 'an-other logic', 'an-other language' and 'an-other way' of doing research that has the potential to liberate social science research from the clutches of hegemonic conventionalism. I would argue that it is, in fact, these perceived limitations of autoethnography that hold the promise for doing research in ways that are in line with quite contemporary anti-foundational social science frameworks such as translanguaging, metrolingualism, and many others. The conceptual and methodological premises of autoethnography enable social scientists to ask big questions of small data (Salazar, Elliot & Norum 2017), which clearly sets them apart from the conventional scientific method that is largely pre-occupied with big data. I would argue that although research methodologies that are driven by big data are useful in certain contexts, they also tend to hide more than they reveal – hence the need for approaches such as autoethnography that help us see the big picture out of small data. This way we get to see and learn more about the minute but quite significant human interest stories that often remain hidden in the masses of big data.

Conclusion

What I have done in this article is to extend the application of the insights of autoethnography by deploying them to

explain the nature of human communication and linguistic usages in unplanned naturally-occurring encounters. I have also located the discussion within contemporary scholarly debates in sociolinguistics and related frameworks of reflexivity and conversational analysis as way to give my write-up the academic flavour that will, hopefully, make it resonate with the majority of target readers. There are at least four important points that can be gleaned from the story described and analysed in this paper. First, the moments of interaction and conversation that Omphile and I had from the very first point of contact through the mini-soccer game disprove – in very clear and unequivocal terms – popular assumptions about the need to first establish the existence of a common code with our interlocutors prior to initiating a conversation. In naturally-occurring human communication, the boundaries of named languages can be crossed without much recourse to deliberate bridging processes such as translation and interpretation. And, in the process of crossing language boundaries, we also simultaneously cross social boundaries and social distances. This creates opportunities for us to enter and experience each other's life-worlds, thus paving way for the establishment of common ground, thus ultimately leading to effective communication and mutual understanding.

Second, the willingness to participate in a common practice paves way for effective communication. The conversation between Omphile and I applied a transactive approach to language use whereby the deployment of our respective linguistic systems was an ongoing process of social transaction. This enabled us to recognise the “synergic network of plurilingual language use as a means to inspire trust in cross-cultural settings” (Khubchandani 1997: 37) as we played the soccer game as if we were

old time friends. Therefore, what this story tells us is that speakers need ways of negotiating difference and converging on practices of mutual interest rather than negotiating codes that are shared with others. Such strategies of managing and accommodating linguistic difference without necessarily resorting to standard language ideological approaches teach us that communication always works (not in spite of) but because of rampant diversity of language practices (Ndlovu 2015: 410).

The third take-home message is one about the centrality of humility, empathy and willingness to come down to the level of our interlocutors in establishing the common ground needed for effective communication to take place. Although Omphile and I had never met before, we were able to establish very good rapport and sustain our conversation not on the basis of a common linguistic code. Rather, our successful and productive interaction was sustained by our mutual willingness to accommodate each other's linguistic systems and social interests. Both of us were ready and willing to participate in a common social practice – the mini-soccer match – which eventually saw us exist as a small community of practice with shared interests. Throughout our interaction our linguistic practices tended to fluctuate depending on our individual and collective evaluations of how our communication process was going. I, in particular, expanded and contracted my linguistic system at various stages during our interaction as a way to accommodate the developing linguistic system of a seven year old. The overall outcome was that both Omphile and I felt very comfortable in communicating and playing with each other.

The fourth point is this: although the conventional scientific method of positivism remains entrenched as the established way of doing research due to its perceived objectivity and

neutrality, it has a dark side. The things that we know foundationally about the conventional method of science (research questions, research design, sampling techniques, and so on) are neither objective nor neutral. They are laden with subjective ideological presuppositions, assumptions and beliefs tied to contextual particularities and cultural specificities of those regions of the world from where they originated. In particular, the very close and intimate association of the positivist tradition of scientific enquiry with the rise and spread of colonial modernity means that many of its common sense assumptions need to be rethought. We need to re-think, for example, the supposed universal relevance of established approaches to research. The majority of them originated from the locality and particularism of social and cultural conditions of the Global North, and then generalised to all other societies – through colonial and other imperial processes – as if the whole world was a homogenous continuum.

Overall, the dynamic of linguistic usages that I ‘show’ and ‘tell’ in the story of my interaction with Omphile would have been missed if I were to follow the scientific method that emphasises conventionalism and systematicity. What we learn from the story I narrated and analysed in this article is that autoethnographic praxis of language research that is anti-establishment is possible – after all. It is possible to develop innovative methodologies that allow us to be specifically attentive of the small details of everyday life that present opportunities to ask big questions of small data.

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Commentaries on Omphile and his Soccer Ball: Colonialism, Methodology, Translanguaging Research

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Commentary on “Omphile and his soccer ball: colonialism, methodology, translanguaging research”

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The paper provides a spirited defense of autoethnography as a legitimate viable anti-colonial methodology for sociolinguistic research, particularly within the normative multilingual contexts that characterizes global south linguistic communities. The author, using the story of his unplanned encounter with a seven-year-old boy, Omphile, illustrates the value of making research sense of what may appear as mundane personal experiences and encounters.

Following an introduction that narrated how scholars of sociolinguistics challenged linguistic normativity and presented language as a process of social interaction, rather than an isolated institution, the author narrates his encounter with Omphile in August 2016. Omphile, like the author, is multilingual. They meet at a University park, while the author is taking a break from an on-going conference. Without prior knowledge of each other’s linguistic repertoire, they engage in a conversation in which they effortlessly utilize four linguistic codes – isiZulu, Setswana, Sepedi and English. Their conversation spans the period of a 20-25-minute soccer game in which Omphile eventually turns out as the victor. The two significant points for the author are (1) how they both employed their linguistic repertoire towards a meaningful discussion, without any prior knowledge of what codes they had available; and (2) the methodological implications of his observations of this encounter, considering that it was not planned and not based on so-called scientific research methods.

In view of the suggestion that translanguaging is a framework within

which “socially and politically defined boundaries of named languages” do not hinder the “deployment of speakers’ full linguistic repertoire” (García and Kleyn, 2016, p. 14), the author considers translanguaging an appropriate framework within which to make theoretical sense of this encounter which he deems to be a reflection of the social reality of global south communities like the one he and Omphile find themselves in. His defense of autoethnography is set against the acceptance of logical positivism as the foundation of supposedly sound social science research. For him, these long-accepted and unquestioned assumptions are not ideologically neutral and objective as we have been made to believe; they are steeped in cultural and contextual conditions favorable to the global north. The author concludes that using autoethnography allowed him to exhibit a community of practice where successful interaction does not rely on common shared codes, but rather on the willingness of interactants to participate in a common social practice, while expanding and contracting their available linguistic systems to accommodate each other’s linguistic systems.

The idea of autoethnography as an alternative compelling methodology for southern scholars is undoubtedly made in a persuasive way by the author. Expanding our knowledge systems should not rely only on particular Western conventional scientific methods underpinned by positivism. Autoethnography which allows an engagement of the readers in a personal narrative from the author is in sync with oral narrative traditions of many African cultures. Oral story-telling traditions have for generations been an authentic avenue to pass knowledge down across generations. The narrative power and feature of autoethnography is reminiscent of the time-tested method

through which members of many African communities make sense of their world, share knowledge and teach important aspects of their cultures. Even though autobiography is presented in the written form, the accessibility of the story-telling approach used makes it an appealing option for readers, and widens the scope of reach of otherwise dense academic write-ups. The narrative about the author's encounter with Omphile presents readers with a number of lessons: (1) the multilingual reality of their community and how that facilitates communicative encounters; (2) the constant covert negotiations that form an integral part of daily communicative practices in such communities. (3) the effective communication that takes place in the context of multiple codes without the use of intermediaries.

While big data driven research has been viewed as largely objective and therefore has the tendency to influence policy, the author's compelling narrative and the theorization that follows provides an example of how small data can be a useful window through which we can understand our varied worlds. Autoethnography allows for the qualitative researcher to "study things in their natural settings, attempting to make sense of, or to interpret, phenomena in terms of the meaning people bring to them" (Denzin and Lincoln 2000, p.3).

In spite of the advantages of autoethnography so lucidly presented through the story of Omphile and his soccer ball, the author seems to be oblivious of the limitations and disadvantages of autoethnography. One thing which I kept wondering about as I read his encounter and the theoretical capital he made of that was the issue of ethics and accuracy. Is Omphile aware that he has become the object of a (scientific) study? He was obviously looking for companionship or a playmate, but he did not bargain

to be a research participant. Even if he became aware that his interaction is being used for research purposes, as a seven-year-old, he is not in a position to provide consent for his interaction to be used for such academic purposes. These sentiments of ethical considerations are echoed by Méndez (2013). The other issue to consider is how accurate the transcript of this encounter is. Presumably there was no recording of the encounter, yet the author is able to recount the conversational interaction verbatim, even if most of it happened over a soccer game. In the absence of note-taking or recording, the veracity of the narrative, like in many autoethnography narratives, becomes the prerogative of the author. The basis for data verification by a third party is unclear. Additionally, the engaging 'telling' posturing of autoethnographers leads to the criticism that autoethnography appeals to emotions rather than rationality. As asserted by Bochner and Ellis (1996, p. 24), "autoethnographers don't want you to sit back as spectators; they want readers to feel and care and desire". How are we able to account for feelings and desires within the context of scientific/academic exercises?

There is no doubt, as lucidly presented by this author, that autoethnography as a methodological approach allows for global south academics to tell their stories and experiences according to them and on their own terms with the added advantage of making research sense of our everyday world, however the glaring challenges of this approach, particularly those bothering on ethics have to be confronted and addressed rather than muted.

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Doing research in the field of language and society: ways to go beyond the constraints of modernist science paradigms

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My main aim in this brief comment is to share a few reflections related to my understanding about what it means to do research in the field of language and society, considering the insightful reflections of my colleague Finex Ndhlovu in his article *Omphile and his Soccer Ball: Colonialism, Methodology, Translanguaging Research*. First of all, coming from a Southern context, I share with him the feeling of uneasiness with the ways research is/can be done in the area, although at the same time, from my point of view, I see a different broader picture that I will try to sketch here.

I share Ndhlovu's concern about the way qualitative research has been appropriated as a generic perspective on doing research in any field of the

human sciences and how this has been replicated in university courses in different parts of the world. I remember when I was a visiting PhD student in the UK participating in courses that taught techniques of qualitative research that were completely disconnected from the epistemological concerns of different areas of knowledge and had little application to the specificities of the diverse contexts of the investigation brought by the students. However as I came from a different context of research, Brazil, an academic environment influenced by the experiences of diverse countries, I understood that it would make no sense from the point of view of my investigations to subscribe to the model of qualitative research that I was being exposed to in the UK and that I would do better to follow my own path. During the course of my PhD, I explored different epistemological perspectives, finally deciding to follow a more 'traditional' stream of ethnographic research in sociolinguistics proposed by Hymes and Gumperz in the 1960s. My reason for this was because of the opportunities provided by that framework, for me to better understand the specific problems that I identified in my investigation.

Despite the fact that ethnography and participant observation are considered to be the *prima facie* examples of qualitative research in different handbooks of social sciences, I would argue that ethnography should not be understood in this way. Rather than just a 'technique' or 'method', it is fundamentally a specific epistemology and ontology. The quality of the research produced depends largely on the deep and long-term involvement of the researcher with the participants and the community where the research is done. This is why that despite the beauty of the human connection and mutual engagement that emerged out of the interaction between my colleague,

Ndhlovu and Omphile recounted in the paper, I would be careful to draw any generalization from that. As all elements of social life can be meaningful, we need time to grasp the different and diverse ways in which socially significant meanings emerge and evolve. Although I can appreciate the depth of engagement in the interaction between Ndlovu and Omphile – and could comment at length on it – it is not clear to me how this interaction can ever really be representative of the multiple ways that Ndlovu and Omphile constitute themselves as subjects in the world. Why do they interact in the way they interact? How do they learn – or understand in the moment of engagement – how to use the different resources of their linguistic and cultural repertoires? What does this interaction reveal about their life histories? Is soccer a main feature of their identities? Why is it so? For me, answering questions such as these is the reason for doing research at all.

In ethnography, theory is not the main, nor the final, result of the research; it is actually a by-product of attempts to understand the complexities of how social realities are constructed by different humans in situated contexts. Likewise, our tools of analysis also emerge out of our prolonged endeavours to understand what it means to be human under specific circumstances. Usually in the case of sociolinguistic inquiries, the complexities of engaging linguistically arise out of the realities of inequality. The research of Hymes and Labov, for example, even if they were produced in the Global North, were the result of an engagement with people that were marginalized in their contexts. Despite the fact that translanguaging has become a fashionable new term to discuss multiple forms of language mixing, the way it was constructed in the beginning as a scientific concept, had

connections with very specific realities and political struggles in the US and in UK. In my perspective, science should be produced in those spaces where the understanding and changing of specific social realities intersect. This is why the transplantation of concepts such as translanguaging or autoethnography is sometimes not really useful as they are not the most adequate answers to a specific context. Autoethnography should be seen as one approach that can be useful and necessary in a given investigation, keeping in mind, as always, that the specific conditions of the field must determine just how appropriate it is in any given instance.

As reflexivity is a main feature of my inquiries, I consider all my research to be an exercise in autoethnography to a greater or lesser extent,. However, I think it is important to carefully consider what level of personal disclosure or exposure of the other I should include in my writing. This is because I am aware that notions about the self and about individuality are not homogenous - quite the opposite: The way people talk about themselves in research is usually a reflex of a modern kind of European self that may lack an equivalent in different cultures. It is important to remember that even notions such as humility and empathy are culturally mediated and that there are no universal ways of presenting the Self , and that because of this, even such sentiments must be seen as emergent characteristics of social interaction.

I totally agree with the idea that emotions should be considered as integral to all research in the social sciences/. Solidarity, friendship, care, love and other feelings play an important role in our academic efforts. However,, this does not mean leaving aside rigorousness in research, and neither does it mean subscribing non-critically to the methodological perspectives of qualitative research; nor to fall back

on a positivist view of science. For me, rigorousness means a deep commitment through long-term involvement - reflexively and from different perspectives - to understanding the multiple layers that constitute linguistic interaction, cultural experiences and social realities. To claim this is not simply to reassert conventional thinking about science. Rather it is to acknowledge that producing relevant science is a tool for struggle, and a means whereby non legitimated forms of life may be legitimated. Because of this, it has the potential to be a tool to face inequalities. From my perspective, to decolonize science is not merely an endeavour limited to the deconstruction of what is produced in the main Northern research centres. It is fundamentally about opening up spaces for multiples ways of doing research. This means it is about using different languages and multimodal resources, and creating spaces for previously delegitimated knowledges to be legitimated, what is actually, an enormous task, but an indispensable one to change the language games played in the global field of science.

Response to “Omphile and his soccer ball: colonialism, methodology and translanguaging research”

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“Omphile and his soccer ball” is an over-long, over-heated polemic (any academic who proclaims that his position on anything “has the potential to liberate social science research from the clutches of hegemonic conventionalism” needs an editor, as well as perhaps a Valium) with a seriously under-cooked point, and I hope the author won’t mind too much if I respond in a combative tone similar to the one he uses throughout the paper.

The author argues that theoretically, sociolinguistics has recently “made quite commendable theoretical and conceptual progress”. In terms of methods, though, the discipline is lazy. It still relies on “conventional” and “traditional scientific” methods, which the author enumerates as “focus groups, oral interviews, and ethnography (in the traditional sense of the ‘research as impartial observer’)”. These cobwebbed tools – all of which the author sloppily and unfairly ultimately boils down to the icky goo of “positivism” – cannot adequately capture “the dynamic, unpredictable and spontaneous ways by which people use language as a social practice”, we are informed. The cure for this methodological malady is what the author calls “autoethnography”.

I deliberately write “what the author calls “autoethnography”” because despite the patter of references to the concept, the author’s use of “autoethnography” is idiosyncratic. First of all, one might wonder what exactly is ethnographic about the single, decontextualized example he uses to illustrate the concept. I know that “ethnography” is used in a footloose and carefree fashion by many sociolinguists, sociologists, political scientists and others, but even when those scholars use the term most rapidly, it still usually means more than *a single interaction*. If an interaction like the one described by the author constitutes ethnography, then what, one might wonder, *isn’t* ethnography? The author’s elastic use of “ethnography” to denote a single interaction stretches the word so thinly that it loses any distinctive meaning whatsoever.

And then there is the “auto” part of all this. It seems to me that if the author was serious about exploring the power that reflexive responses to language have to inform sociolinguistic theorizing, he would have proposed examining memoirs that foreground, precisely, speakers’ reflections on their

life in languages: Eva Hoffman's *Lost in Translation*, Alice Kaplan's *French Lessons*, and Vladimir Nabokov's *Speak, Memory* all come to mind as examples. The extent to which memoirs like those constitute "autoethnography" may be debatable. But they certainly seem to fulfill the author's criteria (which he discusses in his section "The case for autoethnography"). And however one regards them, perhaps we might agree that they all contain much more powerful insights into "those basic questions about how human beings communicate" than does a seven-line remembered example of an exchange between strangers.

That example is the nerve that scaffolds the paper; the soapbox the author stands on to rail against "traditional scientific" data. But what does he propose instead? That we should treat as reliable and illuminating data his memory of a multilingual exchange with a complete stranger. The author presents the brief conversation with the titular boy named Omphile in conventional transcript form as though he transcribed it from a recording. But unless I missed something, he didn't record the conversation. He remembered it. Why should we trust that the author's memory of this unexpected interaction was not only accurate, but *exact*? Call me old-fashioned and pedantic, but I don't. To be able to say anything insightful about how people actually use language, give me "traditional scientific" data any day.

And then there is the point of all this, the goal of the author's proposals. One unstated but clearly evident goal seems to be self-aggrandizement. From what I can tell, the author is very pleased with himself. He characterizes his interaction as an example of "humility" and "empathy". He presents it as an example of "anti-establishment". His interaction with Omphile, he feels, constitutes a "collaboration" between them; one that "level[s] t he power

imbalances" and one that can "provoke revolutionary thinking about the roles of knowledge and knowledge production in social transformation".

That is a lot of bravado to wring out of a seven-line conversation. To the extent that any of this is autoethnography, it is autoethnography at its least reflexive and least edifying.

The paper ends by listing "four important points" that the author wants us to glean from the story he tells.

The first of those four points is a straw man argument. Is there anyone who actually believes that people "need to first establish the existence of a common code with our interlocutors prior to initiating a conversation"? What does a claim like that even mean?

The second point is banal ("the willingness to participate in a common practice paves the way for effective communication"); and the third is both obvious ("our successful and productive interaction was sustained by our mutual willingness to accommodate each other's linguistic systems and social interests" Grice lives!) and self-congratulatory, in addition to being debatable (on what basis should we believe that Omphile shared the author's perception of their interaction as one in which they acted like "old time friends"? In what sense and on what grounds, precisely, does the author mean that Omphile's "linguistic system" is "developing", and that the author accommodated specifically to this?).

The fourth and final point is that "the conventional scientific method of positivism...has a dark side". That dark side is the colonial legacy of both the methods used in sociolinguistics and the knowledge that they produce. Valid and important criticism, that; one that many scholars are directing their attention to, as this paper makes clear. What is difficult for me to see, though, is how a decolonial approach to language

– or anything else – is really furthered by a paper that makes heady claims to progressive scholarship but that backs them up with only a tiny droplet of data whipped up into what amounts, in the end, to little more than a self-important soufflé.

Slippery notions and trickster habitus: putting translanguaging in its place

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Finex Ndhlovu, in his paper, ‘Omphile and his Soccer Ball: colonialism, methodology, translanguaging research’, joins a swelling cohort of scholars who engage with the possibilities of ‘de-linking’ from or exercising ‘epistemic disobedience’ towards a northern inclination to privilege knowledge (epistemology), theory and research methodology thought to originate in Europe, and sometimes North America (Mignolo, 2007, 2009; Connell, 2007). Although many scholars in post-colonial or minority settings have tried to de-link, most of us must admit that there is no clear ‘abyssal line’ between the coloniality of northern thinking and de-coloniality of southern thinking (e.g. Santos, 2012). Instead we are caught within a web of entanglement (Kerfoot & Hyltenstam, 2017). However, if we engage in critical reflexivity of how different systems of knowledge, belief and ways of being have come together and diverged in post-colonial settings, we may be able to arrive at what Rodolfo Kusch calls ‘a mestizo consciousness’ ([1970] 2010). In this, I understand him to mean that this is a consciousness that allows us to have some understanding of how and why things have come ‘to be’ in ‘northern’ and ‘southern’ thinking and experience (that is, to be able to have some insight into two world views). De-colonial

thinking, to my mind, therefore does not require cleavage, but it may well require what Bourdieu has called ‘epistemic reflexivity’, as carefully explicated by Linus Salö (2018) in a recent issue of *Multilingual Margins*. If this is the case, while we need to continue to attempt to disentangle northern and southern thinking, ‘mestizo consciousness’ helps us to see into two (or more) different worlds of knowledge, and to understand how difficult it is to separate them.

Ndhlovu, like Linus Salö (2018) makes a case for reflexivity in research practice, but the approach each takes is not the same. Salö’s focus is on ‘the principle of epistemic reflexivity’ that holds steady a critical eye on the researcher’s own epistemology and notices how this articulates with the eye on the ‘object’ of research. Ndhlovu’s focus arises not from a deliberative focus on reflexivity as method, but rather he arrives at it incidentally after he escapes from what he experiences as an annoying reproduction of coloniality in teaching practices. He arrives therefore in an unplanned situation, with a perspective of translanguaging in mind (an object of his concern). It is then that autoethnography (a methodology) seems to find him rather than the other way around. Ndhlovu’s attention becomes bifurcated between two ‘objects’: one is his experience of autoethnography as a process that he argues is essentially an ‘anti-colonial’ methodology; the other he identifies as ‘translanguaging theory’ which he regards as an ‘anti-foundational’ theory of language. So, whereas ‘epistemic reflexivity’ would involve one eye on the researcher’s epistemology (e.g. how the researcher’s views of language have come about etc.) and one eye on the ‘object’ (e.g. translanguaging), Ndhlovu effectively has two objects about which he reflects. These are his experience of the methodology (autoethnography)

and his experience of translanguaging. In his reflective encounter with Omphile and post-encounter reflections, it was not Ndhlovu's intention to reflect on how his own epistemology of language or translanguaging has been formed, by whom, under what circumstances, and the degree to which this may matter.

I turn now to Ndhlovu's concerns with translanguaging, a relatively new term that has been somewhat controversially appropriated from its Welsh origins (Williams, 1996) and re-purposed in an alternative, apparently new, discourse of linguistic fluidity in New York, Birmingham and London (e.g. García, 2009; Blackledge & Creese, 2010, García & Wei, 2014). Ndhlovu acknowledges that discussions of what we might call linguistic fluidity are not new; they have been circulating among prominent linguists for at least the last 50 years. This chimes with Rama Kant Agnihotri (2007, 2014) who refers to an even longer history of scholarly studies of linguistic fluidity amongst scholars in Europe, India and North America, although then discussed in contemporary vocabulary of the day. After four decades of exploring linguistic diversity and fluidity in India, the UK and South Africa, Agnihotri argues that 'we do not need to invent any new terms ... if we appreciate the true nature of language (i.e. multilinguality)' (2014, p. 364).

Ndhlovu regards translanguaging as a 'anti-colonial' pedagogy and theory. He notes that translanguaging, appropriated from Welsh bilingual education and re-purposed first in New York, and then in Birmingham and London, has become theory, which practitioners in South Africa apparently have not understood, hence his annoyance. In my view, there are a few leaps here that would require suspension of disbelief if one were to go along with the idea that there is 'translanguaging theory'. Ndhlovu is not the only scholar

to be swept up in the fast-moving and alluring current of translanguaging discourse that arises from small-scale ethnographic studies in large metropolitan centres of Birmingham, London and New York, and that inform ambitious theoretical discussions that have received significant coverage in major international forums (e.g. Wei, 2017). These have captivated several linguists searching for 'transformative' pedagogies in South Africa (e.g. Makalela, 2015, 2017; McKinney, 2016), even though the pedagogies have emerged in very different circumstances than those found in African countries. While it is the case that one of the most persuasive proponents, Li Wei (2017), is making a strong case for theory building, I am not convinced that many scholars agree that there is, or should be, a definitive or stable pedagogy or theory of translanguaging. So, it worries me that Ndhlovu indicates that there is a 'translanguaging theory'.

It worries me because I find it difficult to reconcile an assumption that northern scholars, who have recently turned their attention towards the consequences of migration, linguistic diversity and multilingualism could possibly know or understand more of multilingual practices and purposes of communication than the millennia of intelligences and generations of scholars in the most linguistically diverse parts of the world. It worries me because the idea of 'a theory' presupposes that multilingualism (in the guise of another name) in one part of the world is the same as in another. This would take us back to assumptions of the universality of knowledge and reason, and hence dangerously close to re-scripting neo-colonial habitus and hegemonic supremacy of colonial thinking.

This is fundamentally at odds with the contributions of African scholars and thinkers, including those of Léopold

Senghor and colleagues to notions of 'African socialism' and 'la negritude' (Senghor, 1964, 1998). It is at odds with Steve Biko's ethics of communalism and humanity in his conceptualisation of 'black consciousness' (1973) and Ngũgĩ wa Thiong'o's focus on 'decolonising the mind' (1986). It is also at odds with current discussions of 'southern epistemologies' (Comaroff & Comaroff, 2012; Santos, 2012, 2018). It is a stance that is out of step with the rejection of the way that Indigenous (Kusch, [1970] 2010; Smith, 1999) or southern (Connell, 2007) theory and expertise (in this case of multilingualism) is appropriated in northern discourses, repackaged and sold back in the south (Heugh, 2017).

So, it comes back to how we might disentangle (Kerfoot & Hyltenstam, 2017) the interconnectivities and appropriations of northern and southern thinking, when mindful of how trickster habitus continues to encase our educational experiences. It seems to me that there is something that gets in the way of clarity, something that contributes to the apparent success and danger of the 'translanguaging' discourse. I think it may have something to do with the way that an invocation of social justice, as did discourses of human rights two decades ago, obscures debates. Social justice is apparently a more palatable alternative to discourses of human rights, and a commitment that many linguists share. Our northern colleagues recognise fluidity and diversity, and while they seem to acknowledge context, they do not seem to understand that southern and northern contexts are fundamentally different and disparate. There are at least two key considerations that escape reflexive recognition of how these differences intersect with language / multilinguality and social justice. The first has to do with the different purposes for which people use their multilingualism and multilinguality. The

second has to do with multidimensional hierarchies and scales of marginality or majoritisation embedded in linguistic ecologies (cf. Stroud & Heugh 2011). Linguistic purpose and linguistic ecology are not the same in one place or historical moment as in another.

So, we might think of 'translanguaging' as a compelling alternative to discourses, policies and practices of exclusion; one that offers social justice and equitable opportunities for migrant and minority students in US and UK cities in the second decade of the 21st century. The linguistic ecology of such settings and the translanguaging purpose of social justice is in a context where one language, English, is a majority language that intentionally or not is used simultaneously to dominate and exclude minority students. Linguists who work beyond the metropole argue, however, that even if translanguaging might facilitate social justice and access for minority students in urban contexts, it is unlikely to offer equity or redress for vulnerable First Nations peoples and their linguistic needs and purposes which differ from those in cities (e.g. McCarty, xxx).

In Africa and Asia, none of English, French or Portuguese (however fluid or contested are the realities of these names) is a majority language. The tables are entirely turned. Here it is the minority languages that serve to dominate and exclude most people from access to power and resources. Multilingualism, multilingual repertoires and 'multilinguality', on the other hand are the norm, not the exception of minority communities. Access to higher education, high-level engagement in the formal economy and career opportunities, and international participation, even in such contexts of multilingualism, is not and will not be guaranteed through linguistic fluidity or horizontal translanguaging. It does

not matter how much sociolinguists or even applied linguists wish this to be the case. Linguists have not yet been able to change the architectural reality of power. In these parts of the world, unless people have access to what can only be described as a bounded, regulated, restricted code of at least one international language of wider communication, which in most cases is English, they won't make it through the first door of a journey towards access and equality.

Where individuals and communities are already well-versed in the art of fluid multilinguality, what they desire is to expand their multilingual repertoires with meaningful access to the restricted bounded codes that will lead them through the labyrinthine corridors of linguistic exclusion towards participatory citizenship. In such ecologies of language / multilinguality, the last 150 years has shown us that horizontal linguistic fluidity has not yet brought about social justice (Heugh, 2018). An unintended consequence of a horizontal-only approach to multilingualism, certainly in Africa and other post-colonial contexts, unfortunately, reproduces cycles of exclusion, poverty and re-colonisation. Even if it is the case in New York, Birmingham and London that horizontal translanguaging or multilingualism in schools contributes towards social inclusion, and it may well also do so in many southern contexts, it is just not enough to achieve social justice for most people in the world. It is for this reason that I suggest we look very carefully at discourses of social justice and follow the consequences of pedagogies intended to achieve this priority to see where they lead. African scholarship in the early 1990s showed us then that implementation plans in Africa seldom align with the rationale for good policy.

Ndhlovu's paper is an invitation. It is an invitation to engage in

dialectical conversations, such as his conversation with Omphile. It is through such conversations that we may find opportunities to engage in critical reflexivity about our own epistemologies, systems of belief and ways of being, and how these influence our view of the world and how we try to disentangle webs of deceit spun by the trickster habitus of coloniality. Omphile offers us a metaphor for disentanglement. If we are willing to set hubris aside, we have much to learn of the multilinguality and multilingual expertise of children (see also Wolff, 2000). As apprentice teachers, they remind us that they will become scholars in time. They will teach our children or grandchildren, as have their grandparents taught us, and they will use vocabulary that is different from the words with which we are familiar and those their grandparents used before us. As we hope that they may recall and build on lessons we teach, perhaps we owe it to their grandparents to acknowledge how our work builds upon those who precede us. It is through reflexivity, as Ndhlovu suggests, and through understanding southern ecologies of multilingualism and purposes to which people use and wish to use their multilinguality, or 'seeing the point from which you see what you see' (Salö, 2018), that both southern and northern scholars can grow a praxis that resists re-scripting colonial thinking.

Postscript: I should like to acknowledge that I have purposefully (re-) appropriated the word 'translanguaging' from northern scholars in a (southern) pedagogy of functional multilingualism that includes and balances horizontal and vertical dimensions of language / multilinguality in educational contexts.

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**Omphile and his soccer ball:
Colonialism, methodology, trans-
linguaging research**

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The paper raises significant and relevant issues on current research on language and makes a sustained demand for coherence that it claims is lacking in such research.

The author calls attention to the fact that whereas current research has moved its previous focus from language as abstract and bounded system to language as dynamic and ongoing practice, focusing on complex social transaction, much of the methodology still preferred in such research persists in following the allegedly scientific and objective methodology of established research methods and procedures and pre-ordained research design.

The writer counteracts by making a bid for greater coherence between the notion of language as social transaction and a more transactional, dynamic and less bounded methodology for studying it; he proposes autoethnography as a narrative method of research that he claims plays off at least two narrative voices – that of the subject of the research and that of the researcher himself. The writer sees his proposal as a shift in locus of enunciation, an aspect of significant import to decolonial theory and Southern theory.

The shift proposed in the article is based on the analysis of an unplanned

multilingual verbal interaction between the researcher and a child – Omphile - in the vicinity of a university campus; the interaction involves the engagement of both in a spontaneous ball-game. The interaction is then reflected upon for multilingual interactional dynamics to yield, according to the author, not just the customary translanguaging considerations on the “multiplicity in identity construction” but, more importantly, a newer dimension, which the author describes as “communicative translanguaging”.

Almost belabouring the point of lack of coherence, the author reminds the reader that though it seems to have become a given among theorists of translanguaging that a foundational concept of language has given way to the non-foundational notion that what appear to be multiple languages are actually part of an individual user’s single language system, the same innovatory and non-foundational attitude is lacking in the continued preference for bounded and established methodologies of research.

Drawing on decolonial and Southern theories, the author bases his proposal, among others, on Tuhiwai Smith’s call for

“counter-practices of research relevant to the agenda of disrupting the current hegemonic rules of the research game” and proposes a “more innovative methodology” of researching into language “in ways that are in line with the anti-foundational stance of contemporary sociolinguistics theories”.

The suggestion is that autoethnography attends to this demand by offering the possibility of a methodology that embodies ‘an-other logic’, ‘an-other language’ and ‘an-other way’ of doing research that has the potential to liberate

social science research from the clutches of hegemonic conventionalism.

Apart from the methodological issue, the article argues for due importance to be given to the establishment of common ground in multilingual transactions, contrary to what the author calls “popular beliefs” that presuppose that such transactions involve, firstly, establishing the existence or not of a common code between the interlocutors. According to the author, it is this establishing of common ground that leads to effective communication and mutual understanding.

The argument in the article against the hegemony of the scientific paradigm in current linguistic research is well founded and of great current relevance. Several decolonial and Southern theorists, besides the ones cited in the article, such as Grosfoguel (2007), Castro-Gomez (2007) and Sousa Santos (2010) have pointed to the tyranny of modern science as an ego-politics of knowledge whereby the knowledges of certain regions and certain cultures are imposed as rational and scientific. By concealing the fact that such knowledges are produced by subjects situated in specific geographical and historic locations, the purportedly scientific knowledge thus produced is given universal currency to the detriment of other knowledges alleged to be unscientific and not universal because they are seen to be produced in specific locations and thus seen to have limited, local value. By demanding that one’s locus of enunciation be specified, so-called scientific and universal knowledge becomes situated and epistemically susceptible to critique. The author of the article seems to making a similar argument, justifiable and relevant in the eyes of this reader.

A possible shortcoming of the article is that the conclusion, for translanguing

and sociolinguistic theory, that the author arrives at, from the narrative he weaves about the impromptu encounter with Omphile, takes second place in relation to the argument for autoethnography, and is emphasized only in the conclusion. I refer here to the finding that it is the ongoing establishing of common ground between interlocutors that leads to mutual understanding and effective communication. It seems that Omphile and his soccer ball in the title are merely an argument for autoethnography and other non-foundational research possibilities. But this does not invalidate or diminish the argument and thrust of this forceful and timely article.

Omphile and his Soccer Ball: Colonialism, Methodology, Translanguaging Research

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The issues dealt with in the article “Omphile and his Soccer Ball: Colonialism, Methodology, Translanguaging Research” are inspired by linguistic practices that emerged in a chance casual meeting of the author, Finex Ndlovu, with a young boy Omphile somewhere at the Parktown Campus of Wits University. The meeting resulted in a friendly soccer game between the two of them that lasted about 20 to 25 minutes. In this event, Omphile showed a translanguaging capability by employing “a mixture of expressions from isiZulu, Setswana, Sepedi and English” (page 3, this article) within spontaneous, impromptu and momentary discursive actions and performances (see Li Wei 2011) in order to create a variety of multilingual social spaces” for himself (Li Wei 2011: 1223). At no time did any of the interlocutors determine or dictate the language of engagement between them.

Ndlovu uses this event as a point of departure for a discussion of to what extent conventional sociolinguistic methodologies are adequate tools with which to approach radically *non-conventional* linguistic practices, such as translanguaging. He argues that they are not, and that the Omphile encounter illustrates the value of autoethnography as a ‘decolonial alternative’. Autoethnography is a qualitative research method that focuses on self as a study subject but transcends a mere narration of personal history (Chang et al. 2013: 18). Autoethnography allows researchers to use their own life story as data to develop their social research “that will ultimately reflect their level of comfort with emotive self-disclosure and personal orientation in conducting research” (Chang et al. 2013: 18). The bulk of Ndlovu’s paper is a reflection on how autoethnography offers a variety of insights into such phenomena as translanguaging.

In reading the article, it struck me that the communicative translanguaging emerging in the interaction between Omphile and the researcher reflects a ‘negotiated’ process of identity building in which standard language forms (and English in particular) are overshadowed by local language practices often considered as marginal. Importantly, these practices are the visible manifestations of work being done by the two interlocutors to engage with one another with empathy and good, cooperative intent. I understand Ndlovu as making the point that translanguaging, that is, the “multiple discursive practices in which bilinguals engage in order to make sense of their bilingual worlds” (Garcia 2009: 45) (and one could add, their multivocal selves), ‘mesh bits and pieces of language’ in ways not easily accounted for in the translanguaging theories developed in the North “that have crystallised into some kind of traditional orthodoxy in

language research” (page 1, this article). He argues that “the world cannot be fully understood through the use of methods that arose out of a colonial metropolitan reading of the world (Ndlovu 2017)” (see page 8, this article), and proffers auto-ethnography as a decolonial – Southern – methodology. This is because it side-steps the straight-jacketing of conventional, pre-planned approaches to data analysis (see page 4, this article). It does this by (a) not following the sequential, systematic, directed and controlled approaches of the conventional scientific tradition (see page 9, this article); (b) requiring that the researcher to be reflexive about his/her positionality; (c) committing the researcher to narrative accessibility; (d) being willing to engage with unfolding and unpredictable linguistic encounters; and (d) embedding understandings of ‘language’ as emerging the building of conviviality and cooperation rather than being the prerequisite to this. By offering a counter-point to the strictures of conventional - read ‘northern’ - methodologies, a consideration of auto-ethnography assist us in understanding what a more decolonial, Southern, account of phenomena such as translanguaging might comprise. The article thus finds its place in studies that are theorized under the umbrella of Southern Theory, Decolonial Epistemology or Decolonising Indigenous Methodologies.

In spite of the perfect intersection of the autoethnography method in capturing all the multilingual practices here described, I also agree with other voices that have critiqued this research method: While supposedly an ethnographic approach, the many advantages offered by ethnography – such as long term immersion in a ‘site’ and extensive relationship building – is lacking. Surely, though, auto-ethnography, could be combined with an array of other ethnographic tools? Or

would this subsume it into the existing, hegemonic and conventional, research methodologies that the author is arguing against?

Finally, this short soccer game also shows “multilingual competence emerges out of local practices where multiple languages are negotiated for communication; competence doesn’t consist of separate competencies for each language, but a multicompetence that functions symbiotically for the different languages in one’s repertoire” (Canagarajah 2011:1). This assumption justifies the idea the act of translanguaging is transformative in nature, it creates a social space for the multilingual language user by bringing together different dimensions of their personal worldview (see Li Wei 2011). This fact is important because it suggests hows “translanguaging opens up a space of resistance and social justice, since language practices of minoritized youth are usually racialized and stigmatized” (García and Li Wei 2015: 236).

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Translanguaging and colonialism: Some lingering doubts and nagging suspicions

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Broadly speaking, I am in full agreement with the basic thrust of Ndhlovu’s core arguments which I spell out as follows: (a) Translanguaging defies Linguistics in its traditional and time-honoured format; (b) The conceptual toolkit that the science has bequeathed to us over the years is ill-equipped to handle this phenomenon (c) The fact that the dominant languages spoken in the North have by and large been robustly normativised and “homogenised” has helped obscure “the blurring and porosity of language boundaries” which is more starkly noticeable in the more ‘chaotically organised’ societies of the South (d) There is an urgent need to shift the focus of attention from discrete, “named languages” to a study of actual individuals and their speech practices where what goes on most of the time is overlapping and dovetailing of otherwise well-structured systems to form unstable repertoires and, finally, (e) it is imperative that we look for alternative ways of data-gathering more in tune with the challenges posed by new theoretical proclivities and ditch the methods inherited from now-outmoded research practices.

I have no problem with all or any of the points that I culled from Ndhlovu’s fascinating and extremely stimulating paper. If I start penning these words by way of a riposte to it, it is in relation to a term that figures prominently in the title of the paper, but receives scant or insufficient attention along the discussion that ensues. It is the much-maligned term ‘colonialism’. Let me hasten to add that it is not at all my

aim to paint colonialism in a different light, let alone defend it. As a former colonial subject myself, I have no reason whatsoever to do so. I raise the point for the reason that I do not find in Ndhlovu's otherwise brilliant discussion any proof of a direct link between colonialism and the phenomenon of translanguaging.

An early mention to colonialism in the text is when the author speaks of "a segregational/colonial perspective, which has come to be known as orthodox/mainstream linguistics". Now, whatever link there may or not be between segregationalism and colonialism (leaving aside the unquestionably colonial overtones of the very term 'segregationalism'), one is left wondering in what sense the whole point may be deemed to be germane to the issue of any possible link between colonialism and translanguaging. A slightly more elaborate comment made a little later in the same paragraph does precious little to make the point any clearer: "[...] insistence on the study of language structure rather than the study of linguistic communities or communities of practice is consistent with the approaches of colonial linguistics that sought to homogenise what were otherwise disparate communities to facilitate colonial domination and control." Being "consistent with the approaches of colonial linguistics" is one thing; but to infer from thereon that colonialism is *responsible* for linguistics' proverbial penchant for language as a homogenized entity is stretching things a bit too far.

I make a point of drawing attention to this because I think it is all too easy to go down that slippery slope and jump to the conclusion that there is a direct, causal link between colonialism and translanguaging, thus making translanguaging—who would have thought!—yet another millstone around the necks of erstwhile postcolonial subjects. I shudder at the very thought of falling into this treacherous trap, if for

no reason other than that it would make the whole business of translanguaging one of those colonial legacies that would behoove one to get rid of. To look at translanguaging this way may turn out to be itself a sore reminder that the gaze is, despite all efforts, still from the vantage point of the North—giving coloniality the last laugh!

As I understand it, the primary objective of Ndhlovu's paper is to make case for ditching familiar and hackneyed means of data-gathering totally out of kilter with state-of-the-art approaches to understanding language-practices, among which is the one that incorporates the concept of translanguaging. But, before everything else, it is worth asking ourselves just what translanguaging is all about. It is something that people in all multilingual societies have long lived with (even before they came under colonial rule), whether or not they were consciously aware of that. Changing linguistic horses in midstream may be one way of describing their routine communicative practices. The metaphor of braiding, I think, captures it better than the sociolinguist's 'code-switching.' But, to be sure, it involves a lot more than that. Because, the participants have recourse to all sorts of other semiotic resources available to them at the moment of communicative encounters. This was precisely what Bernstein (1957) was referring to when he pointed out that the children in his classic study who were saddled with 'restricted code' (originally, 'public language') employed a rather simplified linguistic system. They used deitics and relative pronouns less often, because they could jolly well make up for the absence of these through their deft use of gestures such as pointing to the object etc.

What I am insisting here is that translanguaging has existed ever since different languages came into contact. Actually, this may even turn out to be the wrong way of putting things. It may

well be the case that translanguaging is what there was at the very beginning. Somewhere along the line distinct languages were formed in tandem with the rise of nation-states and all the rest. The invention of writing systems may have contributed to prising man—the speaking animal *par excellence*—out of the semiotic milieu in which he was quite happy (like the ‘disadvantaged’ child in Bernstein’s study) to conduct his daily communicative activities.

But I honestly fail to see in what sense colonialism may have been at the root cause of the presence of translanguaging all over the world. Mind you, I am not saying that colonialism could not have obscured and obfuscated matters so as to make translanguaging invisible, marginal, nor worth bothering about etc. Quite the contrary. I believe there is sufficient literature on the topic that leaves no doubt as to how the colonial enterprise helped create the idea of pristine, monolithic languages, propped up by their monolingual native speakers.

But that is a different matter altogether. It does not at all go to show that translanguaging is an offspring of colonial brutality or whatever, nor that it is yet another of those unfortunate spinoffs that we can lay at the door of European colonialism. That said, I have no problems with Ndhlovu’s assertion that “the rise of the *translanguaging school of thought* is a welcome development not because it is a novelty. Rather it has to be seen as symptomatic of homecoming by academics and education practitioners.” (italics mine)

To reiterate my claim then, translanguaging as a phenomenon per se has nothing to do with colonialism; the fact that it took so long to attract scholarly attention may well have to do with colonialism’s eagerness to keep it out of sight by portraying individual languages as invested with discrete and uniform identities.

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Review of Omphile and his Soccer Ball

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In the paper ‘Omphile and his Soccer Ball: Colonialism, Methodology, Translanguaging Research’, Ndhlovu puts forward auto-ethnography as a praxis and a way forward to decolonize the more traditional and positivist methods often applied in language research. In so doing, he suggests that auto-ethnography is particularly well suited for practice-based approaches that challenge traditional notions of languages. He centres his argument on the study of translanguaging where he relies on Li Wei’s Moment analysis used in the study of Chinese Youth and their strategies in peer group communication in United Kingdom (Li Wei, 2011). Contrasting his understanding of translanguaging to those who see it as a novelty, he considers translanguaging to be a suitable notion for going back to basics, where notions and approaches converge with practices from below.

Sharing Ndhlovu’s epistemological and methodological stance, but with a dissimilar positioning as a researcher (woman, white, adult), and similarly, carrying out my research in the geographical South, my three comments foreground some perspectives I consider important for further deliberations on the epistemological and methodological advantages of auto ethnography put forward in Ndhlovu’s paper. The following three perspectives are, in my

view, not sufficiently problematized; i) Positionality and auto-reflexivity of the researcher's role in the interaction/ auto-ethnography ii) Metalinguistic commentary and Moment analysis for the analysis of translanguaging iii) Unintentional colonization by sidelining the ethical considerations.

I first discuss the positionality and auto-reflexivity of the role of the researcher. A methodological argument in favour of auto-ethnography, should be one that is robust towards different positionalities of the researcher. In this encounter, Ndhlovu, sits at a bench at the campus of the University of Witwatersrand in Johannesburg, South Africa. He represents a male, black, adult meeting a seven year old boy, that the reader, based on the greetings, and the used linguistic repertoire, understands is a black boy. Imagine my participation at the same conference, very possibly sharing similar frustrations over the lack of insights into day-to-day linguistic practices in the Southern African region, as I was being lectured on translanguaging, by someone who did not realize what it means.

The construction of a shared space of encounter between Ndhlovu and Omphile, would be very different if I were to sit on that bench. The initial greeting would have been different, as would the rest of the encounter. To avoid the construction of essentializing discourses around methodologies, I believe it is important to reflect these positionalities and a richer auto-reflexivity in the promotion and application of auto-ethnography. However valid for all interpretative research, I believe that further deliberations on the biographies and also the geographies of the researcher and their relation the biographies and geographies of those they encounter, are of particular importance to auto-ethnography.

The second comment is to the aptness of combining auto-ethnography

and Moment analysis for the analysis of translanguaging. Probing into the reported interaction between Ndhlovu and Omphile, the example clearly shows how Ndhlovu prompts the change of linguistic resources (languages) from the initial greeting in Setswana/Sepedi to the asking for Omphile's name in isiZulu. The remainder of the interactions continues in isiZulu with some commonly used elements of English. The reflexivity of the researcher in relation to his role in the interactions is paramount to demonstrating the advantages of auto-ethnography as a research method compared to more traditional methodologies and for making his case in relation to translanguaging. As an isolated example, the interactions between the two, and the used linguistic resources, could indeed play out very similarly, in an interaction as part of a more traditional ethnography. One difference, would be the possible changes to the interaction and linguistic choices due to the awareness of Omphile that he was being studied. The advantage of being able to dynamically display the deploying of linguistic resources can just as well be gained through some of the more traditional ethnographic methods mentioned, such as observation or researcher participation in a focus group discussion. Ndhlovu creates an unnecessary polarization towards a broad category of approaches and methodologies whereas a more targeted critique to pre-planned and pre-defined approaches and categorizations that reproduce epistemological geographies of exclusion, could be rhetorically less affective but have the advantage of being more pertinent. Additionally, an important part of Moment analysis that Li Wei applies in his study, is metalinguistic commentary. Auto-ethnography, in the applied approach reported in the paper, powerful as it may be, does not allow for such metalinguistic commentary of the interaction. To enable Moment analysis

a combined ethnography as suggested by Li Wei in United Kingdom and applied by Reite in Mozambique, provides a richer description (Li Wei, 2011; Reite, 2016).

This brings me to the last point, namely the ethical considerations. Adoption of auto-ethnography without providing a posterior information to the individuals we encounter can prejudices the ethical foundations of research. This is particularly relevant to the auto-ethnography involving minors, as in the provided example. In my perspective auto-ethnography, without further deliberations on the ethnical grounding of the practice, can indeed decolonize methodology but, in so doing, run the risk of unintentionally continuing to colonize (or at least abuse) the people that we as researchers encounter.

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Review of Omphile and his soccer ball: Colonialism, Methodology and Translanguaging Research

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In this paper, the author brings a decolonial lens to research methods in sociolinguistics. He argues for autoethnography as an approach to capture the moment-by-moment translanguaging that takes place in everyday multilingual

interactions. These are driven, he argues, not by the need to establish a common language(s) of communication, but by the fundamental need to interact. Based on a chance encounter with a seven-year old boy, Omphile, with whom he shares an impromptu soccer game, he illustrates how such a 'chance meeting', and his reflections on this event, provides a living example of naturalistic translanguaging-in-action. The author further argues that this kind of unplanned, spontaneous, personal reflection (autoethnography) – offers a decolonial approach to research, which enables a 'delinking' from positivist Western modes which, he argues, have dominated sociolinguistics. In the category of the latter, he includes ethnography, focus interviews, participant observation. Thus, even while theorists of translanguaging have challenged the colonial conception of languages as bounded objects, these same theorists, he argues, have failed to bring a similarly critical eye to the methodology they employ in their research.

The key questions that the paper asks are critical ones, particularly as we grapple with the challenges raised by decolonial theory. He asks: "How realistic is it for new philosophies of language to claim they are pushing scholarship forward in a new direction when their theoretical suppositions are supported by data generated through conventional research methods? How do we do ethnographic social science research in ways that allow us to capture the complex relations between society and communication resources? In other words, can we really claim to be theorising in unconventional ways when our methodologies remain conventional?" After all, all knowledge – and by extension – research methods and approaches, are shaped by the context in which they are produced. But, as Mignolo also argues, this 'situatedness' is often concealed by the fiction of the 'detached observer'

whose assumed neutrality serves to hide the extent to which he or she ‘controls the disciplinary rules and puts himself or herself in a privileged position to evaluate and dictate’ the conversation (Mignolo 2009: 4).

The author’s own response to these questions is to advocate for autoethnography, on the basis that it places “greater emphasis on the ways in which the ethnographer interacts with the culture being researched” and “helps us understand how the kinds of people we claim, or are perceived to be, influence interpretations of what we study, how we study it and what we say about our topic” (citing Holt 2003 and Ellis, Adams and Bochner 2011). In other words, it puts the spotlight on self-reflexivity. While I find this rationale very interesting and compelling, it is at this point that I believe some nuance and reflection is required:

First of all, the author sets up a fairly rigid categories or boundaries between research methods: those which rely on ‘conventional research methodologies that are limited to controlled scientific experiments: oral interviews, survey, focus groups, participant observations, and so on’ vs. the ‘anti-foundational’ approach of autoethnography. He then makes somewhat sweeping claims for autoethnography, but does not explain how these can be achieved, and how these might be different to studies undertaken within a critical ethnographic paradigm e.g. “Autoethnography does not subscribe to the procedures and processes of conventional approaches. It is a totally different methodology that seeks to inaugurate ‘an-other logic’, ‘an-other language’ and ‘an-other way’ of doing research that has the potential to liberate social science research from the clutches of hegemonic conventionalism”. Furthermore, he goes on to say that “The conceptual and methodological premises of autoethnography enable social scientists to ask big questions of

small data (Salazar, Elliot & Norum 2017), which clearly sets them apart from the conventional scientific method that is largely pre-occupied with big data”. I would contest that all ethnographic studies are primarily pre-occupied with big data. In fact, researchers have used ethnographic approaches and methods in a wide variety of ways, some of which would clearly be reflexive and multi-voiced (see De Korne and Hornberger 2017, or Kerfoot 2016 for two examples), keenly aware of and sensitive to the ways in which their interpretations impact on and shape the narratives about and power relations in the sites in which they are working. So, what really is the place of autoethnography in relation to the variety and scope of ethnographic approaches generally?

Secondly, If autoethnography relies on the spontaneous reflections of the researcher, how are conversations, such as the one reported on with Omphile ‘recorded’ or ‘captured’ for reflection. After all, it is well established in narrative research that the ‘conversations’ people recount are almost never ‘word for word’ records – they are recast in particular ways by the narrators (researchers’) own memory/subjectivity. In cases like this paper, where the focus is on the very blending and mixing of languages, how can you be sure that you recorded the conversation accurately, if you have no recording to go back to?

Thirdly, and this is perhaps, the issue that concerned me most of all, was ‘what about informed consent’, especially when dealing with children. How do you acquire ‘consent’ when the interactions are chance and unplanned? More specifically, If autoethnography is to be seen as a decolonial approach, how does it address the relations of power inherent in the research context.

To conclude, I would like to stress that I enjoyed reading this paper very much, and I found the historical overview of translanguaging and the illustration

of the researcher's interaction with Omphile very useful. I also think that the questions Ndhlovu asks about research methodologies particularly in the light of decolonial theory are critical. But I was not so convinced by the presentation of autoethnography as 'the answer' to the problems outlined with more 'conventional' sociolinguistic approaches, and I think he 'paints the picture' with rather broad brush strokes. Perhaps if we are thinking about decolonial approaches to research methodology, we should be putting our focus less on 'which method' but rather on 'how' that method is used and how the researcher engages with the 'researched'. In other words, the focus needs to be more on the ethics of research, and the values and principles that underpin it. The author hints at this on page 7 when he calls for "humility, empathy and accommodation" when

researching translanguaging and the "centrality of humility, empathy and willingness to come down to the level of our interlocutors in establishing the common ground needed for effective communication to take place" (page 13). However, I remain unconvinced that he has succeeded in doing this in his paper

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Can the Other be Heard? Response to Commentaries on ‘Omphile and his Soccer Ball’

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Introduction

I welcome the invitation to a right of reply that *Multilingual Margins* journal has extended to me; and I thank all nine discussants for sharing their thoughts on my paper ‘Omphile and his soccer ball: Colonialism, methodology, translanguaging research’. Eight of the nine discussants (Kathleen Heugh, Alan Carneiro, Manuel Guissemo, Kanavillil Rajagopalan, Zannie Bock, Lynn Mario T. Mendezes de Sousa, Nana Aba Appiah Amfo, and Torun Reite) provided what I consider to be balanced critiques that highlight both the strengths and weaknesses of the paper. One reader, Don Kulick, did not find anything positive about the paper. Instead, he raised numerable objections that are pitched in a somewhat confrontational tone that is radically at odds with the views proffered by all other discussants. For this reason, I decided to organise my response into three short sections. The first is a rejoinder that builds on and engages those critical points raised by the eight discussants who are overall in concert with each other. In the second

section I provide a rebuttal of Don Kulick’s review, which I find to be largely dismissive and bereft of any semblance of collegial engagement with the arguments advanced in the paper. I then close with a short paragraph that reiterates my original invitation to engage in dialectical conversations about how best to carry out social science research projects in ways that are consistent with the quite contemporary anti-colonial, anti-foundational and transformative agenda being pushed by decolonial and other like-minded scholars.

Rejoinder – response to eight discussants

In addition to affirming the motivations and lines of argument I advance, nearly all eight reviewers noted some limitations – of one form or another – which is to be expected. Most of the points raised are comments that further clarify some of my propositions that had not been fully explicated; for which I am grateful. In my reply, I, therefore, focus on four crucial points that feature

prominently in the reviews. The first one is about the idea that there is ‘translanguaging theory’. This concern was raised by Kathleen Heugh and also picked up by two or three other readers – though expressed differently. Heugh says “I am not convinced that many scholars agree that there is, or should be, a definitive or stable pedagogy or theory of translanguaging. So, it worries me that Ndlovu indicates that there is a ‘translanguaging theory’” (Heugh, this issue). I take Heugh’s point and agree with her reservations. The impression that there is a uniform body of thought that constitutes a ‘translanguaging theory’ is clearly a consequence of slippages on my part. Looking at the paper again in the light of this comment, I can now see the dangers of assuming that a translanguaging theory does exist. As Heugh (this issue) cautions, the idea of a translanguaging theory “would take us back to assumptions of the universality of knowledge and reason ... re-scripting neo-colonial habitus and hegemonic supremacy of colonial thinking”. This is precisely what my paper sought to question. When I was on a fellowship at the Graduate Center, City University of New York Graduate in the fall of 2017, I had numerous conversations with Ofelia García about the pitfalls of reinforcing the very same Euro-modernist hegemonic thinking that we are seeking to avoid. Though she is one of the key proponents of translanguaging, Ofelia García consistently admitted that the challenge we face is how to chart new alternative paths using a language that allows us to de-link from the colonial matrices of power that are firmly ensconced in the body-politic of the academy. It is here that Heugh’s clever notion of ‘trickster habitus’ comes in handy. A careful rethink of the terminology we use to characterise translanguaging and other allied approaches is required as a way to avoid obscuring debates.

The second point is about what Kanavillil Rajagopalan perceives as lack of “any proof of a direct link between colonialism and the phenomenon of translanguaging” (Rajagopalan, this issue). I would like to clarify that I do not believe that there is a direct link between translanguaging and colonialism. It seems Rajagopalan may have misunderstood what I meant in my discussion of translanguaging in relation to colonialism. The point I was trying to put across is this: though proponents of translanguaging are driven by the anti-colonial agenda, the empirical data that supports their theoretical suppositions seems to continue being generated through the conventional scientific method, which is a legacy of colonial modernity. I, however, do agree that this particular aspect of the discussion could have been expressed much better in order to avoid giving an impression of causal link between translanguaging and colonialism. I would like to thank Rajagopalan for bringing greater clarity into the discussion. In particular, he raises a significant point in saying “It may well be the case that translanguaging is what there was at the very beginning. Somewhere along the line distinct languages were formed in tandem with the rise of nation-states and all” (Rajagopalan, this issue). Often, when sociolinguists talk about the co-construction of languages and nation-states they trace the argument to Benedict Anderson’s (1991) *Imagined Communities*. But I think Rajagopalan’s review is pushing the debate further, towards a bold, combative and more productive direction inviting us to probe the geo- and body-politics of knowledge that is hidden beneath the self-serving interests of Western epistemology (Mignolo, 2009: 4).

The third and most significant line of enquiry that I had completely overlooked in the paper is the question of research

ethics and informed consent. Three commentators – Zannie Bock, Nana Aba Appiah Amfo and Torun Reite – weighed into this glaring omission in the paper. As a member of the Human Research Ethics Committee of my university, I should have had this aspect of research at the forefront of my thinking while writing the paper and I thank all three readers for drawing my attention to it. As Amfo (this issue) rightly points out in relation to the autoethnographic methodology that I advance in the paper, “the glaring challenges of this approach, particularly those bothering on ethics have to be confronted and addressed rather than muted”. Reite draws our attention to the problem of “unintentionally continuing to colonize (or at least abuse) the people that we as researchers encounter”. These are, indeed, very real unintended consequences of relying on autoethnographic methodology, which require our serious consideration. For this reason, I wish to extend an open invitation to the scholarly community involved in this type of work, for robust ongoing conversations on how best to go about with decolonial research in a manner that puts ethics and informed consent at the centre. I think there are at least two crucial questions that we need to exercise our minds on: How are matters of research ethics consideration to be framed and articulated in decolonial projects? And how do current institutional and national policies on research ethics impinge on the discourse and praxis of decolonial epistemological research? I believe addressing these questions is germane because current understandings of research ethics (at institutional, national or international levels) are indexically linked to the apparatus of colonial normative scientific methods that we are seeking to unsettle. In some of my previous projects, particularly with indigenous, migrant and refugee communities, I have had

prospective research participants decline an invitation to participate as soon as I tell them there is a consent form to be signed. But this is part of the national guidelines and local institutional requirements on conducting research with humans in an ethically acceptable way. Institutional Human Research Ethics Committees would insist on evidence of informed consent, often in the form of a signed consent form. How do we reconcile this? Is there a possibility for decolonising research ethics such that it also incorporates indigenous participants’ understandings of ‘informed consent’? Or, alternatively: Is a decolonial epistemology of research ethics possible, and what might it look like?

The fourth and final point I would like to address in this section is about gender discourse and positionality of the researcher, which appears in Torun Reite’s review. Though it had not crossed my mind at the time of writing the paper, I think Reite is correct in saying the encounter with Omphile and the interaction that followed would have played out differently if the researcher was white, female and possibly not a speaker of the linguistic codes that feature in the story. This is a fair and welcome intervention that, in my view, has potential to inform the ways we deploy autoethnography. As Reite argues it is important to reflect the positionalities of gender, race/ethnicity, language abilities and context. I doing so, we can open a window for entering into “further deliberations on the biographies and also the geographies of the researcher and their relation to the biographies and geographies of those they encounter” (Reite, this issue).

In closing this section, I wish to reiterate that the goal of my paper was to invite scholars working in this area of research to join the conversation I initiated around the troubling question

on the reification of conventional Euro-modernist epistemologies that continue to be treated as if they were of a natural kind; the only valid and legitimate ways of reading and interpreting the world. The eight commentators provided several other useful insights that I would have wanted to build on and extend further but time and space do not permit. I now turn to my reply to Don Kulick's review.

Rebuttal – Reply to Don Kulick

I would like to open this section by taking the reader back to the motivations of my paper. I wrote the paper as an invitation to the social-scientific community to take stock of and reflect on the common sense assumptions of conventional scientific methods that guide the way we do research. For this reason, I was not seeking consensus; neither did I expect all readers to be sympathetic to my line of argument. I would have been surprised if this were to be the case because I did not intend to be doctrinaire in my propositions – though I am delighted that eight out of the nine commentators concurred with the overall thrust of my thesis. The point of greater significance here is that this paper speaks from a very specific locus of enunciation; that of decolonial epistemology. The argument I advance joins the long list of pioneering international social science theorists from the Global South who argue in support of the promises that an ecologies of knowledge paradigm holds for articulating the possibilities of epistemological pluralism (de Sousa Santos, 2007; Mignolo, 2002; Kovach, 2009; Bagele, 2012 and Smith, 2009). This is against the backdrop of the dominance of Euro-modernist epistemologies that make general claims to universal relevance while turning a blind eye to the fact they are only a part

of a diverse global system of knowledges. This ideological habit that universalises dominant epistemologies overlooks two important points: that throughout the world there are very diverse forms of knowledge; and that there are many and very diverse concepts of what counts as valid and legitimate knowledge (de Sousa Santos, 2007).

From the onset, I made it clear that:

The style of presentation I use departs slightly from conventional academic narrative techniques in that it does not have the usual elements of a research essay such as research methods and procedures, research design, sampling techniques, and so on. This is because the article is a reflective piece that reports on a random unplanned observation of naturally-occurring communicative practices (Ndlovu, this issue, page 3).

Therefore, evaluating the paper through the lens of Euro-modernist epistemology (as Don Kulick has done) misses the whole point of the argument advanced. To the extent that there was anything to learn from Kulick's review, it was all obscured by the acerbic tone of his report, which is quite unfortunate. Kulick opens his review with a tirade and follows through with series of negative hyperbolic expressions about the paper and myself. I am not going to dignify these with a response except to make the following four points. First, seeking to ridicule and diminish the ontological density of scholars whose views we have misread, at worst – or we disagree with, at best – is a futile exercise that does not help advance the science. Kulick's review betrays an angry man who has been personally offended by a paper that not only challenges but also refuses to toe the line of Euro-modernist value judgements about what constitutes valid

and legitimate forms of research. As a senior professor, I suppose Kulick knows, as well as most of us do, that the contest of ideas has always been and still remains the hallmark that defines the academic enterprise – and that disagreement has to be done in a manner that recognises the fact that there are multiple ways of reading and interpreting the world. For this reason, I believe Kulick could have done well to defend the legacy of Euro-modernist epistemologies without necessarily sounding angry and personally offended by the position I advance in the paper.

Second, Kulick introduces his commentary by characterising the tone of my paper as “combative”. I agree; though I contest the claim that his response is couched in an equally combative tone (he sounds like an angry and offended man – I explain this under point number four below). Decolonial theorists like myself accept the “combative” label as a badge of honour because we are deliberately and necessarily engaged in ‘epistemic disobedience’ (Mignolo, 2009) – that counterhegemonic struggle which calls for epistemological pluralism. Part of our task is to engage with the historical debates surrounding the colonial origins of mainstream scientific methods in the context of the Global South. In a 2017 article aptly titled ‘Decolonising Research Methodology Must Include Undoing its Dirty History’, Sabelo Ndlovu-Gatsheni provides a lucid account of the history of conventional Euro-modernist methodologies; how they emerged as handmaidens of colonialism and imperialism; as well as how, in the end ‘research’ became a critical part of the imperial colonial project. Ndlovu-Gatsheni (2017: 3) goes further and argues:

Our present crisis is that we continued to use re-search methods that are not fundamentally different from before. The critique of methodology

is interpreted as being anti-research itself ... Consequently, methodology has become straightjacket that every researcher has to wear if they are to discover knowledge. This blocks all attempts to know differently. It has become a disciplinary tool that makes it difficult for new knowledge to be discovered and generated.

Scholars who try to exercise epistemic disobedience (like I did in my paper) are disciplined into an existing methodology, thus draining them of their profundity. There is no better way to describe the intentions of Kulick’s review than what Ndlovu-Gatsheni says in the above quotation. But it is about time defenders of Euro-modernist epistemological hegemony got used to the fact that it is no longer business as usual. They better get used to not only listening to themselves and start listening to other voices, especially those articulating opposing and ‘unfamiliar’ views from the Global South. Decolonial and Southern theorists are seized with the task of unmasking the role and purpose of research; they are shifting the phenomenology of research in order to “re-position those who have been objects of research into questioners, critics, theorists, knowers, and communicators” (Ndlovu-Gatsheni, 2017: 4). Scholars of Southern and Decolonial persuasions have voices too and will not allow their voices to be silenced any longer – all for challenging Euro-modernist epistemologies and their misdirected claims to universal relevance. At the heart of it all is the fact that forms of knowledge that are produced following only one tradition of knowing are partial, biased and, to a large degree, incomplete. This is because no single type of knowledge – on its own – can account for all possible interventions in the world (de Sousa Santos, 2007). Dani Wadada Nabudere (2011: 1) could not have put

it any better when he said mainstream Euro-American scientific knowledge and theorisation alone is unable to explain everything about the world because there is “great deal of uncertainty in the way we understand the world, as well as in the way human beings understand each other in different environments and cultural contexts”. Writing nearly one and half decades ago, Immanuel Wallerstein reminded us that:

[W]e live in a very exciting era in the world of knowledge, precisely because we are living in a systemic crisis that is forcing us to reopen the basic epistemological questions and look to structural reorganizations of the world of knowledge. It is uncertain whether we shall rise adequately to the intellectual challenge, but it is there for us to address (Wallerstein, 2004: 38).

The conventional scientific tradition that Kulick seeks to defend is not immune from the changing dynamic in the global knowledge economy. In this vein, I wish to reiterate the cross-cutting argument of my paper. My goal is to invite the progressive scholarly community to engage in conversations that probe the universalisation of Western thought that imposes normative criteria and standards for evidence, validity, coherence and intelligibility in knowledge production and dissemination (Buendia, 2003; de Sousa Santos, 2002).

Third, in his dismissal of the story that motivated me to write the paper and the prognosis I proffer for methodological innovations, Kulick says (i) that the seven-line recollection of my interaction with Omphile is way too small and insignificant for us to read anything meaningful out of it; and (ii) “Why should we trust that the author’s memory of this unexpected interaction was not only accurate, but *exact*? (Kulick, this issue; emphasis in the original).

Both points may hold sway, but only to a limited extent because all personal stories rely on memory and recollection. So, this is not something unique to the story that undergirds my paper. Kulick then uses this as an opportunity to dismiss the way I deploy the insights of autoethnography. He argues that a better way to illustrate the potential benefits of autoethnography would have been for me to examine “memoirs that foreground, precisely, speakers’ reflections on their life in language: Eva Hoffman’s *Lost in Translation*, Alice Kaplan’s *French Lessons*, and Vladimir Nobokov’s *Speak, Memory...*” (Kulick, this issue). There is no doubt that these memoirs are valuable resources. But the authors of these memoirs also relied on their memories and recollections – just like I did in telling the story of my encounter with Omphile – unless of course Kulick wants us to believe that these people had pre-recorded their entire life stories, with prior knowledge that such information would come handy someday when they write their memoirs. I, therefore, have great reservations with what I see as Kulick’s privileging of the written word (so-called literacy) over orality as an authentic and reliable source of information. This is consistent with Euro-modernist habits and practices that I critique in the paper. The point I am arguing here did not escape the attention of Nana Aba Appiah Amfo who had this to say:

Autoethnography which allows an engagement of the readers in a personal narrative from the author is in sync with oral narrative traditions of many African cultures. Oral story-telling traditions have for generations been an authentic avenue to pass knowledge down across generations. The narrative power and feature of autoethnography is reminiscent of the time-tested method through which members of many African

communities make sense of their world, share knowledge and teach important aspects of their cultures” (Amfo, this issue).

Memory and recollection are the centre-piece of story-telling; and admittedly, orality does have its own limitations, but so does the written word, which is open to author subjectivity and bias. Both are not infallible and should not be immune from criticism. So, in my view, both oral and written stories (regardless of how long or short they are) should be evaluated on their own merits by taking into account issues such as cultural context and purpose of story-telling.

As I have already indicated above, another aspect of Kulick’s dismissal of the story of my interaction with Omphile is that the data is too small, which implies he is in favour of big data. I do not have any major qualms about this except to say that though big data is useful for some ends, it does not help us answer all questions in every context, particularly questions that relate to issues occurring at the micro-social levels of society. Small datasets such as the story I narrate in the paper encourage us to be specifically attentive of the small details of everyday life that may contain the potential to develop or question big theories (Strathern, 2004: xx). This is about the spheres of possibilities presented by asking big questions of small data. As Amfo (this issue) concurs, the arguments I advanced in the paper provide “an example of how small data can be useful window through which we can understand our varied worlds.”

The fourth aspect of my rebuttal is this. I would suggest that there is in Kulick’s response evidence of what postcolonial critics characterise as internal contradictions of colonialism or the colonial mindset (Sur, 2005; Phillip, 2004; and Radhakrishna,

2000). The typical *modus operandi* of colonial habits and practices is that they fundamentally proceed through affirmation and denial – in equal measure. Kulick affirms the importance of paying attention to the smallest detail, no matter how insignificant it may seem. But in the same breath, he denies the potential transformative power of such minute detail, especially in relation to theory building by Southern scholars like myself. It appears that in his defence of the conventional Euro-modernist tradition of research Kulick wants scholars from the Global South (and their communities) to be content with being suppliers of raw data (as evidenced by his lifelong anthropological work among Southern communities) and not producers of new and alternative theoretical frameworks. This is a classic contradiction of the highest order – and yet not quite surprising because it reflects the colonial habitus of which it is a part.

But scholars speaking and writing from the Global South have had enough of being told that they do not have the right to theorise and we refuse to take such condescending habits and practices lying down. After more than 500 years of Euro-North American epistemological domination, we shall not allow ourselves to continue being bullied and humiliated into silence and submission. We are determined to defend our inalienable right to epistemic freedom.

Conclusion

I conclude my reply by reminding the reader of the original inspirations of the paper, which Kathleen Heugh captures a lot better than I had probably done:

Ndhlovu’s paper is an invitation. It is an invitation to engage in dialectical conversations, such as his conversation with Omphile. It is through such conversations that we may find opportunities to engage

in critical reflexivity about our own epistemologies, systems of beliefs and ways of being, and how these influence our view of the world and how we try to disentangle webs of deceit spun by the trickster habitus of coloniality. Omphile offers us a metaphor for disentanglement (Heugh, this issue).

This call did not escape the attention of all commentators, but one.

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Family Language Policy ten years on: A critical approach to family multilingualism

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Family language policy (FLP) has been establishing itself as a field in the past decade. Yet, much of the scholarly debate around family multilingualism has remained within the boundaries imposed by Western-centric epistemologies. In order to address this issue, this article reviews FLP studies published between 2008 and 2017, and discusses accomplishments and limitations of recent publications. The main argument presented here is that a critical approach to family multilingualism might contribute to the development of FLP in an unexplored direction. More specifically, this paper shows how drawing on a decolonial approach allows for an express engagement with debates that have only been marginally tapped into in current FLP scholarship, for instance, the intersectional dimension of social categorisations such as social class, race, and gender. Furthermore, a decolonial approach provides a robust frame to examine transnational practices by reconciling perspectives that tend to privilege either the material basis of the economic relations of production, or the cultural domain as a locus where these relations gain meaning. Finally, a decolonial approach to family multilingualism takes a step towards redressing the extant underrepresentation of southern theories in sociolinguistics.

Keywords: family language policy, critical family multilingualism, decolonial approach, Southern perspective

Introduction

This article sets out to examine the development of Family Language Policy (FLP) as a field of study in the past ten years. This is done in light of recent debates in the field of Language Policy and Planning (LPP), and in multilingualism research, aiming at discussing the accomplishments and limitations of FLP,

and pointing to possible directions for future research. I limit my coverage of research to between the years 2008 and 2017 for two reasons. First, the definition of FLP by King, Fogle and Logan-Terry (2008) was an important turning point in the development of the field, allowing researchers who have a shared interest in language use in the home to construct a common site for promoting

scholarly debate. Second, while more comprehensive overviews have already been published (e.g. Curdt-Christiansen 2013; Curdt-Christiansen and Lanza 2018; King 2016; King and Lanza 2017; King and Fogle 2013; King et al. 2008; Schwartz 2010; Smith-Christmas 2017; Spolsky 2012), a closer look at the shifts taking place within FLP, and in LPP and multilingualism research, permits the recognition of certain trends and limitations of FLP research done within the proposed timeframe that these overviews did not capture.

The main argument put forward here is that there are a number of issues in the field of FLP that are insufficiently accounted for in the current research frameworks that are the subject of review in this paper. Among such issues are the increasing *transnational*, specifically Southern, families that pose particular questions and reveal specific faultlines in much existing work. Addressing these issues appropriately in a better theoretical framework, I argue, would help to develop the field even further.

This article is structured in the following manner. I first give a brief orientation to some of the questions raised by transnational families for multilingual socialisation. This is followed by an overview of some key FLP studies published between 2008 and 2017. In conjunction with this overview, I note how many of the studies to a larger or smaller extent build on Spolsky's (2004, 2007, 2009, 2012) seminal work in LPP. I suggest that Spolsky's model is insufficient to account for the sorts of questions we need to ask of transnational families, and I offer the sketch of an alternative approach built around decolonial thinking. Finally, I propose that the engagement with the aforementioned developments, and with a decolonial approach to family multilingualism more generally, might promote the development of FLP in hitherto unexplored directions.

Some issues in family language policies of transnational families

While recent studies on family multilingualism have pushed the field of FLP in interesting and innovative directions, it is noteworthy that the theoretical frameworks with which researchers have worked remain within what could be understood as Western-centric, canonic epistemologies. The notion of *abyssal thinking* put forth by Santos (2007) helps us to problematise this disjunction. Santos (2007) describes Modern Western thinking as *abyssal thinking*, that is, one which divides social realities in two realms: "this side of the line" and "the other side of the line" (p.45). Whatever is on this side of the line, he argues, results from the way modern Western thinking has forged social reality and, thus, reaches an ontological status not only as valid, but the *only* valid way of conceptualising social reality. Whatever is on the other side of the line is deliberately rendered invisible and, thus, not recognised as valid forms of living, thinking and producing knowledge. An overarching claim of this article is that much of FLP research to this date has been done "on this side of the line". That is, the predominance of canonic epistemologies in FLP (e.g. the pervasiveness of Spolsky's tripartite framework in which language policy is understood to be composed of language practices, language beliefs, and language management) obscures the lived experiences of people and theories from the global South. Following Santos (2014, 2018), the global South here is understood not only as the geographical South, whose populations have most been negatively impacted by the domination from the global North, but also pockets in the global North where certain populations have to struggle against oppressions and injustices.

Shifting the focus of FLP in order to embrace the particular struggles of people from the global South as well as incorporating into the FLP theoretical apparatus concepts and theories stemming from this geopolitical location of knowledge production would allow us to answer questions that have not been asked in FLP studies, or examine those that have been asked but through a different perspective. For example, Veronelli (2015) proposes the notion of decoloniality of language to explore the connections between language, communication and coloniality. Particularly, investigating the linguistic dimension of the consequences of coloniality, Veronelli (2015) argues that the hierarchisation of races/ethnicities, constitutive of and emerging from coloniality/modernity (Quijano 1989), is accompanied by the idea that the means of expression employed by different peoples can also be ranked following a superior-inferior continuum.

Bringing this discussion under the scope of FLP research can open up analytical possibilities yet to be explored. Smith-Christmas (2016, 2017) proposes three prototypical contexts that have characterised research on family multilingualism: OPOL (one person, one language), immigrant community, and autochthonous community. She raises some issues for better understanding the language practices of multilingual families according to different contexts, for example, the relevance of the notion of social class to investigate OPOL practices (e.g. many families that have employed this “strategy” have been classified as middle class), or the stigma attached to the language practices of families in immigrant or autochthonous communities. I suggest that a critical approach to family multilingualism may provide a more robust theoretical framework to anchor social categorisations (such as class), as

well as shed light on the nuances that differentiate migratory trajectories (i.e. South-South, South-North, North-South, North-North). Such an approach could (i) help to unpack the discursive reproduction of the hierarchisation of race/ethnicity, gender, and social class in intercultural encounters of parents from the global South living in the global North (Lomeu Gomes, forthcoming); (ii) tap into the affective dimensions of the embodied experiences of being othered as people make sense of themselves as belonging to/constructing multilingual families; and (iii) challenge canonic understandings of central concepts such as “family”, “language” and “policy” that are recurrently taken for granted. In the next section, I demonstrate that current FLP research has focussed on issues other than these. Then, I suggest what a critical approach to family multilingualism could look like. In the conclusion, I sum up the main points introduced in this article.

Family Language Policy ten years on

Re(de)fining FLP

In the past ten years, the field known as family language policy (FLP) has gained momentum, arguably due to FLP being formally defined in 2008. According to King et al. (2008: 907) FLP can be defined ‘as explicit (Shohamy 2006) and overt (Schiffman 1996) planning in relation to language use within the home among family members.’

Fogle (2013: 83) has expanded this definition claiming that the decisions parents make about language use in the home are not necessarily overt and explicit, and including language learning as well as literacy practices: ‘Family language policy refers to explicit and overt decisions parents make about

language use and language learning as well as implicit processes that legitimize certain language and literacy practices over others in the home.'

In line with more recent understandings of FLP in general, the implicit and covert dimension of language policy within the home had already been stressed by Curdt-Christiansen (2009: 352) who went further to include literacy practices in her definition: 'family language policy (FLP) can be defined as a deliberate attempt at practicing a particular language use pattern and particular literacy practices within home domains and among family members.'

These redefinitions attest to the dynamicity of FLP as a field of study continuously developing to encompass more nuanced understandings of the factors and processes at different levels of analysis related to language practices in the home. While these empirical developments have promoted greater awareness about certain issues and contexts that had been overlooked in the past, the epistemological and ontological horizons of FLP research have not changed much, which motivates a closer analysis of the directions in which the field has been going as well as the paths yet to be taken.

Scope of this overview

In order to define the works to be reviewed, the publications (i.e. original research papers, introduction of thematic issues, commentaries, editorials, published monographs, edited volumes, and book chapters) had to: (a) contain the phrase family language policy/ies either in the title or as keywords in the abstract; (b) have been published between January 2008 and December 2017.

The methodological rigour evinced by the criteria above is not to be confounded with a nod towards epistemological universalism. Whilst

the latter assumes that the ultimate goal of any scientific endeavour is to produce objective knowledge following positivist methods and relying on tenets such as neutrality, validity, reliability, generalisability, and reproducibility, establishing strict selection criteria for the material to be reviewed does not exempt the author from recognising that the review below is one of the many possible ways of interpreting the development of FLP as a field. Further, it should be highlighted that the inclusion and exclusion criteria proved to be a limitation because some works that are relevant for the investigation of multilingualism in the home had to be disregarded, especially works that situate themselves within 'language socialization' (e.g. Duff and May 2017; Duranti et al. 2012; Fogle 2012; He 2016), 'language revitalization' (e.g. Hinton 2013), and 'language shift and maintenance' (e.g. Bloch and Hirsch 2017; Gafaranga 2011; Kim and Starks 2010; Lane 2010). Yet another patent limitation is the focus on publications in English.

Inasmuch as these observations may sound as methodological truisms, the critical approach proposed in this article, in particular the alignment with a decolonial approach (Castro-Gómez and Grosfoguel 2007; Mignolo 2011b), motivates the explicit discussion about promoting epistemic diversity (de Souza 2014) and challenging current geopolitics of knowledge (Levon 2017). Furthermore, while postmodern and poststructural critiques also challenge the neutrality of knowledge production and promote a greater involvement with methodological and epistemological reflexivity, and researcher positionality, a decolonial approach takes yet another step and envisages the need to redress the extant erasure of voices from the global South from current sociolinguistic debates (Milani and Lazar 2017) by

deliberately bringing to the fore such perspectives, be it by focussing on the particular struggles of peoples from the global South, or by drawing on theory developed in Southern contexts.

Overview of FLP literature between 2008 and 2017

In the last decade, scholars have published comprehensive overviews of the field, thematic issue introductions, and editorials, covering a wide chronological range, epistemological and methodological shifts, and remarked its empirical development (e.g. Curdt-Christiansen 2013; Curdt-Christiansen and Lanza 2018; King 2016; Li Wei 2012; Spolsky 2012; King and Lanza 2017; King et al. 2008; King and Wright 2013; Schwartz 2010; Smith-Christmas 2017).

The interweaving of overviews of FLP with my own analysis of publications in the past ten years allows for an understanding of development of the field in a somewhat cohesive fashion, mainly considering three trends: (i) the pervasiveness of Spolsky's framework; (ii) the gain of currency of ethnographic methods; and (iii) the diversity of languages, geographical locations, family configurations. I now turn to a more in-depth discussion of each of these trends.

Spolsky's framework

King et al. (2008) conceive of FLP as an emerging field that brings together the fields of language policy and child language acquisition. The authors discuss how the field of language policy has shifted its initial concerns with solving the language problems of newly independent nations to trying to understand the dynamicity of the (social, cultural, and ideological) systems of which language policies are a part. It is within this understanding of the development of language policy that King et al. (2008)

introduce Spolsky's (2004) framework, which envisages language policy being made of three components: language practices, language beliefs, and language management.

Likewise, Schwartz (2010: 172) suggests that 'research on family language policy (FLP) incorporates analysis of language ideology, practice and management, which were classified by Spolsky (2004) as components of the language policy model with respect to the speech community.' It is noticeable that this definition, based solely on Spolsky's (2004) model, does not include ways in which Spolsky (2007; 2009) himself further developed his theory, nor acknowledges that this model is historically situated in the development of LPP (for overviews of LPP, see Hult and Johnson 2015; Johnson and Ricento 2013; Ricento, 2000).

The restriction to an understanding of language policy based on Spolsky's framework is reinforced by Spolsky himself (Spolsky 2012) and echoed by Curdt-Christiansen (2013: 2) as she maintains that 'FLP seeks to gain insights into the language ideologies of family members (what family members believe about language), language practices (what they do with language), and language management (what efforts they make to maintain language)'.

More recent studies continue to employ Spolsky's model without critically engaging with its epistemological and ontological assumptions. For instance, Oriyama (2016) investigated how Japanese heritage youths in Australia kept contact with the Japanese language after they stopped attending heritage language schools. Besides being one of the few studies that offer a 'long-term longitudinal' (Smith-Christmas 2017: 21) perspective, another important contribution of Oriyama's (2016) study is the theoretical discussion she presents about how family, as a unit of analysis,

can be conceptualised as a community of practice (Wenger 1998), a point first introduced by Lanza (2007). When it comes to her understanding of FLP, however, she echoes Schwartz (2010), Spolsky (2012) and Curdt-Christiansen (2013) and claims that ‘FLP consists of “language ideology” (a set of beliefs in and attitudes toward a given language), “language practices” (how language is used and learned), and “language management” (specific and conscious efforts to modify and control language practices)’ (Oriyama, 2016: 290).

A similar view of FLP is employed by Kang (2015) in her large-scale study involving 460 Korean parents living in the United States with their children under 18 years of age, where she attempts to develop a model to predict language maintenance in the home. Kang used an online questionnaire to collect data about participants’ background information, language practice, language management and language ideology, as well as accounts of parents on their children’s skills in Korean. Supporting her claims on the results of inferential statistics tests, Kang (2015) discussed the inconsistencies found between parental (positive) attitudes towards maintenance of Korean in the home and language practice and language management.

A number of other studies employ Spolsky’s (2004; 2007) tripartite framework (e.g. Altman et al. 2014; Bezioglu-Goktolga and Yagmur 2017; Chatzidaki and Maligkoudi 2013; Dumanig et al. 2013; Kaveh 2017; Kayam and Hirsch 2014; Kopeliovich 2010; Nakamura 2016; Parada 2013; Patrick et al. 2013; Pillai et al. 2014; Revis 2016; Schwartz 2008; Schwartz and Verschik 2013; Stavans 2015; Xiaomei 2017; Yu 2016) with little effort directed to evaluating the framework itself or proposing reformulations.

One of the few exceptions is Ren and Hu’s (2013) attempt to improve Spolsky’s

model by combining its use with notions emerging from family literacy research (i.e. prolepsis, syncretism, and synergy). In another example, Tannenbaum (2012) advocates for a focus on the emotional aspects of family language policy. She proposes looking at family language policy as a defence or coping mechanism and, in doing so, she suggests that FLP research has underexplored the contributions from psychology and psychoanalysis. Tannenbaum and Yitzhaki (2016) take a step towards addressing this limitation by examining the connections between emotions and language practices of multilingual families. Additionally, Berardi-Wiltshire (2017) suggests that research on indigenous language revitalisation might benefit from drawing on Spolsky’s tripartite framework as employed by FLP literature. Finally, Fogle (2013) supports the idea of expanding the ideological component of FLP to include parental beliefs not only about language, but also about ‘family, childhood and caregiving’ (Fogle, 2013: 99).

Despite the prevalence of Spolsky’s model throughout the last ten years in FLP, some scholars have been engaging with other models or theories, particularly in the last five years. For instance, Ó hÍfearnáin (2013) frames his mixed-method investigation of language practices and attitudes of Gaeltacht Irish speakers toward intergeneration transmission within a folk linguistics approach. Smith-Christmas (2014), in turn, situates her study about the three generations of one family involved in the use of an autochthonous minority language (i.e. Gaelic) within the field of language socialisation (Schiefflin and Ochs 1986). In addition, Purkarthofer (2017) creatively combines an understanding of the notion of linguistic repertoire informed by interactional, poststructural and phenomenological approaches (Busch, 2012) with the assumption that

it is crucial to consider the construction of space in social analysis, which is accomplished by drawing on Lefebvre's (1991) framework of the production of space.

Finally, Gallo and Hornberger (2017) propose an ethnographic approach to language policy as a way to account for the complexity and creativity involved in the ways social actors adopt, follow or resist language policies (Hornberger and Johnson 2011). Tapping into under-researched notions and topics in FLP such as borders, securitisation, and immigration policies, Gallo and Hornberger (2017) report the case of an eight-year-old girl (Princess) and her family living in the United States, including her father who was deported to Mexico during data collection. Engaging with yet another under-explored discussion in FLP, namely how languages can be conceptualised as something other than a fixed category, the authors draw on the notion of continua of biliteracy (Hornberger 2002) to demonstrate Princess' active role on her family's migration decisions and language planning. Also, they highlight how the ethnographic approach to LPP allows uncovering the monoglossic language ideologies upon which participants draw in order to make future decisions regarding migration and schooling.

These four studies illustrate that drawing on concepts, theories and approaches other than those sustained by Spolsky's framework might contribute to developing FLP in directions that have not been much explored. More recent overviews of FLP have noticed this move away from Spolsky's model and expanded this limiting understanding of (family) language policy. For instance, King (2016: 727–8) advances the idea that research belonging to 'the fourth phase' of FLP is characterised by '[the examination of] language competence not just as an outcome, but as a means

through which adults and children define themselves, their family roles, and family life; a focus on globally dispersed, transnational, multilingual populations beyond the traditional, two-parent family; and ever-greater heterogeneity and adaptability in research methods to address these shifting needs in the field.'

In the same vein, King and Lanza (2017) identify two trends in current FLP research. The first trend is characterised by the increasing attention given to demographic changes seen through a lens that draws on notions such as migration, mobility and transnationalism to better understand multilingual practices. The second trend involves a shift from examining the relationship between language input and its entailing outcomes, to investigating (rather than assuming) the contexts in which family communication takes place. A methodological implication of this shift is the increased use of ethnographic approaches, which brings us to the second point of convergence among recent overviews of FLP.

The gain of currency of ethnographic methods

The potentially limiting consequences of the affiliation to a single theoretical model (i.e. Spolsky's) as the foundation of FLP implied by Schwartz (2010) are dispelled as she presents future directions for FLP. Among other things, she stresses the importance of collecting and examining naturally occurring speech using ethnographic methods. More recent papers seem to have answered this call.

For example, in her 9-year investigation of language ideologies and practices in Oaxaca, Mexico and California, the United States, Pérez Báez (2013) used interviews and participant observation to demonstrate the influence of external factors (i.e. school and social networks) on attempts

of families to maintain San Lucas Quiavianí Zapotec, and to unveil the language ideologies circulating within the communities under investigation. Kheirkhah and Cekaite (2015) examined the language practices of one Persian-Kurdish family in Swedish through video recordings, ethnographic observations and interviews. These methods allowed them to identify the different strategies used by parents in interaction with the child, and to emphasise the importance of considering children as agents in the implementation of family language policies. Children's agency was also central to Gyogi's (2015) study of two English-Japanese bilingual children and their mothers in London, UK, where she claims that children demonstrate their agency by contesting, negotiating and redefining their mothers' language beliefs.

The increasing use of ethnographic methods in FLP calls for a consideration about the extent to which Spolsky's general model and its underlying epistemological and ontological assumptions are compatible with those of ethnographic approaches to the study of language and society. Perhaps Spolsky's model is better suited for studies that aim at working with larger numbers of participants, identifying general patterns, and predicting likely outcomes. But these are generally not the concerns of ethnographies, whose focus is on gaining in-depth understandings of localised practices while locating these interpretations in longer or broader social processes (Rampton 2012).

King and Lanza (2017) point out that FLP can benefit from recent developments in socio- and applied linguistics, as well as in LPP. They suggest that LPP studies have been increasingly making use of 'critical and qualitative methods' (King and Lanza 2017). However, while the popularity of qualitative and ethnographic methods

is easily perceived in recent FLP scholarship (e.g. Curdt-Christiansen 2016; Schwartz and Verschik 2013; Smith-Christmas 2016; Zhu Hua and Li Wei 2016), I argue that 'critical research perspectives' (Hult and Johnson, 2015: 11) have only been employed timidly by recent FLP literature (e.g. Gallo and Hornberger 2017). Before fully developing this argument (in section 4), I discuss how overviews have treated the empirical advancements in FLP, and I present my own considerations about them.

Diversity of languages, geographical locations, family configurations

King et al. (2008) suggested that future FLP research focussed on issues related to globalisation and transnationalism as these processes might have considerable influence on language practices in the home. Curdt-Christiansen (2013: 2) shows how recent studies have explored this path as they 'include non-middle class, marginalized and under-studied transnational family types as well as Indigenous and endangered languages'. Furthermore, studies in what King (2016) refers to as the 'fourth phase' of FLP demonstrate a focus on family configurations other than those with two middle-class parents.

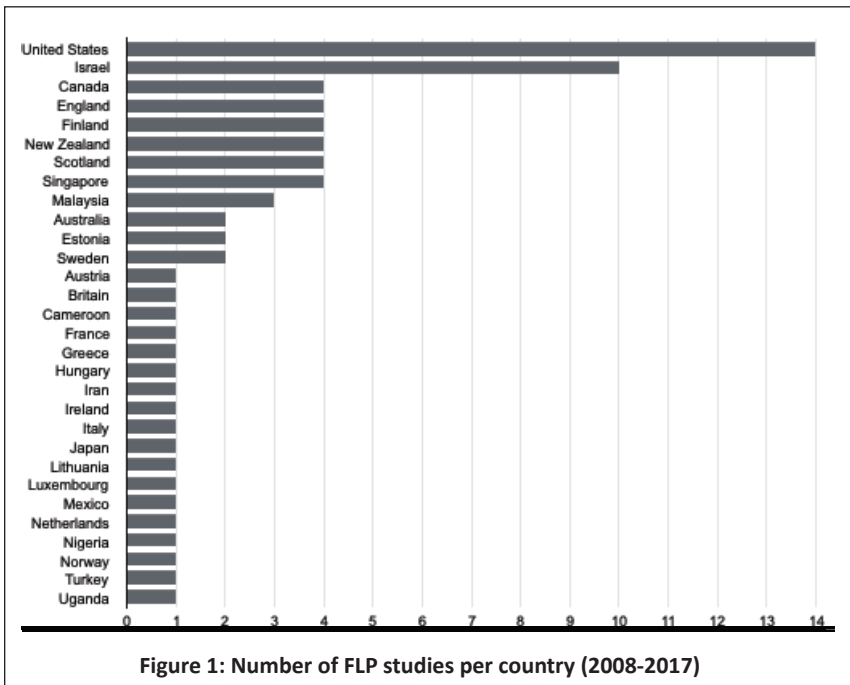
More recently, King and Lanza (2017) note both the focus on families that go beyond the traditional, two-parent model and a greater variety of languages. This is echoed by Smith-Christmas (2017), who recently pointed out that although there has been an inclusion of different geographical locations where data has been collected, a strong focus on North American and European contexts still exists. In figure 1 I present the number of original FLP studies by country where data was collected. As noted, the studies had to contain the phrase "family language policy/ies" in the title or abstracts, and be published between 2008 and 2017.

In analysing recent developments in FLP research, Smith-Christmas (2017: 18) justly remarks that ‘there is a dearth of research situated within Africa or the Middle East (apart from Israel)’. She then suggests that our understanding of language use in the family would benefit from studies that capture the experiences outside the viewpoints of Western, industrialised communities. I concur with her suggestion, and some studies have already been exploring this direction (e.g. Kendrick and Namazzi 2017; McKee and Smiler 2017; Mirvahedi 2017; Moore 2016).

While the relevance of investigating family configurations, locations and languages that we still know little about in FLP should be recognised, a critical approach to family multilingualism supports the idea that bringing voices from the global South into current sociolinguistic debates is not only a matter of changing the context of investigation, but shifting the current

paradigm that renders the global North as the producer of theory and the global South as the source of data against which theories are tested (Connell 2007). Along with the need to expand the scope of FLP not only as places where data are collected, but also as geopolitical loci where knowledge is produced, there has been a need to include research that investigates the particularities of language practices by families that use non-European languages.

Table 1 illustrates that recent scholarship reviewed here (following the aforementioned criteria) has broadened the range of languages examined in FLP. While this effort attests to an important empirical advancement of the field, there exists a stronger tendency to draw on assumptions about language akin to *positivist modernist sociolinguistics* (García et al. 2017). That is, in general, studies seem to subscribe to ideas of languages as being units that can be delineated, separated, named and counted. Rather



than affirming that languages are *not* abstract systems that can be named, differentiated and counted, the point here is that there is an important ongoing debate in socio- and applied linguistics (e.g. Canagarajah 2013; García and Li Wei 2014; Jørgensen 2008; Pennycook and Otsuji, 2015) with which recent FLP studies have not engaged.

For instance, in Seloni and Sarfati's (2013: 9) investigation of language ideologies and practices of families in Turkey, they justify the employment of the term Judeo-Spanish for it is a "neutral, self-explanatory term" (Harris 1982: 5) embraced by most scholars working on the topic.' Interestingly, Harris (1982: 5) continues 'Others

consider it a pseudoscientific term to be used only for purposes of popularization', demonstrating how naming languages is not exactly a neutral enterprise.

Another insight that table 1 yields has to do with how languages and language varieties are named. In Curdt-Christiansen's (2009) study, she employs ethnographic methods to identify the values assigned to Chinese, English and French by Chinese parents in Quebec, Canada, and how these are linked to particular linguistic markets. More recently (Curdt-Christiansen 2016), in examining the language ideologies and practices of three multilingual families in Singapore, Hokkien and Mandarin (rather than the all-encompassing label Chinese) are

Table 1 – Languages* investigated by FLP studies between 2008 and 2017 (in alphabetical order)

Albanian	Ibibio	Polish
American Sign Language	Igbo	Punjabi
Amharic	Irish	Qur'anic Arabic
Arabic	Italian	Russian
Azerbaijani	Japanese	San Lucas Quiaviní Zapotec
Cantonese	Judeo-Spanish	Scottish Gaelic
Chinese	Khmer	Sinhala
Dutch	Korean	Spanish
Efik	Kurdish	Swedish
English	Latvian	Tagalog
Estonian	Lithuanian	Tagalog/Visayan
Farsi	Lokaa	Taiwanese
Finnish	Luganda	Tamil
French	Malacca Portuguese Creole	Teochew
Fulfulde	Malay	Thai
German	Mandarin	Turkish
Hakka	New Zealand Sign Language	Ukrainian
Hebrew	Nigerian pidgin	Urdu
Hokkien	Norwegian	Vietnamese
Hungarian	Persian	Zapotec

* The names of the languages are reproduced here the same way researchers used in their own works.

the terms employed to account for the languages used at home.

Furthermore, in his survey involving 170 children in Ontario, Canada, Slavkov (2016) aimed at developing a framework capable of accounting for the factors that lead to (or prevent) bi/multilingualism. He was specifically interested in examining the roles of family language policies and school language choice in promoting bi/multilingualism. Relying on descriptive and inferential statistics, Slavkov (2016: 17) concludes that ‘if non-overlapping language strategies are adopted as a best practice at the family and educational levels, all children in Ontario, and potentially the rest of Canada, can become bilingual and many of them multilingual.’ A concerning corollary of this proposition is its implicit idea that bi-multilingualism is inherently good, and it should ultimately be pursued, obfuscating the social, cultural, political dimensions of language practices, which should be central to sociolinguistic analysis.

The three examples above are representative of a more common tendency in FLP. While the increased use of ethnographic approaches has eschewed certain taken-for-granted notions and yielded more refined accounts of the situatedness of language practices, a central element in sociolinguistic research, namely, language has not undergone the same scrutiny. In other words, FLP literature has not been particularly successful in openly discussing the ontological status language receives in the analysis. Relatedly, most recent FLP studies have not engaged with conceptualisations that challenge the notion that languages are autonomous systems that can be separated into discrete units, named and counted. Despite the relative novelty of conceptualisations such as translanguaging practice (Canagarajah 2013), polylingual languaging (Jørgensen 2008; Møller

and Jørgensen 2009), metrolingualism (Otsuji and Pennycook 2010; Pennycook and Otsuji 2015), translanguaging (García and Li Wei 2014; Li Wei 2018, Otheguy et al. 2015), and *Spracherleben* (Busch 2015), scholars (e.g. Haugen 1972; Khubchandani 1983) have discussed this for many decades, which makes the little engagement of FLP literature with these issues (but see Conteh et al. 2013) even more intriguing. It should be highlighted, however, that the employment of these notions is not regarded as a panacea (see Jaspers and Madsen 2016; Orman 2013; and Pennycook 2016). Instead, the point made here is twofold: drawing on these notions might help to elucidate issues related to family multilingualism in innovative ways; and FLP has the potential to make original contributions to the very debate about what language is.

An untrodden path: A critical approach to family multilingualism

Below I summarise certain ontological and epistemological assumptions of Spolsky’s model that, I argue, engender limitations for the development of FLP. I go on to sketch how these assumptions are challenged by what has been described as critical (Pennycook 2001; 2004; Pietikäinen 2016; Roberts 2001) approaches to research on language and/in society.

The first assumption is that ‘language behavior is reflective of sociocultural patterning’ (Fishman 1972: 441). This assumption is echoed by Spolsky in his claim that ‘[language management] is not autonomous, but the reflex of the social, political, economic, religious, ideological, emotional context in which human life goes on.’ (Spolsky 2009: 9) Second, although Spolsky recognises that language varieties ‘are socially or politically rather than linguistically motivated’ (Spolsky 2009: 1), and

underlines the ‘sloppiness of the labels we have available’ (Spolsky 2004: 161) to describe multilingual practices and multilingual contexts, the understanding of language that his model puts forth is that it is an abstract, bounded, discrete entity that can be neatly delineated, categorised and counted. Third, the salience of role relations (Fishman 1972) between participants, subsumed under the notion of domain, as opposed to perspectives which bring to the fore social categorisations such as race, ethnicity, gender, sex, class, age and ability. Fourth, while recognising the need for ‘a detailed study of the face-to-face interactions in which language choice is imbedded’ (Fishman 1972: 442) as a requirement to support the validity of domain as a concept, Spolsky builds his case drawing on methods other than face-to-face interactions, or other data generation tools typically employed by ethnographic approaches.

In the past two decades or so, scholars investigating issues within the fields of sociolinguistics and applied linguistics have been qualifying certain strands of research as *critical* (e.g. García et al. 2017; Heller 2011; Martin-Jones and Martin 2016; Mesthrie and Deumert 2000; Pennycook 2001; 2004). The use of this term usually denotes (a) certain epistemological stance(s) taken by researchers, along with respective ontological assumptions. I situate this article within this debate and, below, I present three ways in which FLP might benefit from drawing on a critical approach.

The term *critical* employed here is meant to encompass approaches that take, oftentimes, a social constructivist epistemological stance to the study of language and society, assuming that language practices and social reality are dialectically and recursively entangled. Heller (2011: 34), for example, highlights the constructive dimension

of language in that it has a complex role ‘in constructing the social organization of production and distribution of the various forms of symbolic and material resources essential to our lives and to our ability to make sense of the world around us.’

Furthermore, these approaches tend to be interested in examining social reality as a way to unveil the ways in which power and wealth are unevenly distributed in society. In what has been termed *critical poststructuralist sociolinguistics* (García et al. 2017), researchers tend to draw on the Foucauldian assumption that power is ubiquitously present in society (as opposed to an institutional, centralised, top-down view of power) to investigate ‘language practices in interrelationship to the socio-historical, political, and economic conditions that produce them.’ (García et al. 2017: 5). Moreover, authors oppose an epistemological stance that stands for the production of objective, neutral and universal knowledge systems, and champion, instead, a stance that assumes the situatedness of knowledge production (Heller 2011; Mignolo 2011b).

Also building on Foucault (1969; 1975) to account for the relations of power, Heller (2011) draws on a historical materialist approach, stressing the need for sociolinguistic analyses to consider the material basis of social organization. She proposes a *critical ethnographic sociolinguistics*, which is built on two pillars: ethnography and political economy. While the former permits an understanding of language use as situated practice and its connections to social structure, the latter emphasises the need to understand the constraints imposed by material conditions on meaning-making activities (Heller 2011).

It is not uncommon for authors to go beyond exposing social inequalities and injustices, and propose ways to address such inequalities and injustices stripping away the neutrality and

objectivity that marked the initial stages of modern social sciences and the early days of sociolinguistics in the 1960s, and unveiling the social and political roles of sociolinguistics as a discipline that could advocate ‘for a more equitable future’ (García et al. 2017: 6). However, the necessity for research-led social transformation is not necessarily the ultimate goal of a critical approach, as the steps that precede it might indicate possible ways of action, if any (Heller 2011).

One final aspect of the notion of *critical* that has motivated its use relates to a certain degree of scepticism that inspires scholars to question taken-for-granted concepts, approaches, and methods, regardless of how prevailing they are. A phrase that captures well this sceptical posture is ‘the restive problematization of the given’ (Dean 1994: 4, as cited in Pennycook 2004: 799). Assuming this posture is what yields the questioning of the ontological status of language supported by *positivist modernist sociolinguistics* (García et al. 2017). Therefore, rather than understanding languages as abstract entities that can be separated, named and enumerated, languages are thought to be ‘the consequence of deliberate human intervention and the manipulation of social contexts’ (García et al. 2017: 6). It is within the context of this discussion that I present how a critical approach to family multilingualism contributes to the development of FLP.

What could decoloniality mean for the field of FLP?

What if FLP research explored more explicitly the implications of taking a stance that considers the relationship between language and social reality to be mutually constitutive of one another, rather than unidirectional? What if family multilingualism is theorised

through conceptualisations that expand (or squarely challenge) notions of language as abstract, separable, and countable systems? To what extent can ethnographic methods be employed cohesively with Spolsky’s framework? In sum: what if the interdisciplinary nature of FLP promoted an engagement with pressing discussions in socio- and applied linguistics (e.g. Busch 2015; Canagarajah 2013; Jørgensen 2008; Li Wei 2018; Pennycook and Otsuji, 2015), LPP (e.g. Hult and Johnson 2015; Johnson and Ricento 2013; Ricento 2000) and social sciences (e.g. Castro-Gómez and Grosfoguel 2007, Connell 2007; Mignolo 2011b; Santos 2014) that have not been thoroughly explored in recent FLP studies?

Rather than providing definite answers, I aim at opening up a discussion about the limitations of FLP as a field and possible ways to push, transgress or erase its boundaries. To this end, I propose a critical approach to family multilingualism. Particularly, I argue that a decolonial approach to the study of family multilingualism offers a perspective which underscores the intersectional dimension of social categorisations such as gender, race and class, while attending to the political and economic dimensions of the transnational centre-periphery divide. Furthermore, such an approach takes a step towards disrupting the current unbalance of geopolitics of knowledge, foregrounding Southern perspectives in the analysis of language practices.

The effort made by researchers to contribute to the development of FLP by investigating a great variety of contexts is, indeed, laudable. Along with the increased use of ethnographic methods, the expansion of scope in terms of languages, countries and family configurations can have a substantial impact on FLP literature, and possibly beyond, as it may yield more in-depth understandings about the situatedness

of language practices. Notwithstanding, this push of boundaries of the empirical scope of FLP research can only go so far if epistemological and ontological shifts do not accompany it. Put differently, FLP as a field can have its development severely restricted if it draws solely (or mostly) on notions of languages as fixed category. One way to overcome this limitation would be to draw on conceptualisations of language presented in section 3, and investigate their suitability for the study of language use in the home.

Furthermore, despite engaging with discussions such as the demographic, economic and political implications of transnationalism and globalization – mainly through a political economy analysis, though not always explicit – there has not been significant and express engagement with theoretical frameworks that assume the complexity, heterogeneity and fluidity of cultures (e.g. Ahmed 2000; Anzaldúa 1987; Appadurai 1996; Bhabha 1994). Finally, as long as the relevance of investigating families that go beyond the ‘traditional, two-parents model’ is framed within a logic of ‘denial of coevalness’ (Fabian 1983), FLP as a field of inquiry might restrict itself to a liberal understanding of diversity (Kymlicka 1995), and overlook debates that shed light on issues such as social class (Block, 2015), gender and sexuality (Fabrício and Moita Lopes 2015; Milani 2018), race and ethnicity, (de Melo and Moita Lopes 2015; Reyes 2017; Rosa and Flores 2017; Samy Alim et al. 2016; Williams and Stroud 2014), and disability (Grue 2016).

One way to overcome this limitation, and in line with the growing need to include southern perspectives in current sociolinguistic debates (cf. Levon 2017; Milani and Lazar 2017; García et al. 2017), the critical approach to family multilingualism proposed here draws on the works of scholars involved with the decolonial turn (Castro-Gómez and Grosfoguel 2007). Castro-Gómez

and Grosfoguel (2007) claim that while the forms of domination employed by European nation-states might have changed, the structure that sustains the relations between ‘central’ and ‘peripheral’ countries remains the same. That is, despite the legal-political decolonization that has legitimated the independency of former colonies, the structures of domination based on the hierarchisation of races/ethnicities and gender/sexuality set in place in the sixteenth and seventeenth centuries are still reproduced through the international division of labour between centre and periphery, and contribute to the contemporary social and economic divide (Castro-Gómez and Grosfoguel 2007).

Additionally, while other approaches to the examination of class, gender, and races in a context of globalisation may favour the economic or the cultural domains in their analyses, a decolonial perspective envisages the entanglement between culture, and economic and political processes. Put differently, Castro-Gómez and Grosfoguel (2007) suggest that world-system analysis as put forth by Wallerstein (1991) builds on the Marxist paradigm of base/superstructure, and assumes that culture (superstructure) derives from relations of production (base). Conversely, postcolonial studies invert this relationship and support the idea that economic and political relations do not have a meaning in themselves; rather, they gain meaning in specific semiotic sites. Each approach, thus, is considered to build their analyses upon opposing ontological assumptions. Drawing on a decolonial perspective may offer reconciliation between these conflicting approaches whilst sharing some of their concerns.

Following this discussion, a critical approach to family multilingualism drawing on a decolonial approach might be useful for pushing the development of FLP in a direction that has not been

explored. That is to say, incorporating in FLP research the propositions put forth by Castro-Gómez and Grosfoguel (2007) related to the ontological status of social categorisations allows for a useful framing of these categorisations while undertaking an analysis of family multilingualism. Moreover, the deliberate effort to draw on theorisations from the global South, particularly those related to globalisation, transnationalism and the effects of Western, modern scholarship (e.g. Castro-Gómez and Grosfoguel 2007; Kerfoot and Hyltenstam 2017; Mignolo 2011a; Mignolo 2011b; B. Santos 2014; M. Santos 2017) can inform social analysis in ways that have not been much explored in sociolinguistics in general, let alone in FLP, and shed light on debates about transnational practices, identity negotiation and language use.

Finally, an issue that is still unresolved in FLP is the extent to which certain practices can be conceived of as management (or policy) if they are covert and implicit. Curdt-Christiansen and Lanza (2018: 126) see this tension as the ‘blurred distinction between the concepts of language practices and language management’, while Pennycook (2017) takes a more direct stance in suggesting the irreconcilability between an understanding of language policy stemming from Fishmanian sociolinguistics (i.e. Spolsky’s framework) and an understanding that highlights the situatedness of language practices. I claim critical, ethnographic approaches (Martin-Jones and Martin 2016) to FLP may open up a promising site for carrying on this debate about language practices and language policy.

Conclusion

While serving as an important common ground upon which scholars with similar interests contributed to the emergence and establishment of a scientific field of inquiry, a discussion of the implications

of the assumptions of Spolsky’s framework is lacking in current FLP literature, unlike in LPP literature (cf. Albury 2016; and Pennycook 2017). The relevance of this discussion lies on the possibilities opened up by a critique of Spolsky’s framework at a theoretical level and its implications for FLP research. Therefore, in this article I discussed the epistemological and ontological assumptions of Spolsky’s framework, and the potentially limiting implications of its pervasiveness in recent FLP literature.

However, because Spolsky’s model draws largely on tenets supported by Fishmanian sociolinguistics, it holds certain assumptions that are difficult to reconcile with critical approaches to the investigation of family multilingualism. I showed that some FLP studies are already going beyond Spolsky’s framework, either by trying to expand it or by drawing on different theoretical frameworks. Additionally, I suggested that drawing on recent debates about how language can be conceptualised may be a productive path to follow in studying language practices in the home. Finally, I showed how a decolonial approach to family multilingualism might lead to original discussions about issues that have not been much explored in recent FLP literature.

A potential complication of the increasing interdisciplinarity in FLP has already been raised by King (2016: 731): ‘the field of family language policy risks splintering in such a way that there is diminished capacity for researchers to exchange findings, collaborate, or even make meaningful sense of others’ work.’ However, in section four I argued for the ways in which a critical approach to family multilingualism might contribute to the development of FLP, and because of that, I suggest that the risk brought up by King (2016) is worth taking.

Acknowledgements

A previous version of this paper was presented at the launch of the Harmonious Bilingualism Network (HaBilNet1) on May 24-25, 2018 in La Hulpe, Belgium. I should like to thank my academic supervisors Pia Lane, Elizabeth Lanza, and Alastair Pennycook for comments on earlier drafts. Also, I benefited from the constructive feedback I received from Kristin Vold Lexander, Sébastien Lucas, Maria Obojska, and Bente Ailin Svendsen. Moreover, the insightful comments from the anonymous reviewer and the editor were crucial in shaping the article into this final version. Shortcomings and mistakes are my own. This work was partly supported by the Research Council of Norway through its Centres of Excellence funding scheme, project number 223265 (MultiLing).

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Volume 5
Number 2
December 2018

A journal of multilingualism from the periphery

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