
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’, invites us to think more carefully about ‘theories’ from the north, particularly in relation to educational linguistics and sociolinguistics. In this he argues that conventional methodologies and theories are unsatisfactory and that instead we need to seek alternative methodology/ies that are congruent with alternative ways of viewing language, for example, in relation to what he identifies as ‘translanguaging theory’. In his paper, Ndhlovu joins a swelling cohort of scholars who engage with the possibilities of ‘de-linking’ from or exercising ‘epistemic disobedience’ towards the assumed supremacy of knowledge, theory and methodology thought to originate in Europe, and sometimes North America (Mignolo, 2007, 2009; Connell, 2007). In this response to Ndhlovu’s paper, I respond not so much to the overall thrust of his paper – that is the articulation between his proposed research methodology, autoethnography, and the object of his enquiry, translanguaging, but rather I focus on the north-south (N-S) entanglements in relation to ‘translanguaging’ as pedagogy, and theory. Ndhlovu is provoked to write his paper from an experience of

educators recounting their approach to translanguaging as pedagogy, although his interest seems more towards discussion of translanguaging as theory.

At this point, I should acknowledge, that I have ambivalent views of the affordances and potential of translanguaging. I am concerned that this is a pedagogy that has apparently emerged from a context of bilingual education in Wales, appropriated and re-purposed in New York City, then journeyed back to the UK, not to Wales so much as to Birmingham and London. From these northern metropolises, it has had eight years of charmed passage across platforms of considerable influence (several major international conferences, particularly in the USA), and then exported to southern contexts, such as South Africa (cf. Heugh 2015, 2017, 2018). At first, the interest was with translanguaging as pedagogy in the context of bilingual education (Wales) (Williams, 1996), and then in post- or beyond- bilingual education (USA) (García, 2009; García & Wei, 2014). Recently, particularly in the work of Li Wei (2017), translanguaging is being proposed as new theory in applied linguistics. It is only during the second half of 2018 that it has begun to receive serious critique on platforms located both physically and ideologically in southern contexts (e.g. at the Sociolinguistics Symposium 22 in Auckland, and in a panel discussion of theory at the International Congress of Linguists in Cape Town).

My first reservation is that if an apparently northern pedagogy, and possibly ‘theory’, is being embraced in southern contexts, in the context of discourses of post-colonialism and / or de-coloniality, then we have a theoretical problem of habitus that tricks us into thinking that new northern theory is any less hegemonic than in another historical period. Alternatively, we have an example of ‘entanglement’ (Kerfoot

& Hyltenstam, 2017). Assuming it were the latter, how then do we go about teasing apart this entanglement of apparent dissonance between a northern articulated pedagogy and possible theory that is seemingly embraced by critical post-colonial (even de-colonial) scholars in the south who claim to distance themselves from the hegemony of northern thinking? My second reservation has to do with the epistemological antecedents of translanguaging. Are we sure that this is a pedagogy and emergent theory that originated in Europe or North America in the first place? Is it not the case that it has been very much part of an ongoing discussion of the nature of language-multilinguality-linguistic fluidity that has criss-crossed southern and northern debates, albeit couched in different vocabulary current at various historical points in time? Are we sure that those whose names have grown in eminence in relation to this term in recent years have discovered or are making novel of something about which people who live in linguistically complex parts of the world are far more adept and knowledgeable? Is this perhaps, an example of how southern knowledge is appropriated by northern scholars without fulsome acknowledgement, renamed and sold back to the south in yet another instance of epistemic erasure?

Although many scholars in post-colonial or minority settings have tried to de-link, many of us find that there is no clear 'abyssal line' between the coloniality of northern thinking and de-coloniality of southern thinking (e.g. Santos, 2012, 2018). Instead we are caught within a web of interdependencies,

some more generously transparent and communitarian, others more predatory and individualistic. If we engage in critical reflexivity of the interconnectivities between south and north it is possible that this may lead us towards a conscious awareness of how different systems of knowledge, belief and ways of being have come together and diverged in post-colonial settings. We may even be able to arrive at what Argentinian Rodolfo Kusch calls 'a mestizo consciousness' ([1970] 2010). By this, I understand Kusch to mean 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 that have become intertwined). De-colonial thinking, to my mind, therefore does not require cleavage, but it may well require reflexivity in relation to pedagogy, practice and theory. Here we might recall Paolo Freire's perspective of critical and reflexive pedagogy that reintroduces a 'humanising' dimension to what has become a lopsided claim of 'objectivity' (Freire [1970] 2007; see also Zinn et al. 2016). We might also consider Pierre Bourdieu's 'epistemic reflexivity' (e.g. 1989, 1990), as carefully examined by Linus Salö (2018) in a recent issue of *Multilingual Margins*.¹

Ndhlovu contributes to contemporary debates that circulate in various branches of linguistics, from the perspective of a sociolinguist, concerned with contemporary transgressive views and theorisation of language and what he identifies as a potential disjuncture between these and the conventional methodologies employed to investigate

1 I should like to acknowledge how profoundly insightful is Linus Salö's paper, and also how much more difficult he made my work in responding to Finex Ndhlovu, and how much the articulation between the points of departure of both scholars has forced me to think about the affordances and silences within translanguaging discourses. I should also like to thank Necia Billingham for drawing my attention to Linus's paper which I had intended to read after, but not before writing this. Thank you, Necia.

language in society. Like Linus Salö (2018), Ndhlovu makes a case for reflexivity in research practice, however their axes of critical gaze differ. 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 interest arises not from a deliberative focus on reflexivity as method from the outset, but rather he arrives at it incidentally after escaping from what he experiences as a disappointing demonstration of bilingual teaching practices masquerading as translanguaging. He arrives in an unplanned situation, with a perspective of translanguaging in mind (apparently an object of his concern). It is then that autoethnography as methodology (along with Omphile) seems to find him rather than the other way around. What happens next is that Ndhlovu's attention becomes focussed on matching an assumed anti-colonial methodology with an assumed 'anti-colonial' or 'anti-foundational' theory of language and his reflexive gaze articulates between the two. In his reflective encounter with Omphile and post-encounter reflections, it was not Ndhlovu's intention to examine the foundations of his own epistemology of language, from whence these came, under which circumstances, or the degree to which this may matter.

Ndhlovu's point of view of translanguaging and mine are different. He is more interested in translanguaging as theory, whereas mine is to do with pedagogy particularly in the education of students from marginalised communities. The term itself is relatively new, and as suggested above was somewhat controversially appropriated from its Welsh origins (Williams, 1996) and re-purposed in an alternative, apparently novel discourse of linguistic fluidity in New York, Birmingham and London

(e.g. García, 2009; Blackledge & Creese, 2010, García & Wei, 2014). Like Rama Kant Agnihotri (2007, 2014) before him, 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. Agnihotri suggests even longer than this, although discussed in contemporary vocabulary of the day. After four decades of his own exploration of linguistic diversity and fluidity in India, the UK and South Africa, Agnihotri, however, 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). In this he includes translanguaging. Curiously, rather than acknowledging the historical antecedents of and connections with discussions of translanguaging, most authors seem to focus on a growing list of reasons why translanguaging differs from its antecedents (language mixing, code switching, translation and so on. Frequently they stigmatise the work of scholars associated with teaching languages; bilingual and multilingual education; and the process and practices of code-switching, code-mixing, and so on.

Few seem to notice or be perturbed that translanguaging as appropriated from Welsh bilingual education and then re-purposed in New York, Birmingham and London, is somewhat different in form, function and purpose from its Welsh origins. These are much closer to purposeful use of code-switching. The authors offer few, if any, explanations of how or why the appropriated and re-purposed translanguaging has advanced earlier work on linguistic fluidity, for example that in Africa and or other post-colonial contexts of considerable diversity (as discussed in Agnihotri, 2014; Heugh, 2018; Heugh & Stroud, in press). Instead, translanguaging has captivated the imagination of several

linguists searching for ‘transformative’, de-colonial pedagogies (e.g. Makalela, 2015, 2017; McKinney, 2016), despite it having emerged from very different circumstances from those found in African countries.

This 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. What would such an assumption imply of the intellect and expertise of scholars and ordinary people for whom multilingualism and multilinguality are and have always been everyday realities? It worries me also because the idea of ‘translanguaging theory’ presupposes that multilingualism (in the guise of another name) in one part of the world is or can be 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 the habitus and hegemonic supremacy. In other words, are we not seeing the reproduction of (neo-)colonial thinking in contemporary discourses of translanguaging?

My worry continues though. Most proponents of translanguaging, and indeed the term itself, presuppose borders between languages that must be crossed. The very notion of ‘porous borders’ obviously invokes borders, despite all claims to the contrary. Surely either there are borders or there are none? Thus far the only linguist who entirely refutes the notion of language/s is Agnihotri who argues that ‘there is only multilinguality’ (2014, p. 364) and he is not a northerner, he is quite firmly a southerner.

In the growing doxa of linguistic fluidity, translanguaging, and so on, there is a dark silent space of denial. Just as people use what I would prefer to call their multilinguality (or functional multilingualism) for horizontal means of communication, in circumstances where conviviality is intended, preferred, or appropriate, we also use language for purposes of exclusion. We use language to exclude others for numerous reasons and this is not a recent phenomenon, as we know from the Old Testament or Biblical ‘shibboleth’ test. Academic discourse in both written and spoken ‘forms’ excludes most people of the world, as does legal, financial, medical, engineering, actuarial discourse. Children engage in play and styling activities in which they certainly use their multilingual repertoires for inclusion and demonstrations of linguistic fluidity, and even tolerance of adults, as does Omphile. However, they also use their multilinguality to draw on their knowledge of linguistic boundaries for purposes of hierarchical or vertical exclusion, for example as observed in Uganda by Wolff (2000). When our intention is to limit or preclude access, for whatever reason, we create lines of division and borders that circumscribe our use of language. Geopolitical distance contributes to division, so that the linguistic structure of Putonghua is not the same as that of English, French, Hindi or isiXhosa. The degree of porosity between Putonghua and isiXhosa is unlikely to be of a similar order to that between isiXhosa and isiZulu. Borders between languages both appear and disappear, shrink and swell, depending on who is doing what, when, where, why and how.

A contemporary ‘shibboleth’ test for modern day linguists would be to ask people, with considerable multilingual or translanguaging expertise but who have had limited access to one of the ‘so-called’ international languages, to

identify a key impediment to the access of higher education or high-level career opportunities. The answer is likely to be an unequivocal naming of at least one language, clearly identified with exclusive boundaries. Students still need access to one of these bounded languages, if we are to pursue social justice and equity. In the context of on-going mobility, they need opportunities to expand their horizontal expertise in multilingualism for reasons of conviviality and also the expertise to navigate through circumstances of vertical linguistic exclusion.

It is for such reasons that we may need to rethink a default tendency to position linguistic alternatives as 'either-or' dichotomies, rather than 'both' options. Twenty-five years ago, the debates in South Africa, and elsewhere, got stuck on 'either mother-tongue = bad or English = good', rather than 'bilingual or multilingual' (i.e. both mother-tongue / local language(s) and English) education. With the arrival of translanguaging, we seem to be witnessing yet again a default to prescriptive thinking and false dichotomy in 'either bilingual (multilingual) = bad' or 'translanguaging = good' alternatives in prominent debates. Is this not a resurfacing of prescriptivism, a trickster habitus in new clothing?

If we can't find our way through the mire of false dichotomies and pejorative and prescriptive linguistics, it is unlikely that we will find a direct route towards social justice and equitable access for students in education systems, and it is unlikely that we will shake off the burden of coloniality. For most students in southern countries, particularly in Africa, translanguaging as discussed solely from the perspective of fluidity and in denial of the materiality of vertical linguistic exclusion is not and cannot be the panacea of social justice it is claimed to be.

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

conversations, such as his conversation with Omphile that offers insight into the opportunities of horizontal linguistic fluidity. So too are observations of children learning how to use their multilinguality in strategic moves to navigate linguistic boundaries of exclusion in Uganda (Wolff, 2000). It is through such conversations and reflexive observations 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.

Discussions of linguistic fluidity, multilingualism and translanguaging are particularly important in a world of increased mobility of people and we need to take these seriously in rethinking the implications for language education (cf. Stroud & Heugh, 2011). In this regard, methodologies that have emerged from southern and northern contexts, Freire's notion of 'humanising' reflexivity and Bourdieu's principle of epistemic reflexivity, may assist us on a path towards 'mestizo consciousness' rather than false dichotomies.

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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 translingual considerations on the “multipleness 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 translangualism 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 translanguaging 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 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 Translinguaging 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 translinguaging 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 translinguaging-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 translinguaging 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

References:

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