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# The "Native" as Ethnographer: Doing Social Research in Globalizing Nsukka

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### Abstract

Researchers have noted how local attitudes that connect research to the external world could affect findings differently in different contexts. How this played out for an indigenous researcher is the new perspective presented in this paper. Although an indigene of the study area, I became an outsider of sorts as soon as I began to show interest in malaria in a way that suggested to locals that the results of my investigation could eventually get to the government or the Western world – locals saw those two entities as embodying power and material abundance. Although I worked as an insider, I noticed that my position as a researcher also gave me an identity of the authority and power of the state and the global North. As soon as I began fieldwork, targets began to respond to me more or less as they would do to representatives of government or the North; for whatever I eventually get “out there,” irrespective of my identity, could be crucial information for local wellbeing. Again, locals (especially in predominantly non-literate settings) often associate paper and writing to government and organizations based outside local communities. This played out as I requested some participants to sign consent forms. They were uneasy to sign the forms, and those who did sign became less open afterwards. This brought home the point that ethics of social research need not be rigidly followed across diverse contexts. In all, factors such as level of literacy and general perception of the ultimate purpose of research among the target population will affect data validity to different degrees irrespective of the researcher’s identity.

### Keywords

Control Effect, Data Credibility, Indigenous Ethnography, Informed Consent, Participant Observation

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*Researchers have noted how local attitudes that connect research to the external world could affect findings differently in different contexts. How this played out for an indigenous researcher is the new perspective presented in this paper. Although an indigene of the study area, I became an outsider of sorts as soon as I began to show interest in malaria in a way that suggested to locals that the results of my investigation could eventually get to the government or the Western world – locals saw those two entities as embodying power and material abundance. Although I worked as an insider, I noticed that my position as a researcher also gave me an identity of the authority and power of the state and the global North. As soon as I began fieldwork, targets began to respond to me more or less as they would do to representatives of government or the North; for whatever I eventually get “out there,” irrespective of my identity, could be crucial information for local wellbeing. Again, locals (especially in predominantly non-literate settings) often associate paper and writing to government and organizations based outside local communities. This played out as I requested some participants to sign consent forms. They were uneasy to sign the forms, and those who did sign became less open afterwards. This brought home the point that ethics of social research need not be rigidly followed across diverse contexts. In all, factors such as level of literacy and general perception of the ultimate purpose of research among the target population will affect data validity to different degrees irrespective of the researcher’s identity. Keywords: Control Effect, Data Credibility, Indigenous Ethnography, Informed Consent, Participant Observation*

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The origin of social research was influenced by the physical sciences, which emphasize neutrality and objectivity on the part of the researcher. Early social researchers set off with the impression that such attributes of good science should also be practiced in the human sciences. In recent decades, there have been debates as to how much objectivity is possible for social research because we, as humans, can hardly achieve the sort of distance that physical scientists can maintain from the inanimate objects of their research. For social (especially qualitative) scholarship, many now agree that it is pretentious to claim such levels of neutrality because, as human beings living among others, social researchers cannot be totally impartial and detached observers (Berger, 2015; Giddens, 2009). However, concerns about credibility and trustworthiness of results remain crucial, as has been expounded by several scholars that include Winter (2000), Golafshani (2003), Haralambos, Holborn, and Heald (2015) and Stewart, Gapp, and Harwood (2017).

This paper results from my experience as I did ethnography of the Roll Back Malaria (RBM) initiative as an insider in Nsukka, a locality in southeast Nigeria. Initiated in 1998 through a joint partnership of major international health bodies, the RBM initiative is a transnational intervention targeted at the malarious world. It was launched in Nsukka in 1999. The study was defined by me for a PhD project (Ugwu, 2016) and not the RBM managers. The aim of my project was to explore the attitudes of this local population to the RBM initiative. However, I felt compelled to write this methodological report because fieldwork experience

opened my eyes to something interesting: Members of this local setting naturally assume that social research is linked to the external world, particularly the state and/or the global North. So, it became the case that, although I was born and raised in this locality, my position as a researcher conferred on me (in the eyes of the local population) an identity of the authority and power of the state and the global North. This paper is an attempt to contemplate the merits and challenges of this intersecting identity for the credibility of research results arising from such a context. Vandenberg and Hall (2011) note that when researchers are in positions of power, or when participants sense that disagreement with the dominant power position of the researcher could affect them in some way, they would tend to present only the local sides considered more palatable to such powerful external forces.

Hutchinson (1996), Whyte (2011), and Geissler (2014) have analyzed how local attitudes that connect social research to the external world played out in the different African settings where they worked. This reality has the potential to vitiate credibility of findings, depending on how the researcher handles this challenge. This reality that tends to portray the researcher as connected with powerful external forces will play out in peculiar ways for different categories of researchers working in different settings. Yet the challenges for the local researcher working in a setting where other locals ordinarily link research to external forces (Geissler, 2014; Izugbara, 2000) have yet to be especially articulated.

Being an insider in the area of this study for more than three decades guaranteed me the deep cultural involvement required for understanding the members' social experience from an insider's viewpoint. Interactions and rapport building were easier on account of this. The fieldwork would have been longer and more challenging were I to learn afresh the language and terrain of the area. However, I attempt, in this paper, to highlight important ways in which the challenges that attended my ethnographic encounter— as an insider who came to acquire the status of a symbolic outsider on account of his research – could affect the credibility of results. Where relevant, I draw from experiences that predate the present fieldwork. Some of the challenges I faced as an indigenous ethnographer might be common but their nature, causes and implications have not been particularly highlighted.

### **Context and Summary of Research**

Nsukka is an Igbo local council area lying at the border between northern and southern Nigeria. As the times changed and the area became increasingly drawn into the globalised context, malaria, which the members hitherto perceived and handled as a local problem, became a transnational concern, being vigorously fought through the agencies of the nation state and the international health establishment. It was in this context that the RBM initiative entered their enclave, and it was within this globalized context that the group members constructed the anti-malaria campaign as an embodiment of the external world, and health workers and researchers who show interest in it as some of its immediate representations.

Naturalistic observation (the participating observer option) was my major data collection strategy. To this effect, my membership of this area, where I had lived for more than three decades, was an advantage. Being an indigene for decades guaranteed me the linguistic and cultural competence to operate from an insider's position. There are 18 communities that make up the Nsukka council area from which three – Lejja, Obimo, and Opi – were purposively selected for fieldwork on account of the degree of the anti-malaria campaigns there. My own community, Ihe (an urban part of the council area), was not chosen because it lacked the criterion on which the judgment sampling was based. Communities were chosen either because they had evidence of robust malaria control activities or because they had evidently low level of such activities. There are geographical contiguity, mutually intelligible dialects, and other cultural commonalities that put Nsukka communities into one culture-area. Nonetheless, the

expansive nature of the area makes it impossible for one person (except popular figures like public officers) to be known across the different districts. So, to get entry into the selected districts, I started off training as a malaria intervention worker, working with other intervention workers through whom and with whom I would get into the villages to interact with other locals at the rural level. On this count, many of the locals where I worked initially saw me as a health worker. I managed to keep my researcher-identity, though, by not dressing like the real health workers and by going into the villages alone at other moments, not for health intervention but to ask questions about my research, during which period I would introduce myself to the villagers for who I truly was and what my purpose in their midst truly was. Of course, to the malaria intervention workers, I had, at the outset, explained my mission and had also requested to be trained as one of them to aid my participant observation. Interacting with both the intervenors and their local targets gave me their views about the malaria control initiative; and working with the intervenors in the villages allowed me to cross-check all claims and responses myself. I held focus group discussions and interview sessions with some purposively chosen intervenors and locals; I was particularly interested in choosing malaria intervention workers and local volunteers working in my selected districts. A total of three focus group discussions and 13 interview sessions were held. The study population was comprised of the RBM workers, the local “elite” (chiefs and local volunteer intervention workers), and ordinary locals.

## Experiences and Discussion

### The Native-Researcher as a Symbolic Outsider

“The construction of the status of insider/outsider involves a process of negotiation based on shifting and unstable identities and identifications” (Enguix, 2014, p. 88). I, an indigene of Nsukka, became a symbolic outsider of sorts as soon as I began to show special interest in malaria, asking somewhat formalized questions, taking notes and pictures, and tape-recording interactions. When my interactions with other locals began to be mediated by the kind of social role (researcher) and materials (voice recorder, paper and photography) deemed capable of transmitting outcomes of interactions beyond the immediate environment, participants naturally began to behave and speak to me as though they were before an outsider. Of course, no society can today be described as local in terms of still being in a pristine state. External influences of the present globalized context make it difficult to find locals anywhere whose views about the world are still free of external awareness. Here, this reality caused a suspicion of external provenance of my research in a way that affected how participants thought about what I should report to those external bodies who might have commissioned my study, or who will ultimately be given the results.

*Control effect* on the locals that I worked with manifested in attempts to create on me the impressions they considered needful for potentially beneficent external agencies. Control effect is a well-noted influence of the presence of a fieldworker on subjects (Haralambos, Holborn, & Heald, 2015; Vandenberg & Hall, 2011). It means that subjects who know they are in the presence of a researcher often put up behaviors that create the sorts of impression they want the researcher to take away. Tellingly, informants would remind me of the community’s different needs, willing me not to forget to send the message to “those who have commissioned you to do this.” A young man once requested, “If some opportunity somehow presents itself *there*, you know you have to help get us in.” Another put it: “When you are done and have written your report and handed to *nd’óyibó* (roughly translating to government or the North), who have commissioned you, we hope it brings us good things from *there*; you know those things we need, don’t you?” Aptly, Vandenberg and Hall (2011) have suggested that

participants tend to present only the sides of local issues considered palatable or necessary for outsiders seen as connected with power.

Obviously, villagers in the rural communities in Nsukka observe that it is government health workers (most of whom come from outside their immediate districts) that organise the malaria control activities, including the training of local volunteers for the intervention work. However, the trepidation with which the government health workers themselves announce and prepare for the visits of foreign (usually Western) malaria control managers suggest to the locals that the ultimate drivers of the malaria intervention are indeed somewhere beyond the local, possibly beyond their own government. It was also in such a superior light that I began to be held, even in my own neighborhood, as soon as I started asking questions about malaria in a formal sort of way. Some villagers expressed an expectation that the report of my study was likely to be transmitted beyond their immediate community – to the external world. That external world is ultimately linked to the forces of government; at some distant level, that world is also linked to the global North, here viewed as resource-rich, with some remote promise that interfaces with people in my position could, in some way, bring home some of the visions they have held about that external world. Geissler (2014), a European who worked in the Kisumu region of Kenya, reported how local field assistants in his “global” health research envisioned their involvement in that process as an opportunity for international inclusion, motivated by local visions of modernization and development, and a quest to connect with the world “out there.” Hutchinson (1996) had also observed how her Nuer subjects (in South Sudan) saw her field notes as capable of transmitting their intimate interactions to a certain entity that was remote and powerful.

This disposition on the part of research participants might vitiate credibility of information emerging from field interactions. So, this study – with naturalistic observation being the primary data collection strategy – had the potential to keep in check the sorts of “impressive” information that subjects would want the researcher to report. Instances that showed that the interested outsider (whom I became by virtue of my research) was only often told what should impress them played out on a couple of occasions in the course of my fieldwork. For example, the malaria health extension workers initially told me that they dispensed malaria therapies to only those tested and confirmed to be positive to the disease. In any case, on occasions that I was part of a test team (of course after having established rapport and undergone training as a volunteer health worker), therapies were given both to persons who tested positive and negative. Persons who tested negative to the disease were told to also take the drugs (usually the Artemisinin-based Combination Therapies [ACT]) “for preventive purposes.” Again, local intervention volunteers would easily tell me – as they also do government and Western visitors – of their willingness to undertake malaria control activities. However, it had to take prospects of some reward to get most of them to attend our malaria related discussions (more on this below). Analyzing the context and work conditions that motivate this conduct among the health workers and their local volunteers will be straying from the focus of this paper.

### **Seeking Written Consent**

Informed consent is now a cardinal ethical requirement in studies that focus on human beings. The need to show the research community that such an ethical observance was kept in the course of the present fieldwork would have been served had I obtained signed consent documents from participants. However, it was my conviction that – especially where it would not be harmful to do so – such a need could be sacrificed if insisting on it could hamper data quality. In Nsukka, I observed that the thought of “being written” (Whyte, 2011, p. 29), of being captured in writing – especially in a way participants suspected could get to government

authorities – was a disturbing thought to conceive. I originally set out with a plan to obtain written consent from participants before proceeding with interviews and focus group sessions. I first tried it out with the Nsukka central malaria focal person (the first respondent that I interviewed) and it took about five minutes to get him convinced that there was no risk in signing the consent form. And, as the interview set off, his mannerism began to play out in a way that suggested to me he was not at ease giving me the sorts of information on which he would be uncomfortable to be quoted. One of my informants would confess in one of our informal chats that what she had signed on the consent form before the interview session held earlier with her was not her real signature. Another informant decided to take home the consent form to study it closely with her family before she could append her signature. This was to, according to her, “avoid getting into unnecessary government trouble.” The form was never returned, though. She would come up with excuses each time I brought up the issue. Getting a sense that she was uneasy to sign the form and yet did not want to offend me, I let the matter go. I was jolted in a positive way when a member of a chief’s cabinet in one of the selected districts told me in a surprisingly blunt way, “I think people will tell you less than they would have if you don’t stop asking them to sign a paper.” Convinced that going ahead with seeking written consent could negatively affect trustworthiness of information that respondents would be willing to offer, I concluded that abandoning the written consent was a choice I was condemned to make. The internal review board moderating the study, after having been presented with this challenge, agreed to let me proceed without the written consent. In this condition, the purpose of the study would then be explained to participants, and their oral consent obtained before the session would proceed. Whyte (2011), who worked in eastern Uganda, made a related observation that from the colonial times, paper and writing had been associated with relations to government and organizations based outside local communities. She observed that, among members of her study group, both blood and paper were held to mediate relations to inscrutable powers: “the blood of sacrifices to the spirit world and paper to the forces of government.” She echoed Hutchinson (1996) who wrote that, “like ‘blood,’ ‘paper’ is capable of spanning, whether as metaphor or medium, the experiential extremes between social intimacy and social distance” (p. 29).

It needs to be said that the decision to exclude the written consent was likely to have enhanced quality of responses because, as I noticed, respondents felt freer to offer their take on issues if they did not feel the need to choose their words carefully on account of having committed themselves on paper. This makes the point that ethos of social research need not be rigidly followed across diverse contexts. Social researchers – especially outside Western societies where much of the ethical requirements emanate – might need to let local peculiarities guide their adjustments to field realities. Factors such as level of literacy and general perception of research as state-mediated (which was the case in my locus) might vitiate trustworthiness of results should the researcher insist on written consent before proceeding with interview or focus group sessions. Certain people in certain sections of the world, such as the largely non-literate area where I worked, are not comfortable to commit themselves in writing (for such purposes as research).

### **Organizing Interviews and Focus Groups**

In Nsukka, as in many African communities, research is naturally regarded as state-mediated, with some remote connection to the global North (cf. Geissler, 2014; Hutchinson, 1996; Schumaker, 2011; White, 2011; Whyte, 2011). Those two entities are seen to embody power and, more importantly, material abundance. In Nsukka, a person undertaking projects connected with those entities is expected to be abundantly paid. This impression is further bolstered by promises of dividends of research that fieldworkers make to the locals. Social



researchers mine a number of the districts in southeast Nigeria for data. Because these people, like in many other places, might not always be disposed to responding to long lists of research inquiries, a number of the fieldworkers then feel compelled to make promises of possible dividends of their research. They mention hospitals, water and such other “development” projects. This is to induce willingness of their research targets to volunteer information. As these promises are continually made – with such provisions still a long way coming – many “experienced” respondents would prefer to avoid being “fooled” for the umpteenth time; they would choose to have their own share of the dividend of research on hand, no matter how small. Researchers working in southeastern Nigeria, such as Izugbara (2000) and Ezeh (2003), have reported about this challenge. However, its background and implications for credibility of findings, especially from the position of an indigenous researcher, are what I attempt to articulate in this paper.

One challenge I had to deal with in scheduling interviews and focus group sessions stemmed from the fact that in the districts that I was visiting, the mobile network signals were either poor or non-existent. It, nonetheless, took visits, sometimes many of them, and not just phone calls, to accomplish one interview session. The scenario is better imagined for the focus groups. My major challenge, though, stemmed not from poor network signal but from the attitudes of my targets to social research. Given what I went through arranging for interviews, I would ask my key informants to suggest how focus group sessions could be arranged. The key informant in one of the selected districts advised that we ask the leader of the local intervention volunteers to call his other members for a meeting. However, she forewarned that the community members hardly came for such meetings unless they were expecting “kola” (some refreshment) to be offered. The first step taken to get around this challenge was to wait for a day the committee would hold one of its routine meetings and then use the opportunity to hold a focus group session with them. As it turned out, only four out of the 12 members attended the next meeting they had slated; and that number was not enough for a focus group session. The following meeting was even more disappointing; only the leader and the public relations officer were in attendance. We were then left with the choice of asking the chairman to arrange “a special meeting” in which the focus group session would be held. While requesting the health committee chairman to set up such a meeting, I, at first, decided not to promise that kola would be provided. After our third disappointment, we had to give some money to the health committee chairperson to procure “kola” himself. We made that move in order to get the chairperson to assure other local volunteers that kola was already on the ground. Indeed, our next appointment was well honored.

The kola nut (*Cola acuminata*) has great symbolic value in the social life of the Nsukka and the Igbo culture area in general. Deliberation in any public occasion would not begin until the commensality of kola nut has been enacted. The nut is split, and, with one clove held out in the right hand, supplications are made (usually by the host or the eldest in the gathering) to the land and the forebears. Indeed, the kola nut is a concomitant of, usually formal, meetings. When the kola nut itself is not available, the host may offer any other consumables, explaining why kola nut could not be provided and pleading that guests make do with what has been presented in its stead. In that manner, people may offer food, drinks and any other usable items (sometimes even money) in the place of kola nut. Among the Igbo, it is a social norm to expect some form of “kola” from the host when one has been summoned for a meeting. But sometimes, this may need to be mentioned to reassure an invitee. However, when kola is mentioned ahead of any meeting, it invokes the expectation that it would be something other than the kola nut itself. This is because kola is so normal in meetings that its availability ordinarily needs no mention.

When I asked a health committee chairman in another district to arrange a meeting with his members for a focus group session, I had to remonstrate for a while as he hesitated and

chewed on his words. A firm promise only came when I offered that kola would be provided. This was done to save precious time because I sensed, given the way things were playing out, that we would eventually get to that. On the appointed day, I went with a crate of lager beer (containing twelve 75cl bottles) and the session actually held in that first attempt, with nine out of the 12 members present.

A vitiation of the credibility of responses could be one of the implications of how this challenge was handled, given that participants would be eager to impress me – by saying what they felt I would be happy to hear – as reward for having provided some refreshment. However, I was torn between holding the focus group sessions under that condition and the threat of not holding it at all, and I considered it better to hold the session in that circumstance than not to hold it at all. To curtail the excitement that might lead participants to desire to impress me, I ensured that refreshments happened only after the sessions. Also, the robust discussion guide used in the sessions made it possible to moderate the discussions and to ensure verifications through probes and focus on the subject of the study; and this was in addition to my naturalistic observation method.

It might not be possible to determine the extent to which the provision of “kola” influenced responses, but it was also possible to readily double-check oral information through observation. One instance of this was that the local volunteers would easily tell me of their willingness to undertake malaria control activities – but it had to take prospects of “kola” to get most of them to attend our malaria related discussions. Besides the explanation given above about research fatigue and demand to have the dividend of research on hand on the part of local targets, there is also the local framing of malaria among the Nsukka that did not reckon the disease as deserving of all the fuss that the RBM programme managers were making about it. This perspective to the local attitude towards malaria around here is expounded elsewhere (Ugwu, forthcoming).

### **Concluding Summary**

Hutchinson (1996), Whyte (2011), and Geissler (2014), working in different African settings, have reported how local attitudes that connect social research to the external world played out in their respective study areas. What separates their experiences from mine, though, is that they all worked as “outsiders” in their research sites, although all using long-term participant observation. In the present study area (where I was raised) I also noticed that my position as a researcher conferred an identity of the authority and power of the state and the global North on me as well. Locals see those two entities as embodying power and material abundance. Control effect on the locals that I worked with manifested in attempts to give me the impression they considered needful for potentially beneficent external agencies. In literature suggesting that the researcher’s position confers power and dominance, which affects the trustworthiness of what participants say or do (Enguix, 2014; Geissler, 2014; Haralambos, Holborn, & Heald, 2015; Izugbara, 2000; Vandenberg & Hall, 2011), it is not particularly noted whether this also applies to the indigenous researcher. This present report makes that disclosure. Although an indigene of Nsukka, I became an outsider of sorts as soon as I began to show special interest in malaria in a way that suggested to local targets that the results of my investigation could eventually go out to the government or the global North. Members of the local population began to respond to me more or less as they would do to representatives of government or the North; for whatever I get “out there,” irrespective of my insider status, could be crucial information for local wellbeing.

Whyte (2011) has suggested that locals (especially in predominantly non-literate settings) often associate paper and writing to government and organizations based outside local communities. This figured not only as a result of my note-taking but even more seriously

because of my request for some of the participants to sign consent forms. My challenges were captured by an interviewee who jolted me with the following words: “I think people will tell you less than they would have if you don’t stop asking them to sign a paper.” This brought home the point that ethos of social research need not be rigidly followed across diverse contexts.

All in all, experiences of the insider as they affect credibility of research results are likely to play out differently in different conditions. Factors such as level of literacy and general perception of the ultimate purpose of research among the target population will, to different extents, mediate the validity of field data. In my case, I designed the research to make naturalistic observation the primary data collection method. This enabled me to see beyond the impressions that some participants would want me to have. Yet, as human beings living among others, social researchers cannot be totally impartial and detached (Atkinson et al., 2009), more so for researchers, like me, studying their own social location. Since total objectivity is, by definition, a myth, Bernard (2006) suggests that the indigenous researcher worry more about producing credible data and strong analysis and less about whether being local is good or bad for social research. This suggestion resonates with the experience that I have presented in this paper.

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### Author Note

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