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Ghassan Hage

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A not so multi-sited ethnography of a not so imagined community

Ghassan Hage

Department of Anthropology, University of Sydney, Australia

Abstract

This article reflects on key analytical concepts used in the anthropology of migration in the light of the author’s own ethnographic work on Lebanese migrants in a number of international locations. It first examines the notion of multi-sited ethnography and argues that in the study of migrants sharing a unifying culture across a number of global locations, multi-sitedness is less helpful than a notion of a single geographically discontinuous site. The article also examines the excessive usage of the notion of ‘imagined community’ in diasporic research. It argues that there is often very little empirical evidence of ‘community’ presented in the literature that uses the concept. Finally, the article examines the uncritical assumption often made that the study of migration is the study of ‘mobility’. It argues that migrants do not really spend that much time ‘moving’ in the sense assumed by the notion of ‘mobility’.

Key Words

Lebanese diaspora • migration • mobility • multi-sited ethnography • transnational community

INTRODUCTION

I have recently started writing in book form the result of four years of ethnographic research on the Lebanese diaspora. As always on such occasions, I began to reflect more systematically on some of the methodological and theoretical issues I encountered throughout my research, especially that this was a relatively new area of research for me. Like many who embark on such research, I was faced with various new and fashionable concepts from the latest theoretical and ethnographic writings in the field. In this article, I want to go through some of these concepts, particularly those that were useful in starting me thinking about my project but which became less useful as my inquiry evolved and as their capacity to account for my research practice and the empirical reality I was examining declined.

My research involved a study of migration from two Lebanese villages: one in the
north and one in the central Lebanese mountains. Both are Maronite Catholic villages. The one in the north (village N) is far more underdeveloped than the one in the centre (village C). Village N is still shaped by agricultural practices, even though many of the villagers have jobs (from workers to professionals to shop owners) outside the village. It is nevertheless not as integrated in the cycle of Lebanese capitalism (a capitalism based on banking, services and tourism) as village C is. The latter is situated on the road towards and just underneath a ski resort. So, it has always been exposed to the Lebanese middle and upper classes going skiing and has developed an economy aimed at servicing the skiers. It was this exposure to middle class lifestyles that primarily interested me about the second village: how does sub-national geographic and socio-economic location shape people’s aspirations and how does it influence migration patterns?

Both villages, like all Lebanese villages, are net exporters of migrants. The northern village has people spread out mainly in Caracas and Barquisimeto in Venezuela, Boston and Texas, Sydney and Melbourne and a small number in São Paulo. The central village has people in the Arab Gulf, in Paris and London, Philadelphia and Vermont, and in Gatineau in Canada.

In the first year of the research I spent a couple of months in each of these two villages in Lebanon, learning about their socio-economic and cultural composition, and most importantly about their global patterns of migration and the way migrants made themselves felt in the everyday culture of the village. I talked to villagers about their migrant relatives and their own aspirations and watched them debate the pros and cons of their own possible migration, or, for some, their past migration. I then did my first world tour and visited all the main places in the world where the villagers were concentrated, spending an average of a week in each place. In all these places, my social connections were the relatives of people I became particularly close to during my fieldwork in the villages in Lebanon. After some negotiations and the explorations of various possibilities, I chose one extended family from each village (one that has members living in as many of the village’s international centres of migration as possible and who were willing and able to have me live with them). In the four years (mid-2000 to mid-2004) I basically spent my research time flying, mainly between Lebanon, London, Paris and Philadelphia in the case of the central village family and Lebanon, Barquisimeto, Boston and Melbourne in the case of the northern village family. I lived in one of the families’ households in each of these locations, sometimes for one or two weeks but sometimes a whole month. These households were the main locations from which I conducted my ethnography.

ON MULTI-SITED ETHNOGRAPHIES
The first issue I want to deal with here is the issue of multi-sited ethnography. This is an increasingly used buzzword in ethnographies of migration as well as in other areas. I say buzzword because I don't feel that its signification and ramifications are explored by many of its users. As is often the case with the introduction of new concepts, while there is some nuance in the work of the early advocates of multi-sited ethnography, those who apply the concept after them increasingly use it mechanically. It is enough to be doing an ethnography that involves flying between two or three locations for people to call it multi-sited ethnography. This is especially the case with the anthropology of migration. We are told that now that migrants do not simply move between two
locations but circulate between many, now that we are through with return migration and yo-yo migration we are in the middle of zigzag migration. Apparently, we also need a new way of keeping track of people's movement. This is why we need multi-sited ethnography. More generally, in an increasingly globalized and networked world, where culture has been ‘de-territorialized’ and where we are dealing with flows, the idea of such an ethnographic approach is a must. Roger Rouse's work on Mexican migrants in the US is perceived as exemplifying such an approach, even though it was written before George Marcus's seminal texts on multi-sitedness (Rouse, 1991).

The reader might think that having spent a good deal of my time in the last four years zooming between eight different locations around the globe, I cannot but be all for multi-sited ethnography. The fact is, I am not. I did start my research thinking of myself as doing multi-sited ethnography. Now, I have to say that I simply find the idea not practically feasible. That is, it is not a question of being for or against it. It is more a question of whether there can be such a thing, and I simply do not think that there can be such a thing as a multi-sited ethnography.

My first couple of trips around the world, moving from one geographical area to another and staying with the families I was working with, went fine and I could have felt that it was fine to engage in a multi-sited ethnography if it were not for a simple problem. I was constantly suffering from jet lag. Multi-sited ethnography was unhealthy, especially for (most) people who have teaching and families to go back to and therefore cannot take all the time they might wish to take. They need to cover the various sites in a limited amount of time. The body of the anthropologist, even a post-modern one, simply cannot cope with such fast and intensive travelling for a very lengthy period of time. I thought it amusing, as this fact dawned on me, that none of the people who have written about migration and multi-sited ethnography have mentioned something as mundane as the problem of jet lag. I still wonder how they practically managed this multi-sited ethnographic research themselves.

Perhaps someone younger than me could have coped physically with this, also someone with no family to rush to, and unequal child rearing labour to negotiate with their partner and make up for. But even if this were the only issue, I think that there was an even more serious problem that simply made multi-sited ethnography an untenable proposition. In my first couple of trips around the world, I found it relatively easy to stop in France, meet the family, leave for London, meet the other part of the family, leave for Boston and so on. But after a while the issue of landing and leaving became a far more difficult affair. It was not so easy to just land and leave as if I were floating above the cultures I was researching; people's problems, my own relation to them, people's expectations of me, my expectations of them, the questions I was asking, the social relations I was becoming aware of, all of these things changed and complexified the site. As they say, it was getting thicker. Increasingly, it was simply becoming impossible to do what I was doing at first: just hop around. In many ways thick ethnography is not a matter of choice but a function of one's degree of immersion. After a period of becoming more immersed in certain social relations, they force you to be either a thick ethnographer or no ethnographer at all. It was not ethnography but the relation to the field itself which was getting thicker. Thicker and stickier, I might add: as I became more involved I became more subjected to the gravitational forces that pull you into a social field (Bourdieu, 1993) and make it very hard for you to remove yourself from it.
I am interviewing X with Y and Z sitting around. X says something that I know Z feels strongly about. Z knows that I know she feels strongly about it. So the feelings generated by the interview and the analytical labour it requires is now much more complex and more demanding. Now each landing in the field and each departure was a major affair and the time between them was less and less touristy and more and more socially, psychologically and analytically demanding and exhausting. It was clear that one site, if it is to be done thoroughly, was already an exhausting enterprise. How can one study numerous sites in such an involved way? I was actually facing a choice which, in fact, made the whole issue quite clear to me. I simply could not be involved in such an intimate way in more than two sites, at the most. If I was committed to study a transnational family or village as a global phenomenon, then I could not treat all the locations in which each one of their members existed as a separate site. I had to treat all these locations, dispersed as they were, as just one site. But if I am treating the whole family with its various locations around the globe as one site, is this really a multi-sited ethnography? Perhaps it is so, but this would be at the expense of making light of the meaning of an anthropological site. If we are to maintain a concept of the site as something one has to spend an inordinate amount of time and labour on in order to become familiar with, then I was not studying a multi-sited reality. I was studying one site: the site occupied by the transnational family. It was a globally spread, geographically non-contiguous site, but it was nevertheless one site. It was still physically demanding to study such a global site, but at least this was a more realistic way of defining my research object.

To put it more generally, when we are studying a culture X, whose members are located in geographically non-contiguous spaces A, B, C and D, we simply cannot physically achieve a multi-sited ethnography of the web of social relations in which X is grounded in A, B, C and D. We can, however, do an ethnography of the relation between the sub-cultural group ‘X1’ in location A, the sub-cultural group ‘X2’ in location B, ‘X3’ in C and ‘X4’ in D. That is, we need to treat the space where the ensemble of social relations between X1, X2, X3 and X4 is constituted as a site E with its own specificity. But, as a result, we have to live with the fact that our knowledge of X1 as it is grounded in A and X2 as it is grounded in B, and so on has to suffer. It is not an either/or matter but a question of emphasis dictated by what is humanly and practically feasible. No ethnography can capture all social relations in any case, but a certain reflexivity concerning the social relations that one is opting not to cover in depth cannot but be beneficial in allowing a better definition of the limits and limitations of one’s research.

One of the recurring criticisms of past anthropologists is that they treated realities as bounded without investigating movement and flow: that is, they studied X1’s relation to A, for example, without studying X1’s relation to X2. That is true, but I guess what I am saying is that this criticism does not amount to much in practical terms. The early anthropologists could not have done otherwise and neither can we. One can see for instance in the work of Gulick, the early anthropologist of the Lebanese village, a clear awareness of the fact that much of the land in Lebanese villages is owned by people who live in international locations, but that does not stop him from analysing ‘the Lebanese village’ as a bounded entity (Gulick, 1953).

As far as my own research was concerned, as I faced the choice of either doing an ethnography of X1 in relation to A or X2 in relation to B and so on or to do an
ethnography of the relation between $X_1$, $X_2$, $X_3$ and $X_4$, I decided to do the latter. I took my site to be the geographically non-contiguous space where a specific transnational familial culture with its enduring social relations was flourishing. To do so is to make a choice of emphasizing those global relations and the circulation of goods, communication, money, people and emotions that occurs within them. Or, to put it differently and perhaps a bit more simply, it is a choice of emphasizing the migrants’ transnational culture at the expense of their settlement culture, even if, as is clearly the case, one cannot be understood separately from the other.

When conceiving of it in the way I have here, this transnational research highlights another claim that is often made by those who affirm to be engaging in multi-sited ethnography. It is asserted that those who do multi-sited ethnography are engaging in a radical break with the traditional conception of ethnography where both the ethnographer and those who are being studied stay in one place. I have to admit that I always felt when doing my research that I was incredibly innovative and people always reinforced my belief when I presented seminar papers. Then, at one time, as I was looking at a map of the global circulation of communication between the various familial households that I had drawn, another map of circulation between geographically non-contiguous spaces flashed before my eyes. It was the map of the Kula ring. It was a rather interesting ‘revelation’: do they ever think of the Kula, all these anthropological innovators of the global and the multi-sited who rush into trying to be ‘ever so original’ by showing how limited the disciplinary ancestors have been? Was Malinowski not a ‘multi-sited’ ethnographer when he dealt with the Kula, if all that is meant by multi-sitedness is this circulation between geographically non-contiguous spaces? Was he not an ethnographer of movement rather than stillness? As is often the case, one finds that the elders of a discipline have done a great deal more than what the ‘innovators’ like to give them credit for in order to appear as avant-garde. Ira Bashkow has also recently noted that the anthropologists who take the notion of flow as an innovation on a static conception of culture fail to acknowledge how much this idea is already present in the very basic and foundational notion of ‘diffusion’ in Boasian anthropology (Bashkow, 2004). For my part, maybe I will call my research a ‘neo-Kulan ethnography’. I thought this acknowledgement of the disciplinary roots of my approach was a most suitable name for a research that has also ended up emphasizing the migrant’s needs for roots and a criticism of the whole rhizomatic, always on a ‘route’ to nowhere, approach to the diasporic condition.

**TRANSNATIONAL COMMUNITIES**

‘Transnational communities’ was another term which began by being useful when I started my research, though I ended up relating to it critically once my research on the culture of the transnational family was under way. Here is a concept that is truly flourishing in the study of the diaspora. It seems that every diaspora is a community whether it likes it or not. It has been used in all kinds of contexts including by a number of anthropologists in my own field, working on Lebanese villages spread around the world, and where the concept becomes an extension of the idea of the ‘global village’ (Nabti, 1992). The thing that struck me when I began to globally locate the migrants from each of the villages I was researching is that there was no necessary strong sense of transnational community among them. Villagers certainly had a sense of belonging to a village whose...
members are globally spread around the world. But to say that the villagers formed a global community and that they had a communal sense of belonging to a transnational community of villagers was a bit stretching it in most cases. So, I went back to the studies that talk about transnational communities. What sort of evidence was produced there that indicated that the members of a village diaspora constituted a community? Very little, I have to say. I was comforted by Stephen Castles (2002) and Ralph Grillo’s (2001) recent warnings against the facile usage of this concept. It seems that the fact that some people originate from the same village or the same nation and are spread around the globe is enough to make them a transnational community. What’s more, the lack of evidence of empirical connectedness did not appear to bother the researchers concerned. There seemed to be no need to show that any specific enduring social relations between the villagers existed for them to constitute a community. Why? Because apparently these villagers constituted an – and here is a really wildly circulating concept at the moment – ‘imagined community’. It is truly striking to note how much of a ‘passe partout’ the notion of ‘imagined community’ has become in the field of ‘diasporic studies’. This is not to take anything away from Anderson’s originality when he himself deployed the concept (Anderson, 1983). It just seems that today it has become an excuse for not doing any empirical work on trying to establish that a group of people form a community. When a person presenting a paper on a diasporic community is asked what evidence there is to show that the diasporic group they are studying is a community, they give you a superior look and inform you that ‘it doesn’t work this way’ because the community they are studying is an ‘imagined community’. Here ‘imagined community’ seems to have very little community in it and a lot of imagination instead, usually the imagination of the researcher.

So I am pleased to let the readers know that the extended transnational families I studied were not ‘imagined’ in such a facile way. They are ‘concrete’ familial communities with a definite analysable ensemble of social relations that constitutes them. In fact, these families were not even ‘imagined’ in Benedict Anderson’s proper sense either, in that each member did have an empirical sense of the various other members that belonged to the family. As an aside, I must say that I have enjoyed and I really recommend the study of such not so imagined communities when analysing diasporic relations. They are both analytically and psychologically good for you as a researcher; a good deal better, and far more socially useful, than the study of imagined but non-existing communities.

What I try to show in my work is that such familial communities are a source of ambivalent attachment. They constitute a point of attachment, in the way one is attached to a nourishing source. That is, they constitute a person’s roots. At the same time, these families are networks of global routes, defining a field of potential social, emotional and physical mobility for their members. As a network of routes they tell us about where people can travel. As a network of roots they tell us about the psycho-social dimensions of how people travel: for example, do they travel feeling secure or brittle and to what extent? This brings us to the issue of mobility and its relation to the field of migration studies. Here again is another key assumption that I ended up relating to critically. Why is it taken for granted that mobility is important in the study of diasporic cultures? This has led me to another important question: what kind of mobility is foundational in shaping the diasporic condition?
DIASPORA, MIGRATION AND TRANSMATIONAL MOBILITY

Jonathan Friedman, in a very refreshing critique of the transnationalist vulgate, points out that ‘(t)he focus on movement seems to have overlooked some pretty serious facts about the world’ (2002: 33). He points out that ‘less than 2 per cent of the world’s population is on the move, internationally’ and that ‘the discourse (on the primacy of movement in the world) assumes, without any research to support it, that the whole world is on the move’ (p. 33). Though I strongly sympathize with this observation, this does not tell us about the significance of movement for people who are actually considered to be on the move. This is especially so in the case of studying a particular diaspora or a migrant culture. The fact that a migrant has moved or continues to move every now and then is of course obvious. But the significance of this movement in their lives is not clear. Clearly this is not a ‘how much of their time have they spent engaging in transnational movement?’-type question. On the whole, despite the arguments about people now being increasingly transnationally mobile and so on, most migrants move once or twice in their lives. Empirically, that is, they spend a day or two doing that kind of mobility we call migration. Is it not strange that people become associated with something they have done in one or two days for the whole of their lives? Of course, this invites us to think that the reason that people do become associated with such movement is because of its significance in shaping their lives. It is a movement that marks them in a way that the ordinary everyday movement people engage in when they move around does not.

This raises the issue of what is this ‘significant movement’, this movement that marks one for life. After all, no one in the world just sits still for a very long period of time. People are moving all the time. People move from inside their houses to the outside and then back again. People go shopping, go for a stroll, move to go to work every morning and so on. Even when they are at home people move all the time, from room to room, within a room; even when in bed people are known to move. Now, there is among all these movements a particular cluster of them that we associate with migration: these are the movements which mark and define the diasporic condition. From my attempts at reflecting on them, it seems to me that these movements are not easy to capture either empirically or analytically. Instead, they are taken for granted. I do not think many have taken the time to try and define them. Often people opt for the most facile way to do so: they are simply associated in a common-sense way with a movement across nations resulting in relatively long-term settlement. But this is clearly not a satisfactory definition.

It is a mistake to think that if people move across national borders, such a movement will necessarily be the most significant and defining element in their lives. Certainly not when we are talking about tourism, of course. But this can also be the case even when this involves a relatively long period of transnational geographic resettlement. It is well known that in the history of capitalism, internal, particularly rural–urban migration, can have a far more dislocating and alienating effect on families and people than international migration. As Jonathan Parry (2003) noted in his analysis of internal rural Indian immigrants: ‘long distance migration is not only a matter of miles. Most migrants are conscious of having also traveled a long way in attitudes, outlook and style of life’. But there are many people who move across international borders but do not experience this kind of ‘travel in attitude’ that Parry mentions. Indeed there are many people who do not experience their international movement either as a form of cultural dislocation or as migration.
I personally left Lebanon to study in Australia without ever thinking of myself as ‘migrating’. I saw myself as simply studying in Australia. Then I just stayed there, never really thinking of myself as having migrated. So, is migration a subjective or objective definition of movement? In a somewhat different way, the Lebanese merchants and bankers I have talked to in my research never use the concept of migration. They say I am ‘living’ in New York, I am living in Paris, or, ‘where were you living during the war?’ Even if they have been ‘living’ in Paris for 10 or 20 years, they do not say ‘we’ve migrated to Paris’. This is what Jonathan Friedman calls ‘the small worlds of cosmopolitanism’ (2004: 189). For the international cosmopolitan class, equipped with the best passports that money can buy, and a *habitus* that allows them to feel at home in most international cities, the world is their turf. Moving across national borders is no more significant than moving within one’s own town or even one’s own house. They are indeed ‘at home in the world’, as far as this dimension of international movement is concerned. This should make clear that the significance of crossing borders is not some objective experience that defines international migration regardless of who is crossing the borders.

To sense that one’s movement across international borders is significant it has to involve a sense of being uprooted from things that one is familiar with and a sense of being out of place. But here again, one should also be careful not to think that just because we feel we are crossing international borders, the change from one national culture to another is the most significant aspect of our move. For example, I have done some extensive interviews with a man in Boston, who felt that the most significant aspect of his migration was living with his maternal uncle and marrying his daughter. This man experienced his migration as a matrilocalism, which made him feel disempowered and alienated from his paternal filiation, and ended up threatening the viability of the way he had defined his own masculinity to himself. For him this matrilocal move was more important than the move from Lebanese village culture to Boston culture. He said to me: ‘I feel stuck here with my maternal uncle’s family. They even control my kids’. It seemed to me that there was a lot of misery condensed in this ‘I feel stuck’, for it is precisely what people give as a reason to migrate. One migrates because one feels stuck, not in order to feel stuck. This brings us to another critical dimension of the emphasis on mobility.

**MIGRATION AND EXISTENTIAL MOBILITY**

Perhaps one of the most important points I have come to develop in my attempt to define the nature of this ‘significant movement’ we call migration is related to the notion of existential movement. The importance of this ‘existential movement’ was brought home to me by the discourse of the migrants themselves and the discourse of those planning to migrate. Migrants and would-be migrants, like everyone else in the world, like to feel that ‘they are going places’, and they prefer to be ‘going places’ by staying where they are in environments with which they are familiar. It is only when they are ‘going nowhere’ or ‘too slowly’ in such familiar environments that they start thinking of physical mobility. We do not engage in existential mobility in order to experience physical mobility. The contrary is true: we engage in the kind of physical mobility that defines us as migrants because we feel another geographical space is a better launching pad for our existential selves. We move physically so we can feel that we are existentially on the move again or at least moving better. I believe that the movement we call migration cannot be understood without taking into account this relationship between
existential and physical movement. What’s more, such a relationship allows us to construct a whole social physics of socio-existential mobility, explaining different kinds of mobility rather than homogenizing them with one term that equates the travel of the totally-at-home-having-fun tourist and the travel of the fragile, dislocated and hesitant refugee.

This dawned on me on one of my trips to Boston in late 2002. Here are my edited ethnographic notes:

I asked Elie, a member of the family I was working with, the obligatory question of whether he ever wishes to go back to Lebanon. He had just taken me to see his new restaurant. He and his brothers own a chain of Lebanese food outlets in the city and are doing economically very well. He was driving us in his ‘toy’, a monstrously oversized and yet magnificent black, gadget-full four-wheel drive utility truck. He said that emotionally he loved to go back to the village but, he quickly added, there was no point in going back for ‘life in Lebanon had no taste’, *bala tāmeh*. He continued by asking rhetorically ‘what do you want me to go back there for? In Lebanon one runs and runs and stays exactly where one has started from (*whahad byerkud*, *byerkud*, *w’ byib’a mattrahu*.) ‘Here’, he continued, ‘you work and you have a sense of going somewhere.’ As he said this, his ute was burning the highway at more than 140km/hr which gave a further intensity to his words ‘going somewhere’.

The Lebanese are often said to think with their stomachs, and to say that life has ‘a taste’ means that life has a meaning and a purpose. *Hayeht bala tāmeh*, a tasteless life, means a meaningless and purposeless life. And as Elie made it clear, one experiences this meaningfulness and purposefulness best when one has a sense of ‘going somewhere’. In Lebanese, as in the English ‘how are you going?’ and in many other languages, the word used to denote ‘functioning well’ is the same used to mean ‘moving well’. In colloquial Lebanese, to say ‘I am well’ you say ‘*mehcheh’l haal*’, which literally means ‘the state of my being is walking’.

Needless to say, in the phrases just given, words such as ‘walking’, ‘going somewhere’, ‘running’ and so on are not meant in the sense of physical movement. They denote a close association between the viability of life and a sense of existential mobility. Indeed, listening to people’s own evaluation of what makes them migrate, one feels that there is an inverse relation between migration, this all-important physical mobility, and existential mobility. Migratory physical mobility is only contemplated when people experience a crisis in their sense of existential mobility. Or, to put it differently, it is when people feel that they are existentially ‘going too slowly’ or ‘going nowhere’, that is, that they are somehow ‘stuck’ on the ‘highway of life’, that they begin contemplating the necessity of physically ‘going somewhere’.

Of course, this sense of movement that gives meaningfulness and purposefulness to life is not directionless. The positive sense of ‘going somewhere’ certainly does not include the sense of ‘going backward’. Viability is associated with a forward movement in time. This is why it is more often than not associated with conceptions of ‘the future’ rather than the present or the past. Indeed, one of the most pervasive discourses of viability linked to migration is constructed around the theme of the future. What makes life ‘not so good’ in Lebanon is that ‘there is no future here’. People say ‘we’re looking for a better future’.
Clearly, there is nothing distinctly Lebanese in this, except perhaps the strength of the emphasis. The word the Lebanese use to signify staying where one has started, matrahu, means a place. But it also means a dump, where one is thrown as a reject. However, there is an even more ‘damning’ meaning. A matrah is a place where the foetus is thrown after a miscarriage. To stay where one has started symbolizes a miscarried social life, a potential social life condemned to remain unrealized.

Whether it is capitalism that makes us conceive of our human viability in terms of ‘upward social mobility’ or whether ‘upward social mobility’ is simply the capitalist version of a more universal trait that characterizes us as human beings is itself an interesting question of course. It should be noted that the latter explanation is consistent with Spinoza’s notion of ‘joy’. To seek joy is to constantly seek a higher and higher state of agency, control and social efficacy, what Spinoza calls to move towards a higher ‘perfection’. It is important to stress here that, for Spinoza, joy is not a high state of happiness, it is the very movement from one state to another higher state. Joy is the buzz one experiences in the process of ‘moving existentially’.

It is interesting that emphasizing the importance of existential mobility always seems, at least to me, like stating the obvious. Yet it always seems so a posteriori. For despite its obviousness, this mobile conception of the viable self and its analytical consequences is nowhere near emphasized or analytically exploited enough for its explanatory potential in migration and diasporic studies.

Let me include another section of my ethnographic text here to emphasize this point:

I am in Puerto la Cruz. The information booklet I have indicates that here the weather is warm all year long. This is supposed to be one of Venezuela’s main tourist centers. It is the point from which Venezuelans take boats and ferries to Isla Marguerita (a prime touristic destination – it is so not because of the beauty of the island but because it is a ‘duty free zone’). Still it is the Caribbean and it is a tourist zone, so I was imagining the good life: my chance to fuse work with a nice holiday. . . I was looking for a Lebanese man who had recently arrived here from the village to live with his uncle. There is a relatively large Lebanese community here according to my informant in Caracas.

Unfortunately, the part of Puerto la Cruz where I landed was not attractive in the touristy way I was expecting; nor any other way to be quite honest. In fact, for an outsider like me the place is easily experienced as an irredeemably depressed and depressing place.

Like everywhere in Venezuela, the main roads to Puerto la Cruz are exceptionally well maintained. The oil companies need good roads. What’s more, bitumen being an oil by-product, the companies get to sell it to the government. But inside the town where the oil companies’ trucks do not pass, the roads are not always as good, and the footpaths are in need of maintenance. I was driving through the night, and when I arrived early in the morning, kids in their early teens were asleep on those cracked footpaths here and there. Many had some kind of plastic container next to them. They seemed to have been sniffing petrol.

The worst thing, however, was a smell, a very bad smell, which every now and then invaded the place . . .

I keep trying to convince myself that this is a tourist town. There must be more
in it for people coming to have clean fun in the sun. The shopping area doesn't help. Shops are dirty, all selling cheap plastic things of all shapes and the clothes are all made with this shiny synthetic material in which all of Latin America is drowning. And every now and then, there is still that awful smell.

I cross the shopping area very quickly and head towards the beautiful-looking, coconut-studded beach. The light is stunning and the sea looks great. You get your first glimpse at the possibility that you have landed in a Caribbean paradise after all. But not for long . . .

As you head to the beach, you quickly notice a ‘no swimming’ sign. And if you can't read Spanish, the smell coming from the beach drives the point very effectively. The secret of the smell is suddenly revealed. All the town's sewerage goes straight in the beach and to make things worse, every couple of minutes or so, the wind sends the smell up the streets into the shopping centre.

The first to be hit are the restaurants lined up on the road opposite the beach. Like everything else, restaurants are also grubby looking. And the food in them is does not look and does not taste the best.

Plastic city, cracked roads, petrol sniffing, sewerage and the smell of shit: in this environment one is struck by the unusually high number of Lebanese restaurants operated by Lebanese migrants. All the restaurants sell shawarma (Lebanese meat grilled vertically on a big rotating skewer) and Lebanese sweets. I counted six shawarma shops and three Lebanese sweet shops, usually adjacent to the shawarma shop and owned by the same people, on a stretch of beach road no more than 200 m long. My mind is thinking the obvious: why on earth would anyone migrate to live in a place like this?

I go to the restaurant whose name my informant in Caracas had given me. A young man is serving the shawarma. He is the person I am looking for.

‘You like it here?’ I asked after some introductions and a good hour of meaningless chit chat over a cup of Lebanese coffee.

‘I don't like working for my uncle. It is hard working for a relative’, he replies.

‘But do you like it here in Venezuela, in Puerto la Cruz?’ I insist.

‘I like the girls. There's lots of nice girls here. Nicer than the girls in Tripoli (Tripoli is the northern Lebanese capital closest to his village) . . . and more willing to have fun with you.’

I smile: ‘Is there anything else you like here more than Tripoli?’

He thinks for a while. ‘I am not living with my parents, that's good. I can do what I like.’

‘Except at work, right?’ I say jovially, ‘because there is your uncle.’

He nods.

‘You want my honest opinion?’, I said. ‘I am wondering how can someone leave (the name of the village) for a place like this? Your village is so much nicer. This place is the pits. Why would you leave your village, the fig trees, the olive trees, the orange fields, the birds in the sky, and all that and come to a place like this?’

He looks at me a bit puzzled for a moment. ‘Yes, I agree’, he finally said, ‘I really miss the village but there's nothing there. What can we do? I need to make a living.’

‘But couldn’t you make a living like this (cutting shawarma) in Tripoli?’ I boldly insisted. His reply was surprisingly quick, as if he has thought about it before.
‘Yes, but I’ll stay in it for the rest of my life. Here it is only something my uncle has given me until I find a better job.’

‘So you think you’ll find a better job?’ I say.

He looks at me: ‘I don’t know.’

Here we have again the importance of the relationship between existential mobility and physical mobility in understanding migration. ‘I’ll stay in it for the rest of my life’ was the fear of being stuck. The issue was not to have or not to have a job. It was to have or not to have a sense of going somewhere in life. One notes that the promise of a better future here is encapsulated in the ‘I don’t know’. Better the uncertainty, which also means the possibility of mobility, than the perceived certainty of immobility. Often the trauma of migration sets in when one realizes that here too one has ended up being stuck, but in an unfamiliar rather than a familiar surrounding.

CONCLUSION

Perhaps I will be accused by non-anthropologists of anthropological chauvinism, but after spending so many years examining all kinds of work in the areas of migration and diasporic studies, I have consistently found it to be the case that of all the disciplines deployed in studying globalization, migration and mobility, none are better equipped to capture the complexities of such social realities than an ethnographic analysis. This is because, more than ever, such realities still require the double gaze capable of capturing both descriptively the lived cultures with all their subtleties and analytically the global which structures them, both people’s experiences and the social environment in which this experience is grounded, both the experiential surrounding that people are aware of and the macro-global structures that are well beyond their reach. Reflexive ethnographic work is best capable of constantly engaging in this double gaze, and it is with the aim of refining such ethnographic work that I have critically assessed the analytical reach of the key concepts I have examined in this article. Because of their innovative seductiveness and the fact that they do have a purchase on current aspects of our globalized social life, it is in the nature of such concepts when used unreflexively to both give access to but also to blunt one’s capacity to account for particular social realities in the field. I hope this article has exemplified how one can sharpen analytical tools and theories in the very process of engaging in empirical research.

Notes

1 I actually began my research in 2000 with a third Shi’a village in south Lebanon, but following 9/11 the diasporic culture of this village was radically transformed and I decided that this transformation, given its importance, required to be studied in itself rather than in comparative perspective.
2 See particularly George Marcus’s (1998) collected articles on that matter.

References


GHASSAN HAGE is chair of the Department of Anthropology at the University of Sydney. His research interests include inter-communal relations and national identification in Lebanon and Australia, Lebanese migration, whiteness, multiculturalism, racism, philosophical and psychoanalytic anthropology. His books include *White Nation: Fantasies of White Supremacy in a Multicultural Society* (2000), *Arab-Australians: Citizenship and Belonging* (editor, 2002) and *Against Paranoid Nationalism: Searching for Hope in a Shrinking Society* (2003). He is currently embarking on a project examining the circulation of the emotions generated by the Arab-Israeli conflict among Lebanese Shi’a migrants in Europe, the USA and Australia. Address: Department of Anthropology, University of Sydney, NSW 2006, Australia. [email: ghassan.hage@arts.usyd.edu.au]