# Infrastructures in and of ethnography

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**Abstract:** This contribution invites reflection on some of the conditions under which ethnographic enquiry is carried forward. Taking its cue from the concept of infrastructure, commonly understood as the practical supports underpinning an enterprise, it extends the notion to include ideas or assumptions that may be sustaining the purpose of enquiry. It thus takes practical and ideational supports in tandem. Intermittently visible, falling beyond the purview of the topics being investigated, and thus rather less than explicit contexts for research, the infrastructures of ethnographic work afford some insight into its changing circumstances. Importantly, these include changing orientations towards or conceptualizations of the kinds of objects of knowledge regarded as its ultimate aim. The reflections are exercised on materials from Oceania, from both the beginning and the end of the century that Bronislaw Malinowski inaugurated upon his arrival in the Trobriand district of Kiriwina in 1915.

**Keywords:** Infrastructures, Malinowski, Ethnography, Melanesia, Concepts, Supports.

*Preamble*

In the middle of discussing the reasons why Trobriand Islanders decide to fill the yam houses of a chief in a competitive manner (*kayasa*), Malinowski gives an account of what happened in 1918 at the Kiriwinan village of Omarakana. Circumstances force him into what must be one of the most famous digressions in the ethnographic record, an aside on Trobriand cricket[1]. In a book devoted to agricultural practices (*Coral Gardens*, 1935) cricket comes up in the context of some of the disputes that made that season's *kayasa* necessary. Comparing his Polish view of the «then newly introduced» source of competition with that of the Islanders and again with the view (as he rendered it) of the English – for whom cricket was «a synonym for honour and sportsmanlike behaviour» (Malinowski 1935: 211-212) – Malinowski notes it was at once a cause for violent quarrelling and also served «as a newly invented system of gambling» (*ibidem*: 212). We shall come back to gambling. In the meanwhile, what might seem intrusive to the main account also gestures to a context for Trobriand life, a glimpse into other circumstances, which somehow lie "outside" it.

The notion of context, and the elucidation thereof, is frequently brought forward as crucial to both the means and ends of ethnographic enquiry. Its position in ethnographic accounts is itself highly contextual, we might say, and among others Mitchell (2010: 12) has drawn explicit attention to the importance of recognizing multiple contexts as so many diverse perspectives on specific situations. Contexts are in this sense held to be illuminating of the material in hand, even when – and whether or not seen as subject to pre-selection (Widlock 2010) or to infinite regression (Schlecker, Hirsch 2001) – it is known that more contexts could always be marshalled. In addition are those not-yet articulated contexts such as we might apprehend in some of Malinowski's asides. These are not positioned to become reference points for the discussion at that juncture; rather they seem to serve as reminders that there is more going on that the discussion can attend to (they may, of

1. Malinowski's style of writing is well known for its asides, sometimes comprising whole topics and chapter sections, as for example in all the theoretical-ethnographic as well as geographic stopping places that accompany his account of overseas voyaging and *kula* exchange relations (1922).

course, turn out to be precious resources for subsequent generations of anthropologists engaged in quite different discussions). This article will focus on some of the "outsides" of ethnographic research before they are, so to speak, positioned as (explicit) contexts to it[2].

I propose to draw on the concept of infrastructure to capture something of those outsides that appear as not insignificant supports to the ethnographic enterprise. In the course of doing so, I shall not only talk about infrastructures "of" that particular enterprise, but also thicken our sense of the concept through one or two examples of ethnographic moments that deploy it heuristically, infrastructures as they are encountered "in" ethnography.

## *Infrastructures, of different kinds*

Certain current uses of the term infrastructure from beyond anthropology would find within it something of a counterpart in the notion of pre-condition, as in Green's (2014) address to the «conditions of possibility» of doing anthropology[3]. However, with Jensen and Winthereik's[4] reminder that infrastructure was originally a military term designating fixed facilities, the basis upon which a society or organization operates (roads, waterways, power grids, schools, prisons are their examples), current usage that stresses «a basic structure through which, or on top of which, other (organizational) activities occur» tends to have a largely technological referent (Jensen, Winthereik 2013: 2). At the same time other traditions of usage[5] aim for a less reductive emphasis: whether in the form of roads or databases, infrastructures «are entwined with cultural habits, with social and economical organization, with professional and personal identities» (*ibidem*: 2). Either of these connotations may be found in ethnographic accounts. Yet in thinking about infrastructures of ethnographic practice as such, not only ethnographic writing but also the initial enquiries (as in "fieldwork"), we may wish to ponder on another dimension as well. For want of a better vocabulary, we may talk of ideational or conceptual infrastructures.

2. One might imagine them as "externalities" [after Michel Callon], although that concept was first devised to point to consequences of (economic action) that lay beyond the focus of an enterprise and were not thus accounted or costed as part of it. My focus here is on preconditions rather than consequences.
3. The concept is also familiar to anthropologists from another perspective altogether: neo-Marxism. Here the much disputed alignments of base (infrastructure) and superstructure can be thought of as modulations of an overall partitioning of structure (see Humphrey 2018 for a recent exposition). This sense of the concept lies outside the present discussion.
4. They cite the historian Edwards 2003 on the origin of the term.

Consider again how Malinowksi positioned himself. Referring to the number of times he had accompanied parties of gardeners to the inland village of Omarakana, the Paramount Chief's place, and back, he was able to make a comprehensive recording and tabulation of harvest gifts in a more complete manner than had been possible before (the baskets of yams ran to tens of thousands). «On that occasion better than any other was I made to understand how inextricably the economic side of chieftainship was bound up with their political power» (Malinowski 1935: 217). If there was ever a connection being made between familiarity born of long term residence and the ability to synthesize apparently discrete data, here it was in a nutshell. That observation about inextricability is immediately followed by another aside: he recalls his lasting regret at not seeing the finale of this competitive display. A spell of ill-health meant he had to leave Omarakana and move into the verandah of his friend, the trader Billy Hancock, whose house was on the shore of the lagoon.

Here, also in a nutshell, are several dimensions of infrastructure. They draw attention to some of the underpinnings and supports of anthropological scholarship and to the place of ethnographic research within it. The eminently practical conditions under which it is possible to undertake such research at all come into view. Michael Young's (2004) magnificent biography of Malinowski makes it clear just what an uncertain venture Malinowski's sojourn on the Trobriands was, given all its contingencies and serendipities. Hancock's house, for example, turned out to offer a haven to which Malinowski retreated from time to time, it being somewhere where he could store his equipment and, with its darkroom, print his photographs. If we call this a *practical infrastructure*[6], then it has a second aspect to it,

5. The authors are referring to Science and Technology Studies here, later including anthropology as a near neighbour (and see for example Harvey 2010; Morita and Jensen 2017; Nielson 2012). They pursue a quite different agenda from the one defined here, which proceeds with respect to the well known anthropological interest in context. Thus their own book is addressed to information infrastructures for enhancing modes of creating accountability in aid development, including both monitoring technologies and aid partnerships. Here infrastructure is taken not as "outside" but as "central" to new platforms of action: «Making aid infrastructures entails forging new platforms for action that are simultaneously imaginative and practical, simultaneously conceptual and technical» (Jensen, Winthereik 2013: xv). At the same time, «infrastructures retain their own ingrained inertia and sense of direction ... [to the extent that] organizations seem to drift *on top of* infrastructures whose installed bases makes them largely inert» (2013: 5, original emphasis). For further discussion, see Carey and Pedersen 2017.

6. "Practical" is redundant as an epithet for infrastructure, or at least until we wish to compare it with another kind of ("conceptual") infrastructure, see below. The pair of terms makes no sense on their own, although I hope to show they have some heuristic value for the circumstances being described here.

namely its entwinement in the professional and personal style by which Malinowski conducted his observational practice. He combined participating and listening, on the one hand, with, on the other, note-taking and tabulating, the practical – as in praxis[7] – entailments of on-the-ground enquiry. Entwined in this, too, would have been the anticipation of analysis and writing. The end goal of his work is revealed in how he related that subsequent insight to this activity, namely his insight into the connections between economics and politics. In retrospect we can say that Malinowski was simply connecting what the conventions of the time separated, but the obvious point is that the frames of reference with which he came are not to be discounted for their role in the way he went about his task. We might suggest that these, potentially at least, comprised an *ideational or conceptual infrastructure*.

Needless to say, as soon as we envisage Malinowski in his office at LSE or writing away in the house at Oberbozen, those latter frames of reference will appear as the theoretical context of his contribution to the discipline, and there was nothing to prevent such frames of reference being explicitly summoned during the course of enquiries in the Trobriands. However, my interest here is in conceptual supports at the moment at which they occupy an implicit, taken for granted position; in other words, when they are doing their work as "outside" the ethnographic enquiry in hand, and when, as far as the ethnographer is concerned, they are not yet transformed into or positioned as contexts for it. Otherwise put, from everything that the ethnographer mentally brings with him or her we might be interested in discerning what seems to be playing an infrastructural role. That may be as essential to the task as diverse practical infrastructures. It goes without saying that the same ethnographer is crucially dependent on manifold further supports from the people with whom enquiries are being pursued, practical or ideational. The latter include those provided by local habits of acting and thinking whose nature may allow absorption with lesser or greater degrees of consciousness[8]. I leave the observation as itself no more than an aside, a glimpse into an order of support on an altogether other, and invariably multiple, scale.

7. Involving the embodiment or enactment of ideas, but generally comprehended by the (Euro-American) actor as belonging to a world of action, as experiential, actual, workable, effective. I keep with the term "practical" in its plainest, ordinary language sense.
8. My thanks to Andre Gingrich (conference comment) for making this point.

## Practical supports for ethnographic research

First hand enquiry didn't begin or end with Malinowski, but he defined it in a distinctive way. It was in writing up his account that he came to emphasize the advantages of this mode and the quality of information it yielded. Yet its success was not guaranteed from the outset. Regardless of the political issues that detained him on the Trobriands, such "fieldwork" would not have been possible but for a range of practical supports.

European attitudes to exploration, including the expectation of support through governmental institutions, contributed to the possibility of carrying out this kind of research, and spoke to the reach of colonialism. It would be anachronistic to read back into Malinowski's comportment on the Trobriands what later became a critical dimension to ethnographic work itself, including criticism of the consequences of colonial dominion. Nonetheless that was one of its effects. Wanting nothing to do with basing his knowledge on what was already defunct, that is, on reconstructing past practices, he planned at one point to write an account of the changes wrought by the new circumstances in which the early twentieth century Islanders found themselves (Young 2004: 470). In any event, deliberately or not, it was because he had revived interest in various "customs" (such as funeral practices), which the administration had tried to suppress[9], that Malinowski became a veritable thorn in the side of the Assistant Resident Magistrate, Raynor Bellamy. Bellamy, who (in Young's [2004: 382] words) «did more than anyone to set the colonial scene and establish the conditions of Malinowski's fieldwork [in Kiriwina]», had trained as a medical man and was pursuing a programme of modernization; thus his population statistics were the most reliable in the whole of the Territory of Papua at the time, and he all but eliminated venereal disease (2004: 386-7). In the early days of 1915 Malinowski had boarded with him. Noting Malinowski's subsequent failure to acknowledge Bellamy's support, or any of his writings on the Trobriands, Young (2004: 389) repeats that it was Bellamy who created «the largely favourable conditions – administrative and medical – under which he conducted his fieldwork». The very possibilities of freedom for open-ended enquiry based on long-term residence, with all that implied for personal security, the willingness of Trobrianders to cooperate, and so forth, were facilitated by the colonial infrastructure. Of course, many anthropologists have not hesitated to make that point and to subsequently query the role that all this might have played in ethnographic writing.

9. Such as village burial; sorcery; power of the chiefs; polygamy: «Malinowski took the liberal view that government intervention was unwarranted and essentially destructive of native institutions» (Young 2004: 389).

We may contrast the "outside" position that Bellamy's practical supports apparently occupied in Malinowski's scheme of things with a further arena crucial to the success of the very idea of fieldwork, as it came to be formulated, and one that was treated as an overt context for it. When Malinowski first came to write up his Trobriand material, in the foreground of his presentation (in *Argonauts* [1922]) were the prejudices of his imagined audience, or rather the prejudices the anthropologist found among the British. Such people did not think Oceania was a blank because so few anthropological studies had been done there; on the contrary, they already knew that it was full of savages who were either unruly or else over-ruled by the dictates of custom. This outside was brought into an explicit framing of the research endeavour. In its face came Malinowski's brilliant diatribe against uninformed hypotheses about «primitive economics». Part of his advocacy was that there was no substitute for first hand knowledge. This was exactly the impulse that underwrote the experimental sciences of the time, and that had been behind earlier voyages to the Papuan Coast area (such as the Haddon expeditions to the Torres Straits Islands). Indeed, the person now recalled as the British epitome of armchair anthropology, James Frazer, had had a similar impulse – let's actually talk to people who live there – so Frazer consulted missionaries, planters and anyone who would correspond with him from their first hand knowledge[10]. Subsequent anthropologists have learnt to dismiss that way of doing things, precisely because of what was to follow – as Malinowski himself proclaimed, he consulted the Islanders directly.

What made such research possible in Malinowski's time – the network of government officers, traders, missionaries and so on, including their interest or at least neutrality, and the transport and trade connection that underlay this – was not invisible but was external to the central project. Similar circumstances continued in many places well beyond colonialism. One thinks, for example, of the role that extensive postcolonial development, and diverse policy initiatives, have had in continuing governmental funding for anthropology. At the same time, the success of ethnographic research had its own life, and has endured as a model for studies under quite different, non-colonial, non-policy oriented, infrastructural conditions (apropos the UK, one might think of British village studies, which got under way after the second world war). The extent to which ethnographic research anywhere requires practical supports, and is consequently embedded in diverse locales (whatever the locale is, and it need not be local*ized*) should not be underestimated.

10. Indeed, such enquiries were not a negligible part of Malinowski's own studies on the Trobriands.

When such supporting practices become themselves objects of attention then they morph into contexts for research and contexts to be researched; here infrastructures of ethnographic practice can become infrastructures in such practice. What emerge as objects of attention will depend on the interests of the moment. Thinking of Dr Bellamy and his administrative efforts on the Trobriands, we probably find it easier to reflect back on the crucial context of the "colonial" supports of Malinowski's research than we do on the medical infrastructure that colonization afforded, and everything else besides, including communications. Just what, we might ask, lay behind Malinowski's ability to write from the Trobriands in 1918 to Papua's recently appointed Chief Medical Officer (William Strong) for advice, because he was feeling particularly rotten, and receive a written reply, from wherever the Officer happened to be at the time (Young 2004: 478-9)? Equally obviously, then, there will always be a relationship between the factors that the anthropologist highlights and those that remain outside.

Alice Street's (2014) study of a modern Papua New Guinean hospital in Madang directly addresses the infrastructure of this enterprise (the term is in the subtitle of her monograph). Her questions are all about how it is that certain medical practices are sustained in a context where the administrative delivery of funding, medicine, expertise and even statistics is precarious. «X-ray machines do not work well in the hot and humid climate, pathology machines are old and difficult to calibrate, and the reagents stored in fridges are rendered unreliable by the frequent power cuts» (Street 2014: 11). Technology, and with it biomedicine, is imperilled in ways Papua's former Chief Medical Officer would never have encountered. She goes on: «weak states, structural adjustment [international economic sanctions], and extractive capital have led to the degradation of public health infrastructure» (*ibidem*: 11)[11]. She does not mince the criticism.

I take this as an example of those situations where anthropologists work with acute awareness of the practical infrastructures of action, so that – as objects of study – they are open to being re-conceptualized. One such impetus lay behind the cultural critique of the 1980s-90s (for a subsequent reprise see Marcus 2010). This began as a critical effort to make the practice of ethnographic research as explicit as possible: its blind spot was that a

11. Of the Madang hospital she writes: «the visual operations of modern biomedicine become intertwined with the visual politics of personhood in an unstable place where *the infrastructures for producing knowledge and governing populations are tenuous*» (Street 2014: 13, my emphasis). She adds that its infrastructural poverty is also a scientific resource, insofar as it is the basis of externally-funded research enclaves.

politically motivated focus on the conditions of "field" enquiry pushed another infrastructural entity into the foreground. This was the anthropologist's person in term of his or her subjectivity.

## Conceptual supports for ethnographic research

It is fascinating that what was understood as Malinowski's kind of ethnography came to underpin a vision in British Social Anthropology that he himself never brought to fruition. He had put in place a compelling rule of thumb: anything might be significant! His asides and digressions are interesting from this perspective. So such an approach seemingly encouraged study in the round, and simply at the scale of showing the interconnectedness of things it was one that produced huge dividends. But he himself did not articulate what was to become a significant rationale for undertaking ethnographic work. Over the course of the twentieth century, developing ideas of social structure and social system supported a holistic justification of a different order from Malinowski's stated interest, largely in terms of human nature, while also being one to which long-term research immersement became the crucial accompaniment. Ethnographers came to scale-up their findings as a matter of imagining the ultimate object of enquiry being "society" or "culture". When explained these work as contexts, but they are also candidates for enduring infrastructural support.

Conceptual or ideational infrastructures will be as much subject to change as anything else. These days, anthropologists no longer subscribe in an unreflective way to those concepts of society or culture. Nonetheless, perhaps we might recognize the organizing work they once did, how it was that they shaped the purpose of enquiry, not least in encouraging a broad view of what fell within one's scope. Anthropologists still continue to refer to "social" or "cultural" issues or relations, even though the terms have become simple place-holders for the methodological imperative to demonstrate some kind of coherence or interaction between apparently diverse elements, without presupposing a specifiable whole. This leads to a question. What stands in for holism today, for the wider picture that seemed summoned (at least in retrospect) when mid-century anthropologists came to write up, that is, when they were creating an ethnography[12]? Are there, these days, embracing visions that provides infrastructural supports for an embracing method?

12. For some of the diverse ways in which the theoretical construct of holism has been treated in anthropology, see Otto and Bubandt 2010.

An analogue to the very need for that question is suggested in a recent criticism of modern organization theory (du Gay, Vikkelsø 2017). The authors' own statement of purpose points to the need to remedy the fact that «OT [Organizational Theory] has dispensed or lost touch with the "classical stance", has increasingly adopted a "metaphysical stance", and, in doing so, has increasingly assisted the disappearance of its own core object – "formal organization"» (*ibidem*: 20). They point how, over the last quarter century, contemporary organization scholars have transcended the topic of formal organizations by a focus on networks, and similar multitudinous complex relationships (capabilities, assemblages, action-nets) often imagined as exerting their agency in the abstract. These are in turn acted upon as universal imperatives external to the specificity of organizational form and function; the empirical effect is that attention gets drawn away from the task of organization and towards implementing the generic value (e.g. sustaining "networks"). And formality can no longer be taken as central to organizations. Indeed it seems that formal organizations may stand in a similar relationship to present-day organizational research as holistic notions of "society" or "culture" or "structure" do to certain present day anthropological horizons. In du Gay and Vikkelsø's eyes this move has come at too high a price. Anthropology, however, is not in quite the same position.

Today's general social science orientation to networking and the like – the former surely past any theoretical peak, regardless of its continuing utility – is familiar enough within anthropology. Yet long before "networks" became methodologically salient, anthropology was already engaging with an equally flexible construct, viz. relations and relationships. By relations I refer to both conceptual or logical relations (between entities of all kinds) and to interpersonal social relations (pace Pina-Cabral 2017); they have a far wider reach than the organizational concept of network, although they can be imagined in such terms too[13]. We can properly talk of an enduring "relational infrastructure" (to borrow from Holbraad and Pederson [2017: 270] a phrase they apply to the ontological turn) as one of anthropology's most significant and most enduring conceptual supports. Insofar as the discipline investigates relations through enacting relations, the process is personified when research venues are defined through interpersonal relations. That said, as contexts they are one thing, as infrastructure another.

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13. One could cite many examples, among them: Das' (2015: 89 my emphasis) hope that ethnography «will make for deeper understanding of *the web of connections* between [in this case] familial neglect and institutional neglect without reducing one to the other».

Consider Street's relational supports in Madang. As always, and she writes with great empathy, what emerged as crucial were the relations that she formed, in this case not just with hospital patients but with the medical establishment who were also an audience for her book. «It is always difficult as an anthropologist», she says, «to write critically about persons and practices where we work» (2014: 11). Her public agonizing over this (in her monograph) was underlined in a conversation when we met in Port Moresby in 2015: returning for subsequent visits made "fieldwork", she said, harder and harder[14]. The people she knew there she now knew too well; knowing so much, or apparently so, made asking (research) questions more and more pointless – or if not pointless for her, simply incomprehensible or even insulting for the acquaintances to whom she felt very close.

Here a necessary relational infrastructure has become explicit – but not *as* infrastructure. Rather her network of relations had morphed into something else, that is, it no longer supported the anthropologist's research in the same way. Turning the hospital's supports, including relations with the staff, into a study that addressed infrastructure (infrastructure "in" the ethnography) thus set severe problems for ethnographic writing. Writing a critical account, as she felt compelled to do, compromised those relations, both as practical supports (offering enablements of all kinds) and as conceptual supports (all the protocols of getting on with people, maintaining rapport, interviewing, talking). For the relations seemingly dropped away *as supports* and become relations scaled-up in what was experienced as a different register, that is, in "personal" terms (see Riles 2000: 61-69). A significant element here was that valuing a personal cast to relations, implying their contrast with other kinds of relations, would have been appreciated by the urban / medical elite.

Relations may develop in other ways. It is even possible to deliberately scale up a relational infrastructure, as Sergio Jarillo (n.d.) did the same year (2015). Organizing a symposium in the town of Alotau to commemorate the centenary of Malinowski's arrival in the region, he mobilized diverse networks among Trobrianders and other Milne Bay people, including national ministers and politicians, international anthropologists, local schools, and so forth. A good number of the descendents of the Trobriand chiefs whom Malinowski knew were there, give and take local politics, to create a meeting of a kind that might have surprised his (Malinowski's) younger self. The subtitle of Jarillo's write-up, *The academic conference as ethnographic performance*, gives the gist of his analysis: «the anthropology conference, seen as research practice, prove[d] successful in readmitting

14. Alice Street, personal communication, 2015. I am very grateful for her permission to refer to this.

ethnographic subjects [such as the said chiefs] into anthropological dialogues» (Jarillo n.d., abstract). So relations with these ethnographic subjects continued as supports for anthropological dialogue.

The occasion was conducted with considerable diplomacy. I wonder whether that was, in part, enabled by the fact that no-one knew everything that was happening. Its success may thus have owed something to way in which multiple networks were being activated simultaneously, so that several different concourses of interactions could go on without too much interference from one another. The mélange can be understood less as falling short of an idea of comprehensive communication than as actually entailing a necessary ignorance, an occlusion of the ramifications of relations, the partial isolation of actors from one another. There was certainly no single axis of confrontation. However, there was also another factor, owed largely to the circumspectness of the Trobriand men and women present. Their sense of rank and protocol introduced a formality into the situation: apart from oratorical display, there were betel nut exchanges going on here, ceremonial tokens handed out there. This gave the occasion an aesthetic form way beyond the procedures of an anthropological meeting.

To describe it thus turns this short 3-4 day conference in Alotau into a circumscribed arena of its own. It might have been an ethnographic object for Jarillo, who had spent lengthy research time on the Trobriands, and for whom this was yet another venue; for me – simply attending on this brief occasion – it could not possibly be an ethnographic object, even with his write-up. The conceptual supports in terms of interpersonal relations were (for me) missing.

It would probably take a focused enquiry into the nature of meetings to reveal the possibility of such supports. There might be something to be learnt, for instance, from the diverse contexts analyzed by anthropologists collaborating in what they explicitly refer to as *An ethnography of meeting* (Brown, Reed, Yarrow 2017). Theirs is a call rather like du Gay and Vikkelsø's advocated return to the study of how formal organizations work. Brown, Reed and Yarrow give a distinctive focus to meetings by their attention to the mundane forms through which the former operate. These include how relations "within" meeting spaces relate to their transformations "beyond" – the internal and external contexts of these relations – with respect to «institutional structure, time, space, and society» (Brown, Reed, Yarrow 2017: 15)[15]. One aim is to move away from recent attacks on bureaucratic

15. This interpenetration (relations inside / outside) may address some of du Gay and Vikkelsø's (e.g. 2017: 61) disquiet when too much emphasis is given to extrinsic "contexts" under which (their example) formal organization disappears through being interpreted through other phenomena that then appear to be the relevant subject matter.

conduct and open up room for less reductive approaches, indeed to complexify what is concretely going on in a meeting in terms of situated practices, disputes and hesitations, ethical nuances. As already implied, relations internal to meetings are relations that also have an outside to them, not just one but diversely so – and the contributors to this collection were free to bring in whatever outside perspective gave them an analytical vantage point. That said, it would take actual participation in the meetings described here to appreciate what might work as an implicit infrastructure in relation to one's own conduct.

## Two "Oceanias"

I suggested that, from everything that keeps the ethnographer company in his or her head, we might be interested in discerning what seems to be playing an infrastructural role. Given the aspirations of the ethnographic enterprise, as opposed to other kinds of research, potential candidates include what in the past might have supported a "holistic" enterprise, and latterly – until recently, that is, given that the concept no longer holds its once confidential promises (Riles 2017) – perhaps a "global" one. I also suggested that as an adjunct to the explicit concept of context, infrastructures might be usefully apprehended as outside the scope of enquiry. Yet how does one recognize an "outside"? Rather than hypothecate, I enroll some contemporary ethnography to sketch what certain anthropologists appear to have taken as conceptual infrastructures of their work.

Two recent issues of a regional anthropological journal, *Oceania*, throw light on the kinds of objects of enquiry some present-day ethnographic work is intended to illuminate. Each is an edited special issue (November 2014 and March 2015). One deals with work in Melanesia (here Papua New Guinea and Fiji), the other drawing more widely on Oceania, but both include articles on the Trobriand Islands.

The first, edited by Anthony Pickles, is addressed to gambling, in the form of card playing. Recall that aside of Malinowski's. His nine-word phrase, «as well as a newly invented system of gambling», is thankfully noted in the literature on Melanesian gambling as one of the earliest references to this activity, which over more than a century still seems to convey a sense of modernity. There is an ambiguity in Malinowksi's phrasing, however, a function perhaps of its throw-away "outside" status. I have interpreted his phrase (above) as referring to cricket allowing new possibilities for gambling, a metaphor he also used in relation to luck in the *kula* (Mosko 2014: 242)[16].

16. Luck here also has reference to magic, and Mosko warns that it would be a mistake to elide Trobriand concepts with the impersonal kind of luck often imagined by Euro-Americans.

However Pickles (2014: 211), like Mark Mosko, picks it up as a reference to the card playing that became the synonym of gambling throughout Papua New Guinea, and as a practice that accompanied the equally introduced cricket.

Pickles takes present day gambling as an indigenous analytic: his argument is that gambling provides people with the reflexive resources to comment on many aspects of their lives – on their relations with one another, on new sources of inequality and uncertainty, and on speed, change and the nature of money[17]. He makes it clear where his "outside" is: he is «determined to prevent an insular debate in which Melanesianist accounts of gambling alienate themselves from gambling studies [more broadly]», especially insofar as these have taken gambling as a metaphor for understanding social life. «I invite readers to consider our portrayals of indigenous ideas of "what gambling is about" as alternative theorizations of gambling as a phenomenon» (2014: 219; 207). There is a need to *make* them of interest. As he adds, that these theories of gambling may be based upon apparently unusual cosmological premises does not prevent them from intersecting productively with Euro-American forms. If that is articulated as a context for the studies, the comparative potential remains largely tacit, its power perhaps being of an infrastructural kind.

More than that, gambling as an object of enquiry is being treated rather like Brown, Reed and Yarrow's meeting. Such objects of enquiry are increasingly familiar in anthropological studies conscious of a global world. An artefact, a topic, usually taking the grammatical form of a singular substantive ("gambling", "meeting"), becomes a prism through which one may see the delineation of much larger issues. In a sense, the more compact the substantive is, the more dramatic its folding out. The conceptual support is there in that assumption of its microcosmic import, the world in a grain of sand. In practical terms, each contribution to this collection of articles had conventional long-term ethnographic research in the background, although apropos the article on Trobriand gambling, Mosko's work (see 2017) is based not only on many "fieldwork" trips but also on extensive intellectual interchanges with Kiriwinan chiefs among others.

17. Pickles writes in his own paper: «games can be seen to act as "analytics", not simply by reflecting indigenous predilections, but in the manner in which gambling invites participants to engage with the way they want the future to be decided [...] and to craft microcosmic iterations of their idealized forms of causality within relationships» (2014: 274). Pickles is by no means alone among anthropologists in treating (specific) indigenous practices as analytically astute theorizations.

The Afterword by Rebecca Cassidy (2014: 306) is too pointed not to cite. Her PhD had been done on the UK thoroughbred racing industry; for a postdoctoral project, she writes about a particular glimpse of what we might call a global phenomenon, economic insecurity.

> I was due to go to Albania to study shifting landscapes and economic insecurity but the unrest precipitated by the collapse of local Ponzi [pyramidal] schemes meant that I was not allowed to travel. Frustrated, I took a job with a firm of security guards. "White Knights" operated a simple racket, taking a little, but not too much, from companies in and around Cambridge [where she lived] who employed them to protect them from greedier thieves. [...] Among the ex-cons and retired policemen who made up our ranks were two committed horserace bettors [...] I was invited to join them to place a bet on a "sure thing" after helping one of them with his daughter's pony (named Santa because he was bought with the proceeds of Christmas trees knocked off from one of our "clients", a garden centre...). I had stumbled into fieldwork on economic uncertainty after all.

Many years later Cassidy embarked on collaborative research addressing the expansion of commercial gambling, and the global references are overt. «My fieldwork coincided with global economic downturns, massive changes in technology, and an associated period of regulation and normalization in gambling» (2014: 307). Here is another kind of outside: competing accounts and concomitant expectations from the investments that the global gambling industry was itself making in research (funding research centres, as well as charities set up to collect contributions towards research, journals and conferences). This investigative apparatus, she argues was concerned to present the individual gambler as a particular kind of subject. Indeed her article speaks of the way in which «mainstream research turns gambling and gamblers into knowable subjects» (2014: 307), leading to a proliferation of studies to detect pathological gamblers, while leaving open-ended social science approaches out in the cold. (Everyone is looking for a cure for "problem gamblers".) While in one sense this alter-research worked as a context for her own, spurring her to be explicit about the consequences of such an emphasis (one's ambition cannot be limited, she says, to simply providing «more adequate descriptions» of gambling [2014: 307]), the relations she established with such interests and expectations had to be on the "outside" of those relations she wished to set up in her own study. If in this sense they were working as infrastructural supports, they are a reminder that there is not necessarily anything benign in the notion of support[18].

18. Notions such as "support" or "enablement" carry, in English, the same kinds of positive overtones that accompany "relations", "social" and numerous other epithets (the same goes, with reversed values, for those with negative overtones). It is virtually impossible to shake these evaluations off, although through the notion of "infrastructure" I have tried to convey as much neutrality as possible.

An article on the Trobriands also appears in the second *Oceania* issue, to which I briefly turn. This was written by Katherine Lepani (2015), one of the Pacific Islands residents among other researchers brought together by Margaret Jolly in a large-scale project on gender and personhood. It provides fresh ethnographic research on how Trobriand women perceive themselves and the choices they have, while being explicitly framed by a theoretical debate on personhood and agency. This is also how the editors of the special issue cast the whole topic. It is, they say, through «exploring matters of deep concern to the people with whom it engages, [that] this volume can also be seen to reflect the current state of play with regard to large-scale topical concerns in the anthropology of the Pacific» (Morgain, Taylor 2015: 4). That scale is their "outside". There is no need to defend the topics or to argue for their interest. "Gender" and "personhood" are taken as axiomatically informative of general social and cultural issues, indeed as abstract concepts themselves they represent a kind of scaling-up of those matters of deep local concern. However, by contrast with (the topics of) gambling or meeting, they comprise a differently delineated object of enquiry. Theorizing about gender and personhood is addressed to acknowledged issues of universal significance in social life[19]. No microcosms here: they *are* the macrocosm.

What distinguishes the new approaches advanced by this collection is thus in large part to do with how new ethnography is newly theorized. The editors hope their volume «inflects [old] questions in new ways» (Morgain, Taylor 2015: 1). That there is a pertinent theoretical framework serves as a built-in conceptual context. The accompanying certainty, that one's study is supported by the acknowledged interest of what one is studying, may or may not be explicit, and perhaps has a counterpart in an even less explicit claim to significance, one further below the threshold of perceived practice. This is the idea that fresh fieldwork is to be justified by the theoretical or conceptual refinement one will bring to these *already-interesting* topics. Other refinements are also possible. Where, in this collection, "theoretical" intervention is most lightly felt, the article's more general purpose tends to be drawn into regional political issues. The editors comment that together the articles both challenge many of «the concepts that underpin social science» and provide «critical insights into current [local] conditions and concerns regarding transforming relations and articulations of gender and personhood» (2015: 2, 1), and they mean critical as in social criticism.

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19. It is hardly necessary to comment on the scope of the goal implied here, with respect to earlier epochs of what was assumed to be of anthropological significance.

These heterogeneous examples offer a sample of insights into ideational or conceptual supports that work as an implicit infrastructure to ethnographic research, one equally in need of extensive practical supports. The point at which the outside is drawn into view, whether as (external) contexts or as (internal) topics for investigation, indexes or demonstrates the generative potential of new research.

## Conclusion

This contribution has focused on a thoroughly traditional (geographical) region of anthropological research, Oceania, not to produce a yardstick from which to measure other circumstances, but to indicate some recurrent and ever-changing issues over the last century. To regard the practical conditions for ethnographic research as infrastructure perhaps makes it easier to specify what altered circumstances do and do not allow. An obvious (and very familiar) example applies to research that has interpersonal relations at its core: the oft-cited difference between long term residence among people where relations can only be renewed on the spot and commitments to people with whom one can keep in touch by long distance means. Perhaps less obvious are the sources of change that come with unspoken assumptions concerning what is important about ethnographic research as such. I have extended the ordinary English language concept of infrastructure as a matter of practical preconditions to include conceptual or ideational frameworks. It is only in order to effect such an extension that the latter has been added to the former, as though it were another kind of infrastructural dimension. In truth the two run together.

One small part of Malinowski's legacy is that laterality or relationality that comes from an aside, always an implicit comparison, because always evoking an elsewhere. When Malinowski brings in cricket to a discussion of harvest gifts to high ranking chiefs this is one moment out of a multitude of internal comparisons that he is making between diverse aspects of Trobriand life. There is little chance of predicting in advance what will prove fruitful to follow, or may be picked up decades later as of central interest. Nor can one tell which relations will yield productive connections – in the end Malinowski did not write his book on Trobriand social change. As when he said, «I was made to understand how inextricably the economic side of chieftainship was bound up with their political power», that lack of predictability includes relations between analytical concepts. Importantly, a digression from an object of enquiry can be turned around into perspectives into or upon it, as gambling suddenly opens up a way of thinking about harvest competitions.

There are many desirables for anthropological research, and they have never been equally attainable, nor should we overlook the fact that most anthropological discussion proceeds (as here) on the basis of other people's ethnographies. Yet at some point, it is assumed, there will have been social relationships; first hand knowledge; open-ended enquiry; internal comparisons; people's utterances and reflections; significant communications, and so forth. Ethnographic process – the investigation and the writing together – has been an epitome of these desirables. It is in that combination that crucial relations are created, although they are not always apparent. Not least of such relations are those between the ethnographic enterprise and its conditions of enablement, at once practical and ideational, serving or working in the manner of infrastructures.

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