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Collaborative Anthropologies, Volume 9, Numbers 1-2, Fall-Spring 2016-17,
pp. 32-39 (Article)

Published by University of Nebraska Press

DOI: <https://doi.org/10.1353/cla.2016.0010>



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Research through Collaborative Relationships

A Middle Ground for Reciprocal Transformations and Translations?

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Joanne Rappaport's article has two main virtues. On the one hand, she provides a thoughtful summary of very diverse experiences of collaborative research. On the other, she expands the field of vision of our discussions by means of key displacements of focus: from collaborative research to research through collaborative relationships; and from products to processes. She does so by means of a very ethnographic contextualization and historization of the initiatives of La Rosca de Investigación y Acción Social (Circle of Social Research and Action), a group deeply related to Orlando Fals Borda's appealing methodology of "action research."

One common problem in our attempts to discuss collaborative endeavors results from the fact that we always produce situated knowledge within situated contexts and histories. Thus, instead of general statements on the topic, what we all can share are situated experiences. Against such a backdrop, I cannot but make explicit that the reading I can make of Rappaport's contributions mostly comes from my experience with members of the Mapuche-Tewelche people of northwestern Patagonia, in Argentina. I am aware that other experiences might bring about different ruminations. In any event, during more of thirty years of interactions and exchanges, my interlocutors have expected and demanded from me (and I from them) very different things. Hence several of the resulting activities can be thought of as collaborations, but not as what we all try to grasp through the idea of collaborative research and methodologies,

if by these we plainly mean practices aiming at a co-theorization and co-analysis that emanate from an explicit agreement to do so.

Therefore, of the many interesting points raised by Joanne, I concentrate on two. First, the idea of *collaboration as a chain of multifaceted conversations* and then the notion that what seems to be “already there” is “in a continuous state of flux, an ongoing process of creation.”

Colleagues performing conversational analysis point out that what counts in any conversation is who rules the exchange—how and following whose agenda. In terms of a collaborative methodology and from a pragmatic viewpoint, the key point is that there can be different forms of proposing and performing conversations. Some interactions can indeed take on dialogical dynamics. However, others are monologues even if giving the appearance of an exchange. Closed questions that imply expected responses or allow mostly for yes/no answers end up in one party controlling and passing the floor and even in unilateral decisions about when the game is over. Moreover, if we approach conversations as processes, within a single talk there can be more than one form of exchange.

In any event, we can invest “conversations” with a significance that surpass the idea of mere exchange. According to the Online Etymology Dictionary, *conversation* is not a mere dialogue, because it comes from and means “living together, having dealings with others,” and also reflects a “manner of conducting oneself in the world.” Coming from the Latin *conversationem*—a noun of action from the past participle stem of *conversari*, “to live with, keep company with,” literally “to turn about with,” from the Latin *com*, “with” + *vertare*, frequentative of *vertere*—it also means “to turn, turn back, be turned, convert, transform, translate, be changed” (see <http://www.etymonline.com/index.php?term=conversation>). We can thus assume that conversations can also involve reciprocal transformations and translations. But what is it that should be transformed or translated through collaborations as chains of conversations? What or who is it that we attempt to transform and translate?

The collaborative turn can be seen as the result of the severe and necessary criticisms that anthropology has developed of science and scientific research in general, and of our discipline in particular. I believe that what moves most of us to engage in collaborations of different sorts is mainly our aim of neutralizing asymmetries and of undoing the unilateral privileges that researchers have as members of a dominant educated class or group, gender, or region. Now then, insofar as we keep on de-

fining ourselves as researchers (and in that condition we contact or are contacted by different interlocutors), collaborative relationships always entail more than the procurement of “a space of communal and reciprocal reflection,” as if we always worked or interacted in contexts solely structured by a Habermasian communicative rationality. Structural asymmetries and privileges cannot be undone solely by situational arrangements, and thus I wonder if collaborations are not—and might not be better thought of as—the common setting of a middle ground rather than as a chain of generic conversations; that is, as a standpoint or area of negotiation, midway between diverging positions, options, or objectives.

The predicament here results not simply from agreeing on the purposes of the collaboration in terms of the knowledge to be produced, but mainly from setting the political objectives of the common work and deciding what to do when these objectives do not overlap or somehow conflict. For instance, I do not doubt that the historical comics of La Rosca might have “played a crucial role in the process of ‘systematic devolution,’ inspiring organized peasants to adopt the tools of struggle of their forebears,” as Rappaport notes. I just wonder who decided that peasants wanted to adopt such tools instead of being empowered with new ones. On the other hand, I wonder what would have happened if the ideological criteria of ANUC had not matched those established by the Fundación del Caribe at the moment of its foundation. If social needs are socially constructed, so are the criteria of research topics that, as Rappaport puts it, “would be strategically useful to the peasant movement and could be translated into popular education initiatives and political action.” Popular education initiatives are not always the priority of social movements, even if they are key to the methodology that a research team promotes. I remember the exasperation that arose in a Mapuche assembly conducted by a team who developed popular education techniques. After several “games” to promote “the desires for participation,” the elders in particular expressed that they did not appreciate being treated like children, since they had traveled many kilometers to discuss important things. Furthermore, when we are part of the same public spaces and share as citizens the common responsibility of setting coexistence agreements, our own priorities and political objectives may not be in agreement with those of our interlocutors or at least those of some of them. It is well known that social movements are not homogeneous and that “peasants” nucleated within the same organization may disagree and even split up.

Given such a backdrop, should we agree to collaborate even if we do not share our interlocutors' political objectives? Do we benefit from the privilege of withdrawing from the collaborative relationships or of "selecting" which party is the most "legitimate"? By doing so, are we not becoming their unilateral censors? What does the idea of "unlearning our privileges" really mean?

In October 1995 a Mapuche organization asked me to do research on colonial treaties. The fifth Ibero-American Summit of Chiefs of State and Government was to take place in Bariloche, and the king of Spain, Juan Carlos I of Borbón, would attend. The Mapuche had decided to use the occasion "to take up a suspended conversation and negotiation," hoping that the king would agree to honor treaties signed during the eighteenth century. After the event, the International Working Group on Indigenous Affairs (IWGIA) agreed on financing a book about the treaties (Briones and Carrasco 2000). This time, we proposed with my colleague Morita Carrasco to do the write-up in co-authorship with the organization that took the initiative and members of the Rankvulche people, who had been a signatory party of the last republican treaties of 1878. A previous book (Carrasco and Briones 1996) had also been needed by a Mapuche organization and financed by IWGIA, and we had discussed it with our interlocutors, who proof-read it and made several comments, but it was co-authored only by the two of us, because they wanted the book to be invested with academic authority. In the meantime, however, several incidents altered the original agreement to prepare the second book.

On the one hand, the main members of the Mapuche organization who had made us the proposal split off from the organization that was invoking the king of Spain. On the other, during the meetings to agree upon the structure and contents of the book, a serious disagreement arose between our Mapuche and Rankvulche co-authors, the former saying that the Rankvulche were but a territorial Mapuche identity, and the latter saying that they were a distinctive indigenous people. During the process, there were indeed a lot of conversations, misunderstandings, and quarrels. At first we pleaded with the parties to solve their disagreements "in-house" before deciding how to proceed. Since this was not possible, Morita and I resolved first that we would collaborate with those who had made us the original proposal and then that, instead of everyone co-authoring the whole book, there would be three teams of authors: that

of the Mapuche interlocutors, that of the Rankvche interlocutors, and Morita and I as editors and writers of the introduction.

Needless to say, some people felt hurt in the process and criticized us in personal and public terms. Yet, for very different reasons, both books have been highly valued for members of different indigenous peoples in Argentina who were never part of the collaborative relationships that allowed these books to be published. In any event, the idea of a chain of conversations does not fully grasp the conflicting negotiations that took place among different parties (publishers included). Rather, both books were the result of a complex and somehow frustrating middle ground we had to surf with certain discontent, uncertainty, and somehow independent criteria.

Here the point I want to make is twofold. First, it is not always clear how and with whom we can unlearn the privileges that we have as researchers. The reversal of historical asymmetries may require from us that we not give up our own agency and determination to give the last word vis-à-vis disagreements among our interlocutors—this being a very dangerous, unpredictable, and uncomfortable ledge to navigate. Yet, complex as they can be, these interactions do not prevent collaborative relationships from ensuing but rather mean that they are always partial collaborations. Middle grounds cannot result in win-win results for all the parties involved. The best effect we can hope for is that everybody or most people feel that the agreements resulted from voluntary arrangements.

The second issue I want to address is Rappaport's idea that what seems to be "already there" is "in a continuous state of flux, an ongoing process of creation." After examining different experiences, Rappaport concludes that "collaboration should be conceived of as a creative activity, an exercise of the imagination, and a collective refashioning of community imaginaries, as well as the appropriation of ethnographic research methods to satisfy grassroots objectives."

I do not want to melt into a single statement very complex interactions, but I would dare to make explicit a more ambitious "intercultural utopia" by exploring and expanding the idea of creation. True, sometimes we are expected to carry out the methodological training of grassroots researchers, so that they are able to collect information themselves. On occasions, the collection of ethnographic or historical materials is demanded from us, for the collaborative team to use it according to their needs and purposes. But at other times we carry out fieldwork and interviews

together with some of our interlocutors, trying to understand a process of which we all assume that we know very little. In this last situation I am not sure whether what I define as fieldwork is seen as something more than just paying visits to those who know best to maintain fruitful conversations as usual and suitable means “to make, to explain, to think, to speak, and to represent,” as Rappaport quotes Talal Asad. Hence I am not sure either that the idea of an ongoing process of creation captures what happens in these very different ways of maintaining and performing collaborative relationships and methodologies. I wonder whether collaborations involve a process of creation or rather ignite varied and only partially overlapping processes.

One of these processes may well be that described by Vasco when he points to the repetitive discussions through which Guambianos “transformed the personal knowledge each participant carried into collective knowledge that could be harnessed through political action,” as quoted by Rappaport. She stresses that in this and in the Omaha case, “the external researcher was not extracting information, or imposing a theoretical framework, but collaborating in searching out new analytical approaches through which distinct knowledge-systems were placed in conversation with each another.” Another process is that stressed by Fals Borda when he promotes the creation of “*sentipensantes*,” or “thinking-feeling persons” through “imputation”; that is, through the “reimagining of historical experiences by placing oneself into the historical setting as an eyewitness,” again as quoted by Rappaport. Perhaps a different process is that in which the explicit “intentionality of research is harnessed to the work of culture-making” or “to harness the peasant imagination to grassroots political activism.” In any event, the point to be stressed is that there is no guarantee that all these processes occur at the same time, nor that they become nested.

Rappaport also stresses that a participatory methodology is one “fueled by the creative capacities not only of the external researchers, but of the peasants themselves,” especially when they materialize in products like La Rosca’s comics, the iconicity of which attests to both the construction of a dense description that invites non-readers “to recognize themselves in the pages” through a process of co-interpretation, and the provision not simply of “models of,” as academic ethnographies, but mainly of “models for” political action. In Rappaport’s reading, “analysis becomes a central component of collaboration.” In addition, she states,

the fact that during the interactions “adjustments are constantly being made, and interpretation is perpetually taking place—in different idioms and media, from different subject positions, and probably for very distinct reasons”—means that “there is no single party controlling the conversation.”

If creation is the name of the game, why is it that the first impression is that collaborative research elucidates something that was “already there”? Even if Joanne Rappaport does not say so, most examples point to the idea of anthropologists’ providing a service to their interlocutors. I would like to think of collaborative relationships as performing more significant and reciprocal transformations. On the one hand, our interlocutors are intercultural selves as much as we as researchers are. We all can and do read more than our own frame of knowledge and local historicity. We all can and do expect to produce new knowledge about the history and dynamics of the public spaces we have or want to inhabit. Therefore chances are that collaborative relationships make sense for all or many of us, not simply insofar as they allow us to recognize both difference and commonality, but mainly as they illuminate in a Benjaminian sense reciprocal blindnesses and deafnesses, though a complex play of interferences, inter-references, and translations. As researchers we do not have to indulge ourselves by thinking that we do what we do simply because we are good citizens or politically committed people. On occasion we can identify or create conceptual vehicles that may help to refashion community imaginaries. But always and at the same time, collaborative relationships should be embraced to help us objectify and reframe our cultural and disciplinary knowledge and common-sense understandings. Bettering anthropology may not be part of our interlocutors’ agenda but should always be part of our own relatively autonomous agenda. Thus I would underscore Joanne’s argument by stressing that, in neither case, is what collaborations produce “already there.”

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