Linking worlds: a theoretical reflection on some preconditions for ethnographic collaborations in personalized medicine

Ligando mundos: uma reflexão teórica sobre algumas precondições para realizar colaborações etnográficas em medicina de precisão

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Abstract Precision, or personalized, medicine (PM) is a ground-breaking approach to medical care which aims to predict, prevent and treat diseases by studying, on an individual scale, the pathogenic potential of the association between genetic and environmental factors. As one of the most important outcomes of biotechnological research, PM is generated in the lab. Nonetheless, the impacts of PM will be observed outside of the lab, namely, on the modification of population's patterns of use and access to healthcare. Taking PM as object of study, anthropologists are challenged to make a double reflection. The first consists in understanding which peculiarities an ethnography should have to grasp engineers' and other experts' underlying modes of knowing and doing *inside* de lab. The second, more analytical, consists in identifying the indicators revealed by that ethnography which may promote an interpretation of how these modes simultaneously mirror

Resumo A medicina de precisão ou personalizada (MP) é uma abordagem biomédica inovadora que pretende prever, prevenir e tratar doenças estudando, à escala individual, o potencial patogénico da associação de fatores genéticos e ambientais. A MP é gerada no laboratório e os seus impactos serão observados fora do laboratório, designadamente, na alteração dos padrões de uso e de acesso aos cuidados de saúde por parte das populações. Tomando a MP como objeto de estudo, os antropólogos são instados a realizar uma dupla reflexão. A primeira consiste em perceber quais deverão ser as peculiaridades de uma etnografia capaz de captar e descrever os modos de conhecer e fazer dos bioengenheiros e de outros especialistas da biotecnologia *dentro* do laboratório. A segunda, de ordem mais analítica, consiste em identificar os indicadores revelados por essa etnografia que podem promover a interpretação da forma como esses

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and resonate a given cultural will located both upstream and downstream the lab — from and to *outside* of it. The purpose of this paper is to reflect on the hypothesis stressing that an ethnographic collaboration might configure an effective way of doing this.

modos de conhecer e fazer se apresentam, simultaneamente, como espelhos e caixas de ressonância de um dado arbítrio cultural situado a montante e a jusante do laboratório — de e para *fora* dele. O propósito deste artigo é refletir sobre a hipótese de a participação em projetos colaborativos poder fornecer as condições para a realização de tal etnografia.

Keywords: Personalized medicine; precision medicine; biotech labs; deep play; participant observation; ethnographic collaborations.

Palavras-chave: Medicina personalizada; medicina de precisão; laboratórios biotecnológicos; *deep play*; observação participante; colaborações etnográficas.

Introduction

As major steps in biotechnological development, the discovery of the protein synthesis mechanism (Hoagland et al., 1958) of the tRNA molecule and the sequent antisense therapy that followed it (Zamecnik and Stephenson, 1978), complemented by the discovery of the PCR mechanism by Kary Mullis' team in the 1980s (Mullis et al., 1986), have transformed both biomedicine and, to a great extent, ethnography's *modus operandi* (Rabinow, 1996; Rabinow and Stavrianakis, 2013). In the first milieu, we are now witnessing the emergence of a new form of biocapital (Sunder Rajan, 2006) built upon a new medical knowledge-power connection — precision/personalized medicine. In the second milieu, the pro-

duction of new 'epistemic things' (Rheinberger, 1997) and experimental collaborations (Rabinow, 1996), which require an ethnography carried out in a 'studying up' and 'at home' (Forsythe, 1999) context, provoked a profound reflection about social sciences' epistemology and anthropology's 'mode of production and being' (Rabinow and Keller, 2016).

Anthropological interest in science and technology is not new. Indeed, although studying 'down', Bronislaw Malinowski stressed in 1925 that 'primitive humanity was aware of scientific laws of natural process [and] that all people operate[d] within the domains of magic, science, and religion' (cited by Harding, 2018: 6). Inspired by anthropological methods and theories, ten years after Malinowski's statement, Ludwik Fleck stud-

ied 'up at home' how scientific facts were constructed in the laboratory and how they were received by different social arenas (cited by Harding, 2018). Together, the two approaches define the major traditional-like trajectories of the anthropological studies of science — ethnoscience, represented by Malinowski, and technoscience, represented by Fleck —, thus defining the poles between which a space remains 'yet to be filled with substantial scholarly work' (González et al., 1995: 868). A lot of work has been produced to do that. Indeed, we have been witnessing some important *tours de force*, such as the feminist critique to the rhetoric of biosciences (e.g. Martin, 1991; Strathern, 1992; Rapp, 2000) and other 'feminist, multicultural, antiracist, technoscience projects' (Haraway, 1994: 61), as well as many works inspired by post-structuralist and post-colonial agendas (Escobar, 1994; Rabinow, 1996; Downey and Dumit, 1997 — see Franklin, 1995; Hess, 2007a; 2007b; Fischer, 2015). All of these works on the anthropology of technoscience contributed equally to fermenting the rehabilitation of cultural anthropology's epistemology after the *Writing Culture* crisis (Forsythe, 1999; Marcus, 2002; 2007). It is now widely accepted that those works 'disrupt[ed] the traditional fieldwork story' (Forsythe, 1999: 6). It is not the case, though, that ethnographic writing (and its then criticized production of related tales) has lost its central place in anthropological modes of inquiry.

The inquiries into technology and science were the very basis of this change (Forsythe, 1999; Marcus, 2002; Strathern, 2006; Harding, 2018). In PM-related anthropological inquiry, the result of that reflection and those movements was the so-called collaborative turn, which forced an epistemological shift of focus from the Malinowskian fieldwork model — Latour and Woolgar's (1986 [1979]) *Laboratory Life* 'monography' being the paradigmatic example of this model —, to interdisciplinarity and experimental ethnographies. We have witnessed an ethnographic opening to the inclusion of several arenas of scientific and social performativity and intra-actions in the configurations of anthropological knowledge. Another kind of otherness has emerged — the interdisciplinary.

As a matter of fact, since the anthropologist is familiar with the figure of otherness from the very emergence of ethnographic fieldwork as a methodological endeavour, the interdisciplinary otherness found in collaborative projects is not in itself an epistemological novelty (Strathern, 2006), but it gives rise to new forms of relationships between ethnographers and subjects. The transformation of the ethnographic relationship is one of the richest and most complex epistemic things which have emerged from collaborative experimentations and it needs to be addressed (Franklin, 1995; Forsythe, 1999; Fischer, 2015; Delgado and Åm, 2018; Harding, 2018; Latimer, 2019).

The transformations which appeared in ethnographic encounters in the context of collaborative interdisciplinary projects in PM-related fields enabled new situations to be confronted and new concepts to be used to respond to the new 'demands of the day' (Rabinow and Stavrianakis, 2013). The case is that, when we look at PM, we seek primarily to study *up*, *sideways* and *through*, which brings important limitations for Malinowskian participant observation, which primarily studies *down* (Nader, 1972; Gusterson, 1997; Hannerz, 2010; Ortner, 2010), especially if we are trying to study experts' work in 'their' biotech world — the laboratory (Viseu, 2015). This world is usually black-boxed, closed and veiled from outsiders' eyes, both literally and symbolically. Additionally, its extreme technical and business-like specialized facilities and functionalities create a heterotopy, an unsituated situation, located somewhere outside the common world, and walled in a kind of citadel. These characteristics of scientists' habitat bring important challenges to ethnographic collaboration, as will be seen later.

Some questions arise here, such as: how shall outsiders in general vindicate access to biotech experts' knowings and doings, that is, their particular methods, or 'knowledge devices',[1] by which they successively make entangled more-than-human nature cultures (cf. Haraway, 1997), and bring them into the social light? And we, anthropologists, how shall we get access to such knowings and doings in order to effectively interpret their place, value and politics in the common world? Facing these questions, in this paper I aim to reflect on the limitations of Malinowskian-like ethnographic endeavour to address in practice this relatively new world of ethnographic collaborations by the interpenetration of two vectors: the peculiarities and difficulties to reach biotechnical experts' modes of knowledge *inside the lab*, and to the integration of anthropologists in a transepistemic arena of research (cf. Knorr-Cetina, 1982), where they may be involved in epistemic partnerships and sharing (Holmes and Marcus, 2008); and the downstream advantages of collaboration to transduce those partnerships' results into practices adopted *outside the lab* by policy-makers and laypersons, who are themselves special kinds of experts (Fals Borda and Rahman, 1991; Holmes and Marcus, 2008). Together, re-

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[1] I refer here to the notion of 'device' from Foucault's 'dispositifs de governmentalité' with a slight evolution promoted by the ontological turn, when it came to signify a particular type of assemblage, or arrangement, namely in Deleuze and Guattari's philosophy. The method as a 'knowledge device' means that it shares a particular characteristic with other forms of power-knowledge, since they all are 'patterned teleological arrangements which assemble and arrange the world in specific social and material formations' (Law and Ruppert, 2013: 229). This definition suggests that the world is full of such arrangements. From this perspective, field-sites are devices too, since they, as well, are 'patterned teleological arrangements' (Candea, 2013).

flections on these two vectors may help us to envision the place that PM-related collaborative interdisciplinary models of inquiry occupy in the *deep play* of modernity and to identify some of the resulting *ethical plateaus* (Fischer, 2004) — such are, I stress, the most important outcomes of collaborative ethnographies on the study of biotechnological worldmaking.

Collaborating inside the lab

PM is developed *inside* the lab. It is being noticed that the 'experimental ethos' is now disseminating across social fabrics (Holmes and Marcus, 2008). Inside the biotechnology lab, this ethos has, in recent years, reflected an unusual exploration of the *jeu des possibles* (cf. Jacob, 1981) by producing specific synthetic-biological hybridizations, thus provoking new arrangements of human/non-human intra-actions (Barad, 2007). Such new arrangements give rise to new modes of social assemblages (cf. Latour, 2005), simultaneously conditioning the change of the *possibles* and boosting the revelation of emergent (Rabinow and Dan-Cohen, 2006; Faubion, 2016) naturecultural hybrid forms, which, in the end, will change social common understandings about the world (Haraway, 2003; 2016) and about how to live in it (Richards and Ruvenkamp, 1996). Consequently, they will challenge the anthropology's knowledge devices and equip-

ment for these to successfully address such change as well (Rabinow, 2003).[2]

Also, the very scope of ethics is challenged by the discovery or invention of such forms (Puig de la Bellacasa, 2010) to adjust to particular emergent modes of what Karen Barad (2003) called 'posthumanist performativity'. This 'quickening of the unknown' calls for an 'epistemology of surprise in anthropology' (Guyer, 2013), which may have the ability to grasp the conditions under which discovery and invention happen, as well as their effects on society. This invites anthropologists to shift the contexts of their endeavour and to agree 'to take knowledge practices in the plural [and to reflect on and to practice] new modes of apprehension' (Strathern, 1995: 3). In order to achieve this ability, an epistemology of surprise needs to embrace an equipment composed by 'the intellectual instruments through which thinking might be facilitated' (Strathern, 2016: 382), aiming to capture the movement space, that is, the setting 'in which both the subject conducting inquiry and the objects and objectives of inquiry are in motion' (Rabinow and Stavrianakis, 2016:

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[2] One of the main limitations of classical knowledge devices use in the contemporary is the fact that humans' biological/bodily dimension is being virtually obliterated from social anthropologists' ethnographies (Ingold, 2016). In addition to the problem of truncation, which clashes with anthropology's holistic epistemological *a priori* premise, this fact brings to the debate the problem of the centralization of analysis on social representations and practices instead of on bodily mediated senses and experiences.

405). This kind of epistemology makes ethnography an experimental system inside a broader experimental system. As Fischer points out, through such motion inside the lab, ethnography becomes 'a differential generator of surprises' (2004: 389). As an experimental system, ethnography will understand the lab as a *third space* that produces prototypes.[3] In this way, ethnography establishes itself 'alongside the traditional serendipitous path of fieldwork and involve[s] explicit intellectual partnerships with persons who might otherwise be viewed as facilitators or subjects of research' (Marcus, 2014: 399). This transformation of the ethnographic relationship is central in collaborative endeavours.

After the genome project and its opening to social engagement via the ELSI (Ethical, Legal, and Social Issues) program, interdisciplinary collaborations were subjected to an update, in order to adjust to new modes of social engagement, being the RRI (Responsible Research and Innovation) model the one now in force. As we'll see, this latter model is being increasingly criticized. One of the main critiques stresses the fact that it seeks to impose a rigid recipe for interdisciplinary collaborations, leaving little

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[3] A prototype 'is a version of a product, or a set of concepts in material form, far advanced in development, but still open to revision, experiment, and some rethinking, based, in part, on engagement with "others" (end users, research subjects, non-experts, amateurs) as inside respondents, if not late-stage partners' (Marcus, 2014: 399).

room for creation and experimentation. Balmer et al. (2016) presented five gold rules for interdisciplinary collaboration: to do collaborative experimentations, to take risks, to make collaborative reflexivity, opening-up discussions on unshared goals, and promote neighbourliness. The authors add that:

> The onus in collaborations tends to be on social scientists to work towards integrating themselves. However, it is important to emphasize the need for scientists and engineers themselves to experiment with how they conduct their everyday work, make knowledge and develop technical innovations as part of an interdisciplinary mix. (Balmer et al., 2016: 741)

So, the scientist and the engineer are the hosts, but they also should open discussion on unshared goals to collaborative experimentations and reflexivity. The 'intellectual partnerships' to which Marcus (2014) refers to go along to this need, meaning that, once *in the lab*, anthropologists should manage their presence by avoiding internal differentiation, promoting, thus, the spontaneous emergence of a lateral knowledge, that is, a way of knowing that 'intends to rethink, adapt, and enact ethnographic method in a novel way that involves a different calculus, recognition, and practice of relations between anthropologists and subjects' (Marcus, 2013: 206). This is an ethnographic approach radically different from that of *Laboratory*

Life, in which the ethnographer was situated in relation to *his* subjects as Malinowski was in relation to Trobrianders. This transformation of the relations between anthropologists and subjects is crucial in collaborative endeavours.

Traditionally, subjects were treated as informants and the ethnographer positioned his/herself outside his/her condition as a means of maintaining a 'distant gaze', or 'strangeness', while, in experimental collaborations, subjects are experts who must be treated as partners, interlocutors or even special para-ethnographers. This way, the distinction between expert and non-expert is diluted and loses its significance, opening, thus, a pathway to the discovery of lateral realities enclosed within the experts' practices and between them and those of anthropologists. The dilution of these differences may perhaps function for the interlocutors as a sign of an attempt to level disciplinary hierarchies, which may lie at the very base of the lack of communication between ethnographers and biomedical scientists, which has been pointed out as a major problem in ethnographic collaborations in biomedical settings (e.g. Prainsack et al., 2010). So, similarly to the provocation of lateral realities, anthropologists may conduct their behaviour in order to grasp and gather their data adopting a strategy which Marylin Strathern (2010) called indirection, i.e., a way to capture reality without directly seeking particular spots or problematic issues.

This kind of management will allow the anthropologist to visualize the adjacencies between the not yet been, the moving being, and the possible becoming that is sequentially revealed along the experimental systems' 'economy of displacement' (Marcus, 2013: 206). It also will allow the anthropologist to contextualize those adjacencies in a broader framework where he/she positions him/herself among complex assemblages and raises new questionings (Rabinow, 2011). Ethnographers, therefore, will be 'able to *observe the observer observing* while having dialogic relationships with subjects within the literal spaces of scientific work (labs, seminar rooms, conferences, bars, etc.)' (Marcus, 2013: 209; italics in the original). This broader framework is the place where *deep play* is played and where *ethical plateaus* are revealed, which I will explore later in this article. As Fischer points out, 'the test of an inventive, illuminating or instructive ethnography is how well it opens such *deep play*, while remaining accountable [read 'ethical'] to both specialist and generalist audiences' (2004: 389).

However, the ethnographer-scientist relationship within collaborative projects does not only have implications for the ethnographer at the epistemological level. In fact, the transformations provoked by the 'collaborative turn' have intense political value and interfere in the very status of the scientist in the laboratory as well as in science's status in soci-

ety. Until a few years ago, science was artificially separated from society. Among other aspects, more related to the question of methodological individualism, this separation promoted a status quo of the scientist who situated his/her activity outside the reach of public scrutiny. The frontier between science and non-science was one of the strongest ideological constructions of scientists, reflecting the defence of a status of immunity to criticism and scrutiny and reifying science by a supposed natural separation from society (Gieryn, 1983).

By integrating researchers from the social sciences into projects on the natural sciences, collaborative research attempts to suture the discontinuity that has traditionally separated the two cultures (Marcus, 2007; Harding, 2018) and, at the same time, it holds the scientist accountable for the social and environmental impacts of the facts he/she produces. The demolition of the frontier between science and non-science implies that scientists 'must give up intellectual control of their research projects' (Harding, 2018: 56). One of the most interesting forms of this withdrawal is revealed by the displacement of the justification of products and of the relevance of scientific activity from facts to social concerns/choices (cf. Latour, 2004). The conditions to produce scientific facts therefore depend essentially on the relevance that society attaches to them. This means that the relevance of science is no longer dependent on

the authority of facts. In other words, the facts produced within the laboratory are no longer exported simply because they have proved to be relevant within the laboratory (Stengers, 2016). The eventual potential disapproval of facts produced in the laboratory means the reduction of the scientist's arbitrariness in determining what relevant scientific facts are (Stengers, 2016) — and this is where collaborative arrangements apparently reflect the change in the status quo of the scientist in society. To a large extent, the negative reactions of scientists to the obligation to collaborate with social scientists (see Prainsack et al., 2010) can possibly be explained by this transformation as a reaction to the loss of total authority over the production of scientific facts.

By having to collaborate with scientists that they have learned to stigmatize as 'lesser' scientists, or even as 'non-scientists', scientists in the natural sciences experience limitations to which they were not accustomed. On the one hand, they see their exposure to the gaze and criticism of the experts on social observation increasing; on the other hand, they realize that, without this exposure, they have no access to public funding packages for *their* projects.

The introduction of anthropology into collaborative projects therefore ends up serving a broader purpose than the scope of the project under development. As Laura Nader recalls, in the context of the contribution of anthropology to the boundaries of power/knowledge,

'[t]he anthropological contribution…is critical to a relocation and a rethinking of the future of Western science traditions at a time when the Western myth of total superiority is shrinking' (1996: xiv). And, she adds: 'the presence of mutual ignorance between scientists and laypersons [leads to the idea according to which much] about science is taken for granted — its bounded and autonomous nature, its homogeneity, its Westernism, its messianic spirit' (Nader, 1996: 1). The political nature of such boundaries reflects on a picture of science as an ideological production revealing the 'scientists' attempt to create a public image for science by contrasting it favourably to non-scientific intellectual or technical activities' (Gieryn, 1983: 781). No boundary is ideology-free, as anthropologists have long known.

So, since science is a social activity — not a purely techno-rational one — it is its integration into a wide global sociocultural context that anthropologists primarily should try to underline (Krautwurst, 2014; Harding, 2015). This second wave of the anthropological studies of science and technology (Hess, 2007a), no longer focused on the analysis of scientific controversies (Hess, 2015), as was the case of laboratory lives and scientists' agonistic fields descriptions (cf. Latour and Woolgar, 1986 [1979]; Latour, 1987). The facts produced inside the lab need to be socially contextualized. As Sandra Harding puts it, 'the new must always be sutured into the old' (2018: 45).

To suture the new into the old is a function which anthropologists must engender. This gives rise to a third wave of the anthropological studies of science, that is, one located between the knowledge of traditional societies' modes of knowing and that more recently focused on the study of the sociocultural context of Western science. The third direction appeals to a linkage between the new and the old and between 'Western science' and the 'Restern primitive mentality'. As Laura Nader underlines, the acceleration of the global flows and *scapes* 'renders the search for a more balanced, indeed more scientific, treatment of disparate knowledge systems inevitable as notions of intermingling idea systems themselves become objects of study and manipulation' (1996: 6–7). To avoid the wound becoming chronic, this third direction would link

> studies of technoscience and other knowledge traditions, focusing on both context and content. Linking the West and the rest erases boundaries or at least makes them less formidable, enabling ethnographers to lay bare Western science practices; linkage encourages mutual interrogation. (Nader, 1996: 6)[4]

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4 I want to use this classification 'West/rest' essentially in its operative value, that is, as it hyperbolically represents both dichotomies new/old, modern science/laypeople's knowledge. So, I invite the reader to understand this 'West' and 'rest' as geographical hyperboles of such different modes of doing and living science (these being formal

This problem is not solved, yet — as neither is the older 'two cultures' problem. As a matter of fact, I think that the science/culture divide and the epistemological individualism are the same problem. And, thus, yes, interdisciplinary experimental collaborations may help to suture the wound.

The very cleavage between the two sciences (cf. Snow, 1959) is a symptom of a more profound, radical difference between two republics — the Republic of Science and the *res*-public of people (Fuller, 2000). This is a subject that goes much farther than the horizon I want to reach here. The important thing to retain is that the transformations imposed by non-experimental, formal collaborative modes of inquiry are themselves a product of an attempt to managerialize the production of knowledge in order for it to be accepted by both industry and the public in a general, quasi superficial way (Strathern, 2006). The fact is that this apparent consolidation of the audit cultures brings some other more localized and atomized concerns into play. The trading zones between disciplines in a given fact production context are yet to be set, and it is not easy for ad-hoc instruments such as the Responsible Research and Innovation (RRI) model to solve both the problem of public access to science (Di Giulio et al., 2016; Delgado and Åm, 2018) and the problem of interdisci-

plinary cohabitations (Felt, 2009). Felt and Fochler's notion of 'epistemic living spaces' encompasses very well the idea I want to point to as an expected function for collaborative projects to achieve a real culturally competent science (and science politics):

> By epistemic living spaces, we mean researchers' individual or collective perceptions and narrative re-constructions of the structures, contexts, rationales, actors and values which mould, guide and delimit their potential actions, both in what they aim to know as well as in how they act in social contexts in science and beyond. (Felt and Fochler, 2012: 136)

The RRI's underlying idea is apparently innocuous. However, it becomes more problematic when one looks at how the model is applied when one seeks to construct such 'epistemic living spaces'. Delgado and Åm (2018) show an interesting reflection on the limitations of such an application. Although the principle of the integration of social knowledge into the scope of scientific projects (at least remotely) at an early stage has been accomplished (at least in part), there is no certainty that the RRI model will attain its goals from that very early stage. The fact is that any attempt to formalize a social relationship in a collaborative endeavour by a ruler-like measure — especially when this is a new phenomenon and where we are dealing

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systems of fact production or informal activities of being-in-the-world).

with a strong and long-lasting difference between researchers' epistemic cultures — is a reductive way to put the question, as mentioned above (cf. Balmer et al., 2016). Delgado and Åm (2018) identify three challenges in RRI interdisciplinary collaboration which will necessarily impact the project's implementation and its supposedly socially inspired outcomes: newness, complexity, and indeterminacy. Together, these challenges reaffirm the call for an experimental approach in collaborative projects, that is, a condition of openness for the projects to develop towards the ideal of the social good (being this a necessarily situated concept, as the authors point out). Despite its 'performative capacity' (Ribeiro et al., 2017: 81), the RRI model of collaboration needs to have an experimental inspiration.

So, connecting experimentation and collaboration, and associating this connection to our ethnographic way of working, a composite and highly performative ethnographic collaborative experimentation may be achieved. As was already seen, ethnographic collaborative experimentations are open modes of ethnographic inquiry which produce (and forecast the possibility of the emergence of) third spaces. Their underlying philosophy is similar to that of experimental entanglements, i.e., they focus on the level beneath disciplinary epistemological differentiations to grasp the common ground that unifies all human kinds of knowledge production (Rheinberger, 1998). Looking

at this level of epistemic pre-individualization (cf. Simondon, 2005), we undress our particular impediments which hinder our attention and vision. The case is that we must look to the products of those entanglements as emergent forms of knowledge that neither party in the inter-type relationship was expecting. In other words, experimental entanglements will lead to looking simultaneously to the pre-determined forms of knowledge production and to the products of collaborative intra-actions and their always problematic *natures* (Barad, 2003; 2007; Rheinberger, 1998). As Fitzgerald and Callard put it, drawing on Rheinberger's experimental systems theory:

> [I]f we want to understand, or, indeed, help foment, the formation of new knowledge practices, we should not — as much discourse under the 're-gime of the inter' does — focus our gaze at the scale of disciplines or paradigms. Rather, we should… be alert to: 'the digression and transgression of smaller research units below the level of disciplines, in which knowledge has not yet become labelled and classified, and in which new forms of knowledge can take shape at any time… novelties generated in one system can quickly spread and create effects at other places ([Rheinberger,] 2011: 315).' (Fitzgerald and Callard, 2015: 17)

We, social scientists, are aware of this (or should be). It is perhaps significant to

this discussion to recall that Hans-Jörg Rheinberger, Karen Barad and Gilbert Simondon, who helped to reflect on the very nature of experimental entangle-ments, came from different areas of ex-pertise, like mechanics, physics and his-tory — and all of them achieved a single, although rich and variegated, perspec-tive on the necessity of an epistemic-ontological confluence. Perhaps, then, we really need more humility, which Delgado and Åm, relying on Sheila Jasa-noff's appeal, see as 'a necessary element of collaboration' — humility 'in the sense of acknowledging one's own knowledge limits' (Delgado and Åm, 2018: 6). Sheila Jasanoff extends this need to the politi-cal level of science and technology regu-lation when she says that:

> [G]overnments should reconsider ex-isting relations among decision-mak-ers, experts, and citizens in the man-agement of technology [and science, I would add]. Policy-makers need a set of 'technologies of humility' for systematically assessing the unknown and the uncertain. Appropriate focal points for such modest assessments are framing, vulnerability, distribution, and learning. (Jasanoff, 2003: 223)

Thus, the emergence of new prac-tices of knowledge challenges any models of scientific governance — and therefore, without humility, any of those models are nothing but political instru-ments of which the main function will be to capture discourse in order to make it a part of the bigger and potentially all-en-compassing 'scriptural economy' (cf. de Certeau, 1984). Ethnographic collabora-tions are precious ways to prevent such discursive totalitarianism. Due to their flexible nature and capacity to promote reflexivity, they situate the focus in the right place: in-between those who pro-duce science and those who consume it, rightly linking technoscience and ethno-science (Nader, 1996). We must decolo-nize science (Mignolo, 2018), both inside and outside the lab. Even if a few deny the evidence, almost all of us see the per-nicious effects of a scientific and techno-logical development without ecological validation before implementation. An extended debate on the relevance and effects of scientific facts on human and non-human world's lives is not an option — it is an obligation. Having the humility to recognize that is the start for a world otherwise. And a world otherwise neces-sarily comprises addressing the pressing issue of informing the public about sci-ence and technology, namely, in the case I am reflecting on — PM. Populations need to know how biosocialities (Rabi-now, 2005) (will) impact their lives, since PM-related science and technologies are producing many new languages, which

> include those of clinicians, scientists, patients, policy makers, parliamentar-ians, journalists, academics, activists, and lobbyists — to name only a few. Since choices and decisions are being

made, we might as well learn what we can about them by documenting and analysing the languages, concepts, principles, emotions, and experiences that give them shape. (Franklin and Roberts, 2006: 77)

This enterprise is not easy. Collaborations inside the lab will not succeed if the anthropologist's work ends when he/she is 'outside again' after 'being inside' the lab.

Collaborating outside the lab

PM is f(o)unded and implemented *outside* the lab. As I see it, any experimental milieu is a socio-technical assemblage inside a much broader social system where para-ethnographic discourses and reflections are sometimes even simultaneously common and heteroglossic. Discourses have a crucial role in the understanding of social reality and in linking experts and citizens (Bakhtin, 1981; Harré and Gillett, 1994; Fischer, 2003). That said, the excessive focus on practices, as Law (2011) advocated, reveals a partial, thus biased, reality. We face here a concomitant problem, I think, one that theorists and researchers in the medical anthropology of the contemporary, or even in social studies of science and technology, must address more deeply — the problem of two discourses, or even the ambivalence within the mainstream discourse on technoscience, namely biotechnology, which

is marked by an ambivalence of economic and social values (Klecun, 2016) or even a dichotomy between normalisation and diversification (Kaufert and Kaufert, 1996). Such ambivalence points out the terms in which the *deep play* is played, since it dichotomizes the interests of technology developers and those of the public. While the former's discourses reflect an ethics of normalization through expressions like 'rates and ratios, survival times, the calculation of risk, mortality and cost-effectiveness' (Kaufert, 2000: 166), those of the latter refer to the central ethics of salvation, a kind of a soteriology revealing that persons live and interpret biotech and biomedical social roles differently. It is by means of discourse analysis that we may envisage the potential unethical issues enclosed in the ideology that underlies the contemporary neoliberal *deep play*. This duplicity and ambivalence of PM-related discourses refers to a double understanding of the implications of biotechnical construction of health and illness on the adjacencies between the not yet been, the being and the becoming, whose configurations determine the broader framework where the anthropologist positions him/herself between complex assemblages and where he/she raises new questionings.

In order to grasp the broad spectrum of the implications of PM in society, we must thus extend the case towards the exterior of the lab, both upstream and downstream, that is, collaboratively ob-

serving the discursive formalization of biotech researching protocols and the way these are framed and included in the rhetoric of innovation and disruption (Lepore, 2014). In the end, the primacy of the interest, that Riles (2015) refers to as an imperative for collaboration, is rooted in this value-creation neoliberal principle. If it is certain that we must moderate the critique *inside the lab*,[5] it is also certain that we must be cautious about the value-creation principle as a crucial constitutive element of post-modern and neoliberal *régimes de véridiction*. So, critique must appear on the page, even if it is also *deferred* along with other formalities of experimental collaboration projects.

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[5] It is understood somewhere that the resistance from the upper echelons to the acceptance of the anthropologist getting into their fields in order to do participant observation is due to these latter reactive attitudes to the critique. It is also stated that, particularly in the post-modern moment, anthropologists have exaggerated their critique, turning it more into a denunciation than a scientific analysis. The upper echelons' reactive attitude may reflect this unfortunate vice that some of us exaggerated and may even have contributed to anthropology's crisis. It is up to us to recover their trust, namely showing them that our work is reliable, and it is based on scientific criteria and less on moral ones. Anthropologists must thus avoid turning into 'moral voices' (Dullo, 2016) propagating a romantic populism, which 'intersects with intensified academicism in the form of arch-scholarly performances of would-be radicalism' (Heyman, 2016: 182). Selecting subjectivities using moral or other ideological criteria is always a dangerous thing to do, especially when we seek to address political-ethical arguments. In fact, we must keep in mind that ethnography 'is far from a democratic or egalitarian method' (Franklin and Roberts, 2006: 92).

That said, to extend the case both up and downstream of the lab, I propose that we consider transduction a main function of ethnography, in agreement with Helmreich's process of constitution, structuring and modification of spatial and logical relations between different forms of experience (Helmreich, 2007). Inside necessarily transepistemic collaborative projects, we are, in fact, confronting a radical linguistic difference between constitutive epistemic communities that is not resolved by translation. Helmreich draws, among others, on Gilbert Simondon's theory of individuation, according to which living creatures and non-living objects evolve or decay towards a final form (Simondon, 2005). Along this process, information moves from stage to stage without its quality as information being altered (only its mode of existence changes). This is the central idea I want to adopt from now on. When I speak about accepting transduction as ethnography's main function, I am referring to a combination of Simondon's transductive flow of information with the biological and chemical processes of communicating between different kinds of cells or other biosemiotic corpora. In a collaborative fieldsite, we can imagine all the experts, including the anthropologist, as different such kinds of biosemiotic corpora, which, in the end, appear as different forms of information processors. That is, all experts share a common nature, but they are still different in their special functions. As a transducer, the ethnogra-

pher performs a function similar to that of biochemical ligands, as he/she transfers information between agents and between (science) cultures. Furthermore, through collaboration, the ethnographer can increase the transductive effect by linking discourses, sensations, experiences and practices, that is, arrangements of information, between different forms of fieldsites, such as conferences, laboratorial experimentations, and society at large. These three main levels of information circulation will then be linked throughout ethnographic transduction, and, since they are taken together, they configure one and same mode of (cosmological) experimentation: collaboration (here broadly understood as a means for adaptation). It is this cosmological dimension that ultimately makes collaboration anthropologically meaningful.

Among all the forms of ethnographic experimentation, the biotech-related is one of the most complex. As Fischer recalls, 'the spaces of interactions among [the] technosciences become particularly complex and interesting sites for cultural analysis — not only for understanding emergent technologies themselves but also, more importantly, for tracking implications carried over into culture at large' (2007: 38–39). Such *sites* constitute a 'network of transductions [that the ethnographer helps to make] audible, visible, perceptible, and even, sometimes, democratically subject to accountability' (Fischer, 2007: 42). It is through such

democratic accountability that we arrive at PM's very destination: society.

Significantly, Michael Fischer listed four features that the anthropology of science and technology combines in its performances and products which may help to carry on the enterprise of linking worlds: a 'commitment to opening the "black box", a 'global perspective that does not limit itself to Western Europe and the United States', a 'set of methods that can deal ethnographically (at the required close-up scale)' and 'artistic and literary aesthetics' (Fisher, 2015: 182–183). These four features provide a strong source to go 'alongside the life sciences' (Latimer, 2019) as well alongside populations to build a world otherwise. Fisher generously offers four reference figures to transduce technoscience's 'legacy knowledge into public futures', that is, to make discourses flow and fill the interstices between disciplinary narratives: 'test drives and libidinal drives', 'protocols and networks', 'landscapes or ethical plateaus', and 'knitting global moieties split by the cold war' (Fisher, 2015: 183–184). Each of these 'genealogical cables [is] made up of many wires or threads, feed into the anthropology of science and technology's translation of legacy knowledges into public futures, buffering future shock, and tracking emergent forms of life' (2015: 184). In short, these 'cables' permit problems to be coped with by means of both curative and projective attitudes. This speculative potential of anthropology of science and technology allows us

to collaborate not simply as observers but also as critical designers (Delgado and Porcar, 2013). Stressing Fischer's figures, I find that 'test drives and libidinal drives' are especially suitable to *figure* this kind of work in PM-related ethnographic collaborative experimentations. For instance, this figure draws our attention to the fact that we are witnessing 'undergoing epistemic revolutions' which are signalized by two main epistemic changes — 'the molecular biology revolution and bioecological imaginaries' (Fischer, 2015: 183). A peculiarity of these PM-related 'test drives' is the fact that such revolutions alter 'the old idea of vitalism (i.e., that life could not be reduced to the laws of chemistry and physics) to the more contemporary recognition that indeed one can synthesize living tissue' (2015: 183). This radical transformation brings laboratorial scientific facts directly to populations' minds, since it both clashes with some ancient cultural and religious premises and feeds some fictional scatological or progressive futures. So, it is *natural* that these epistemic revolutions produce fear as well as desires, the former being more attached to laypeople and the latter to venture capitalists. These are the two sides in dispute. Nevertheless, they have an identical underlying interest — to construct a world otherwise. And it is the way of doing this that forms the significant controversy of our times. Instead of focusing the attention on controversies between scientists or between scientists' networks, an anthropology of science and

technology must primarily address the fundamental controversy between scientists and populations, in which other actants and agents are also involved (among them industry and state). How to link these two sides is the ongoing and future anthropological enterprise in matters of technology and science. It is also through such a linkage that the figures mentioned by Fischer are addressed. Thus, addressing 'protocols and networks' implies critically analysing the modes of doing of technological infrastructures without which the epistemic revolution will not reach society at large (here treated more as an ensemble of consumers), as well its respective impacts on populations' and individuals' lives. Signalising and describing local dramas and observing how those transformations and apparatus create ethical dilemmas, whose ontologies are scattered throughout society, associating several forms and including several standardizing devices, is a kind of a linkage that needs an experimental ethnographic ethos. Such an ethos will necessarily combine observational multi-sited descriptive ethnographies and interdisciplinary collaborative forms of engagement. Only by such means can anthropologists capture the emergence of the 'new landscapes or ethical plateaus' which emerge from the redesigning of the relationship between ecologies of general and local knowledge. Another and final form of anthropological linking is the knitting of 'global moieties'. This is the plan in which anthropology shows its very uni-

versal dimension. How do discourses and materials spread throughout the world? The production and dynamization of global flows is not a culture-free process. There are switches, transducers and translations (Fischer, 2015) on the way. How, for example, PM scientific facts reach an 'exotic' place is surely a matter of concern, especially when we note that such 'exotic places' are being produced (or always have been) inside our own societies. In fact, there is a clear gap between 'triumphalist development schemes and their down-geared implementation' (Fischer, 2015: 183) — and this is a gap which an anthropologist who seeks to link worlds must fill. So, it becomes clear that an analysis of 'the convoluted, discontinuous linkages between what grows inside the castle walls and what grows outside' (Martin, 1998: 32) is a paramount issue in the anthropology of PM-related worlds. An anthropology of the production of PM-related worlds can't limit its horizons to the walls of the citadel. There are no laboratories located outside the social world; the traditional presentation of such culturally and socially free apparatuses was just that — a social construction. The same happened when the clinic was born with its correlated hospital institution located outside the world (Foucault, 2007 [1963]), and the same was criticized by Zarathustra when he encountered the ascetic who lived on the mountain as a means to get closer to the humans (Nietzsche, 2005 [1883-1891]). It is, then, a second order construction to

(supposedly) anthropologically analyse laboratory life as a stranger, even when we agree that there is no such thing as a technical/social divide (Knorr-Cetina, 1995).

There are many projects to bring scientific knowledge to society (e.g. Nunes, 2012; Viseu and Maguire, 2012; Martin et al., 2015; Di Giulio et al., 2016; Carvalho and Nunes, 2018; Fasanello et al., 2018; Nunes et al., 2018; Pickersgill et al., 2019). Among others, the anthropologist Christopher Toumey has been especially concerned with this arrival. His studies focus mainly on the conditions by which nanotechnology is understood by para-ethnographers such as laypersons. His work is founded on experimental collaboration and he has been involved in projects like South Carolina Citizen's School of Nanotechnology (SCCSN), whose main goal is to detect what society at large knows about nanotechnology and its implications. In the project, bioengineers, teachers and other lab technicians collaborated, as well as the public and the ethnographer. Toumey (2016) found that people with different backgrounds and interests see nanotechnology differently; hence, there is no unique definition of nanotechnology. This kind of interpretive difference, Toumey (2011) argues, results from the fact that public engagement with nanotechnology is barely developed. During the SCCSN project, Toumey accidentally found that this detachment was caused by the mode by which information was

being communicated. He concludes that

> the process of building public un-
> derstanding must not be a one-way
> communication from active experts
> to passive laypersons. On the contrary,
> it must include ways for laypersons
> to express their questions, their con-
> cerns, and their values, and for them
> to receive responses from experts.
> (Toumey, 2006: 29)

The kind of collaboration in which Chris Toumey was involved shows a means to identify ethical plateaus that we wouldn't detect otherwise. Here, too, collaboration proves to be an effective toolkit. Toumey situated himself between experts and between them and laypersons and has transduced information through dialogue in a two-way communication process. In brief, theory, laboratorial practices and participated implementation were linked by a workplan that became possible by means of collaboration. Eventually, collaboration is the only way of achieving such an ending. By linking all the parties, known as stakeholders, in the gospel of innovation (Lepore, 2014), collaboration plays a paramount role in bringing technology developers and users closer, thus promoting democracy. And this is particularly achieved thanks to the versatility of the anthropologist's role, who, as a biochemical ligand, links different modes of information processing.

Conclusion

The first anthropologically guided ethnography inside a biotechnological laboratory — that of Latour and Woolgar (1986 [1979]) — was carried out due to a confluence of several factors, mainly informal, including the opening of a laboratory and a struggle between two Nobelists, one of whom invited Bruno Latour to conduct the fieldwork. Another famous ethnography in biotech laboratory is that of Paul Rabinow (1996), who complained that he was seen by his fellow scientists as an ethicist. In recent times, laboratory studies lost a great part of their ethnographic interest for anthropologists, especially since the criticism of *Laboratory Life* (e.g. Knorr-Cetina, 1982; Lynch, 1982; Amsterdamska, 1990) and with the emergence of multi-sited ethnography in response to the global and transnational flows of the anthropological objects. Together with the crisis of representation, fuelled by the *Writing Culture* (see Marcus, 2002), these events weakened the consistency of the exotic Other metaphor — and, consequently, led to the weakening of ethnographic 'strangeness' (Bhabha, 1994; Marcus, 1995; Appadurai, 1996), as used by both Latour and Woolgar (1986 [1979]) and Rabinow (1996).

The second wave of science and technology studies emerged from traditionally peripheral approaches and was greatly influenced by anthropology's *modus operandi*. After the crisis of repre-

sentation, a new impulse powered the emergence of an anthropology as cultural critique of the contemporary (see Marcus and Fischer, 1999), which began the experimental moment in anthropology's epistemic forms. It was in this context that 'anthropologists…found that their new collaborative methods ultimately produce[d] more interesting insights [than the traditional ethnographic modes of inquiry]' (Riles, 2015: 169). This is true especially when the ethnographer — despite the 'view from afar' strategy — wants to move into the biotech lab while remaining a well differentiated and contrastive observer in relation to the informants. Effectively, in this type of fieldsite, classical ethnographer/informant differentiation should not be tolerated. Indeed, based on the many reflections presented above, considering the difficulties of doing participant observation in lab-type sites, collaboration seems to be the best path to follow. Through collaborations based on interlocutor/interlocutor relationships, both the ethnographer and the biotech engineer experimentally construct lateral realities, and, thus, they start performing a first-level transduction, making the anthropologist's work easier in performing successive transductions closer to other disciplines' ecologies and, in the end, closer to society at large.

So, instead of being, from the classical ethnography perspective, observers and informants, respectively, anthropologists and natural scientists or bioengi-neers should be dialogical interlocutors. And it is up to the anthropologist to make the transformation of the ethnographic relationship from a complementary to a symmetrical one. At least, this will facilitate the emergence of an effective communication between the different ecologies of practice. For the scientist, such symmetry will serve as a means for him/her to trust in the anthropologist's work, since he/she shall feel that, in a symmetrical relationship based on mutual understanding, there is no place for excessive critique. For the anthropologist, he/she will understand that such symmetry is fundamental as a milieu where different kinds of vocabulary have the opportunity to converge and, consequently, to be transduced in outsiders' discursiveness, thus opening up the hermetic alchemy-like lab to the wider social world. In the process, it is not the relative experiences of the encounter between interlocutors that count — it is their sense, as Paul Ricoeur (1976) would say.

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