BETWEEN "CHAOS" AND "ORDER": THE CZECH ANTHROPOLOGIST AND HIS EXPERIENCE IN THE CHANGES OF TIME[1]

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Abstract

The main purpose of this text is to show how complex and long the disciplination or disciplinary practice of an anthropologist conducting his fieldwork in non-European areas can be. In the first part, the text is very much retrospective. I focus on my long formative period, during which my ideas about becoming a full-time ethnologist[2] were born in a kind of unconscious vacuum. I revisit my first field entry among the Tarahumara (Rarámuri) people of northwestern Mexico and attempt to bring the reader up to speed on my determined efforts to reach out to native communities and my *chaotic* actions during this first field experience in a non-European setting as a then third-year ethnology student. This will be followed by a reflection on my next two field researches among the Tarahumara in 1996 and 2001, the first of which resulted in an M. A. thesis and the second in a dissertation. In this section, I try to show a certain shift in the approach to fieldwork, which was no longer mere chaos, but led to a more systematic organization of fieldwork findings and their elaboration into a more extensive

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[1] This text is an expanded and significantly revised version of the paper entitled *Field Research on Two Continents: The Anthropologist and the Changing Roles in the Field*, presented at the workshop *Research in Non-European Areas: Methods, Techniques, Problems, Challenges* on 18 October 2018 at the Centre for Ibero-American Studies (SIAS), Faculty of Arts, Charles University in Prague. The paper was based on my fieldwork in northwestern Mexico and on the east coast of Madagascar. For the purposes of this text, I have chosen to reflect only my research among the Tarahumara, as I have since returned to them in September–October 2021 in order to prepare the ground for a future new anthropological project, which I briefly summarize in the final section of this article. I would also like to take this opportunity to thank both anonymous reviewers for their valuable comments and recommendations that helped improve the entire manuscript. However, I alone am responsible for all errors and inaccuracies.

[2] If I use the terms anthropologist, ethnologist, anthropology, ethnology in this text, I will use them as synonyms, although I am of course aware of the epistemological differences between these terms, which have been and are the subject of professional debates. For the purposes of this article, which is more methodologically oriented, I do not consider it expedient to explain these differences. However, if the reader is interested in this debate, I refer to, for example, the following articles: Zdeněk R. NEŠPOR – Marek JAKOUBEK, "Co je a co není kulturní/sociální antropologie? Námět k diskusi" ["What is and what is not cultural/social anthropology? A Framework for Discussion"], *Český lid* 91/1, Praha 2004, pp. 53–79; idem, "Co je a co není kulturní/sociální antropologie? Závěr diskuse" ["What is and what is not cultural/social anthropology after two years. Conclusion of the discussion"], *Český lid* 93/1, Praha 2006, pp. 71–85; Nikola BALAŠ, "Čím vším může být sociokulturní antropologie" ["What can sociocultural anthropology be"], *Český lid* 103/3, Praha 2016, pp. 473–490.

qualifying thesis and several technical studies. While in the first part I go back into the deep past in order to show the complexities that a budding ethnologist can or must deal with if he wants to penetrate a completely different environment from his own, in the second part I discuss some of the methods of field research that I have not been familiar with in the past, or have used unconsciously or without better understanding of them. These I find useful in current and forthcoming return research among the Tarahumara. They resonate strongly in contemporary anthropology, and are constantly being refined. In this section I will also outline, with respect to methodological horizons, my current planned research project focusing on the human relationship to biodiversity in the context of environmental and climate change, which is increasingly impacting (not only) Tarahumara communities.

Keywords: Mexico; Sierra Tarahumara; Tarahumara; methodology; ethnography; field research; global ethnography; extended case method; glocal ethnography; multi-sited ethnography; reflexivity.

1. Dreams, ideals, excitement

Dreams, ideals, excitement: these words are perhaps the best description of the period preceding my first purposeful research trip to the Tarahumara people in the state of Chihuahua in northwestern Mexico. I had long dreamed of one day going to places that I had only read about for so many years, known about from adventure and travel films, or from sporadic lectures by those who had the good fortune and courage to visit the various Indian groups. My first "guides" to the remote regions of Central and South America and sub-Saharan Africa were writers, led by Karl May, James Fenimore Cooper and Jules Verne, later travelers Jiří Hanzelka and Miroslav Zikmund, and ethnographers specializing in indigenous cultures: Miloslav Stingl, Václav Šolc and Mnislav Zelený. The last mentioned author even lectured to my classmates and me in our first year about the native groups of all the Americas, and I remember that we most appreciated Zelený's account of his many trips, especially to the various ethnic groups of the Amazon, on which he is undoubtedly the greatest expert in our conditions.

My childhood ideal, like that of many of my peers, was the fictional character of the Apache chief Vinnetou rather than the real-life chiefs and warriors fighting against the United States Army, such as Tecumseh, Osceola, or Sitting Bull, about whom I learned in nonfiction or adventure films. Although I also dreamed of other lands I would like to visit to learn about native life, my clear goal was to travel to the places where my real and fictional native heroes lived. In particular, indigenous territories in Arizona, New Mexico, and Texas became the target of my dreams and idealistic plans for future ethnographic travel. Unlike most of my friends, who lost their childhood ideals over time and turned to more pragmatic pursuits, my desire and dreams of exploring the life of remote societies intensified in early adulthood. However, through a confluence of circumstances, I abandoned the idea of exploring the Apache, Navajo, or other native groups of the American Southwest and moved a few hundred miles south to where Mexico, or more precisely the state of Chihuahua, lay. Deviating from the original long-standing intention to conduct research in a dream field is nothing new in anthropology. The famous anthropologist of Czech origin, Leopold Pospíšil, once found himself in a similar situation when he

commented on the choice of his first field: "If I could have chosen my field of study, I would probably have gone to the Tarim Basin in Central Asia or the Ruwenzori Mountains in Central Africa."[3] We all probably know that Professor Pospíšil became a world-renowned anthropologist primarily because of his long-term research on the Kapauku in New Guinea, a far cry from his former ideas about the optimal choice of destination for such research.

As we can see, it was easier for me. I did not have to make such a radical change in the choice of terrain, I even stayed in the same area, because the native groups of northwestern Mexico belong, like the Apache, Navajo or Hopi, to the so-called Southwest or Great Southwest.[4] Thinking back on what sparked my interest in the Tarahumara as "my" Indians, I recall three key moments. Probably the very first impulse was reading Miloslav Stingl's slim book *Indiánský běžec*.[5] In it, this legendary traveller and expert on the native cultures of America, Australia and Oceania reflected, among other things, on the incredible running abilities of the Tarahumara people lost somewhere in the canyons, ravines, and plateau mesas of southwestern Chihuahua. As a lifelong athlete who was an avid middle and long distance runner at the time of reading this book, I was absolutely fascinated by much of the information about the feats of these native runners. The second moment was a chance visit to the Votive Church on Roosevelt Square in downtown Vienna in August 1989. It was my first time in the West, and I tried to absorb as much as I could of the history and culture of the places I visited in Austria and Italy during my trip of several weeks. Although I was more interested in older sacred architecture, perhaps out of curiosity or to relax, I entered the Neo-Gothic church. Its interior did not particularly captivate me, yet the dozen or so minutes I spent inside were enough to give me a clear idea of my until then completely hypothetical terrain. In the sacristy of that church there was an exhibition of beautiful colour photographs by the Jesuit missionary Luis Guillermo Verplancken (1926–2003). The main subjects of the Jesuit's pictures were Tarahumara men and women of all ages and situations and their mystical homeland of the Sierra Tarahumara in northwestern Mexico. My visit to the church included a brief encounter, which was however quite crucial for

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[3] Leopold POSPÍŠIL, "Tyrolští rolníci z Obernbergu a otázka dlouhodobého výzkumu" ["The Tyrolean peasants of Obernberg and the question of long-term research"], *Český lid* 84/1, Praha 1997, p. 15.

[4] For example, the North American cultural anthropologist Kroeber situated the Tarahumara in the southwestern range and pointed to their ambiguous position between the Sonora-Gila-Yuman and Mexican cultures see Alfred Louis KROEBER, *Cultural and Natural Areas of Native North America*, Berkeley 1939, p. 40. Hypotheses about the ethnogenesis of the Tarahumara and some other northern Mexican Indian groups are discussed at length in another work see Marek HALBICH, *Ztraceni v kaňonech a na rančích. Sociální a ekologická adaptace Tarahumarů v severním Mexiku* [Lost in the Canyons and on the Ranches. Social and Ecological Adaptation of the Tarahumara in Northern Mexico], Praha 2019. On the inclusion of Sonoran and Chihuahuan ethnic groups in the Greater Southwest, see, for example, Ralph L. BEALS, "Relaciones culturales entre el Norte de México y el Suroeste de Estados Unidos. Etnológica y Arqueológicamente", *Tercera Reunión de Mesa Redonda sobre Problemas Antropológicos de México y Centro América*, Ciudad de México 1943, pp. 191–199; Clark WISSLER, *The American Indian, an Introduction to the Anthropology of the New World*, New York 1917.

[5] Miloslav STINGL, *Indiánský běžec* [Indian Runner], Praha 1969.

my future, with the American musician Romayne Wheeler. I soon understood that the exhibition of photographs and the presence of an American pianist in the Votive Church were related. At that time Romayne had been living in Chihuahua for several years and, through his concerts in Austria and other countries, contributed one-third of his earnings to the Misión Tarahumara run by Father Verplancken. When I emerged from the dark church environment into the heat of Vienna in August. I had a clear idea: if the social situation in Czechoslovakia permitted, I would go to northern Mexico and try to get among the Tarahumara. My dreams and aspirations took on clear outlines, and the very thought of a possible journey to the Indians in the inaccessible regions of northern Mexico provoked and excited my mind.

As is evident from this brief reminiscence of more than thirty years ago, I had no idea that I would find a topic or an interesting issue in particular that I wanted to investigate scientifically. I confess that I was primarily interested in the excitement of the journey and the tantalizing possibility of traveling in pursuit of my dream. *Where* was more important to me than *why*, *how* or *what*. Everything else was secondary, but basically understandable, since I am returning to the time when I was still studying at the Faculty of Education in Ústí nad Labem, majoring in the Czech language and civics, and my immediate future was heading towards a career as a secondary school teacher in my native Liberec, or Ústí nad Labem. With this approach I was, somewhat incidentally, placed among the Ibero-American and even Mexicanist areal specialists. In short, I was not aware of what was obvious to Pospíšil and many other anthropologists, namely, that ethnographic research does not consist of the researcher's selection of a particular geographical area, country or ethnic group, but that it depends much more "on the requirements of the theoretical work itself and not on an ethnographic interest in a particular area".[6]

2. Fear of amoebas, but mostly just *being there*

Although eventually I completed the Faculty of Education course successfully,[7] I did not become a primary or secondary school teacher. My admission to study ethnology at the Faculty of Arts of Charles University became a decisive official step on the road to becoming a full-time anthropologist. Although I threw myself into my studies with great enthusiasm, many lectures and seminars directed me to narrow down my future research plan, I was nevertheless disappointed by the lack of methodological preparation for independent field research.[8] When I first flew

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[6]   POSPÍŠIL, "Tyrolští rolníci", p. 15.

[7]   I expressed my desire for a professional interest in Latin America in my first master's thesis, in which I focused on the figure of the naturalist Tadeáš Haenke (1761–1816) in the context of late colonial history see Marek HALBICH, *Tadeáš Haenke a počátky národně-osvobozeneckého hnutí v Jižní Americe* [Tadeáš Haenke and the beginnings of the national liberation movement in South America], Ústí nad Labem 1990. This work, however, was based solely on research and analysis of the literature and archival material available at the time.

[8]   For a detailed discussion of the development and characteristics of Czechoslovak social and cultural anthropology, or ethnology, see Nikola BALAŠ, *The late socialist Czechoslovak ethnography and folklore studies and its influence on the Czech tradition of sociocultural anthropology after 1989*, (PhD Theses), Prague 2020.

to Mexico in early October 1992, I had completed ten days of fieldwork in the Strakonice region where we would always go out in pairs for a full day to predetermined locations to seek out people with whom we would then conduct impromptu interviews. We then discussed these interviews and partial observations in the field each evening in a campsite on the banks of the Otava River on the outskirts of Strakonice with the two educators who accompanied us at the time. After returning to the department, I summarized the results of this study visit in a fifteen-page essay, for which I received credit at the end of the semester. If I add to this the professional lectures and the valuable advice from an excellent teacher and outstanding expert on Mexico, Oldřich Kašpar, on how to behave in a completely different environment. This was about all I could have before my first research trip.

Somehow, with this qualification, I found myself one day in October alone and without any pre-arranged contact in a small train station in Creel, northern Mexico.[9] My previous visits to Mexican libraries and archives and the trip to Teotihuacán had helped me to acclimatise solidly to the high altitude and the generally different climate, but had not helped me to get a clear idea of what I would be *doing there*. Nor did the extremely long train ride of more than forty hours from Mexico City to Creel, with a change in Chihuahua, do much to help me sort out my thoughts. My mind was filled with differently coloured emotions and feelings, ranging from great fear of the unknown to the excitement associated with the belief that I would finally visit the places where the Indians live. I was hardly aware at the time that this chaotic mixing of feelings was one of the manifestations or phases of *culture shock*, roughly along the lines that the Canadian anthropologist with Finnish roots, Kalervo Oberg, expressed at the beginning of his instructional text on this initial encounter with an unfamiliar environment: "Culture shock is precipitated by the anxiety that results from losing all our familiar signs and symbols of social intercourse […]."[10] Of course, I knew nothing about the scientific concept of culture shock at the time, and many years later, as I reflect back on those first weeks of my Mexican trip, I look back with a wary smile at how I fulfilled the various other symptoms of culture shock that Oberg discussed in his short study. One of these symptoms is excessive hand washing and a great fear of drinking unfamiliar water. Before we left, we were warned many times about the dangerous amoebae found in soil and water that can infest, for example, the intestines or the liver and other internal organs.

If there was one thing I was truly afraid of in the early stages of this first research trip, it was the invisible amoebae,[11] which I knew could harm my body long after

9   On this trip, which was part of the 500th anniversary of the (re)discovery of the Americas, I was accompanied by two of my classmates, Miloslav Pokorný and Jan Menšík, who, after two weeks of acclimatisation in the capital Mexico City, went to the mountainous areas near Lake Pátzcuaro in the state of Michoacán to study some aspects of the culture of the Purépecha/Tarasco Indians.

10 Kalervo OBERG, "Cultural Shock: Adjustment to New Cultural Environments", *Practical Anthropology* 7, New York 1960, p. 177.

11 In Czech nomenclature, they are known as *měňavky*. With hindsight, and taking into account the nature of the terrain I was in for a few months, I believe that the greatest danger to me was the *Pelomyxa palustris*, found in rotting stagnant waters, and the *Entamoeba histolytica*, which can cause unpleasant dysentery after infecting intestinal tissue. A few days before leaving for Mexico, I contacted

I returned home. But as I approached my first destination, the original logging settlement of Creel, named after the early twentieth-century governor of Chihuahua, I had forgotten about the amoebae and similar animals, but the psychological symptoms of culture shock became more apparent. The euphoria of being enchanted by the landscape of the northern fringes of the Sierra Tarahumara was quickly replaced by a gripping anxiety and heaviness stemming simply from the knowledge that I had no idea what was going to happen in the coming minutes, hours and days. But as soon as I descended to the narrow concrete platform of the Creel train station, I suddenly felt a certain relief, perhaps because I did not have to go anywhere else for the time being, but probably because only now, after more than two weeks of somewhat dull and static preparation in university classrooms and several Mexican institutions, I was standing on the very doorstep of a campus inhabited by members of the ethnic group I had come to represent: Tarahumara or Rarámuri, as they are nowadays referred to in the literature and as they call themselves. Although thirty years ago there were several hundred of them living in the city of Chihuahua and other major cities in northern Mexico, it was only in a small square in Creel that I saw the first Tarahumara families and individuals standing or sitting alone, mostly clustered around two small Jesuit churches and a large shop selling the artwork (*artesanía*) of these Indians. Admittedly, I had to fend for myself first, find a place to sleep, and buy some food, but by constantly peeking in on these people, I more or less unknowingly began my first field research.

3. Moving between *here* and *there*[12]

That was not, however, how I approached it at the time; according to my ideas about field research at the time, I still was not *there*. Thus, for me, there was only

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Mnislav Zelený by phone because of my concerns about amoebae, and he was incredulous at my inquiry about overheating water from untested sources. Of course, I could not realise at the time that Zelený's slightly ironic answer implicitly indicated the necessity, or at least a certain readiness, for the inevitability of symbiotic coexistence of a field anthropologist not only with the humans he is supposedly "studying", but with the entire environment that surrounds him, which includes numerous organisms, including amoebae, that are hidden to the naked eye of the researcher. On symbiosis from an evolutionary biology perspective, see Lynn MARGULIS, *The Symbiotic Planet. A New Look at Evolution*, Phoenix 1999; or from the perspective of multispecies ethnography, e.g., S. Eben KIRKSEY – Stefan HELMREICH, "The Emergence of Multispecies Ethnography", *Cultural Anthropology* 25/4, Arlington 2010, pp. 545–576.

[12] The title of this section alludes to Evans-Pritchard's assertion that the anthropologist experiences a double marginality in the field, consisting essentially of a double alienation. The moment the ethnographer enters the field, he becomes a stranger twice over: he ceases to be a member of his community, but he is also not a member of the society he has come to study: "One enters into another culture and withdraws from it at the same time. One cannot really become a Zande or a Nuer ora Bedouin Arab, and the best compliment one can pay them is to remain apart from them in essentials. In any case one always remains oneself, inwardly a member of one's own society and a sojourner in a strange land. Perhaps it would be better to say that one lives in two different worlds ofthought at the same time, in categories and concepts and values which often cannot easily be reconciled. One becomes, at least temporarily, a sort of double marginal man, alienated from both worlds […]" see Edward Evan EVANS-PRITCHARD, *Witchcraft, Oracles and Magic among the Azande*, Oxford 1976, p. 243.

a settlement with an exclusively or majority native population. While I was not naive in the sense that I was looking for a pristine native community, I did not want to be "disturbed" by outside influences, by which I meant tourists, independent travellers, missionaries of all kinds, traders, aid workers, and other actors who would be more prevalent in such a community. With this relatively clear idea in mind, I began my first stay of approximately two months in the Sierra Tarahumara, the southern tip of Mexico's largest state, Chihuahua, and the northernmost tip of the vast Sierra Madre Occidental mountain range that stretched from Central Mexico through Zacatecas and Durango into southern Chihuahua and southeastern Sonora. The Sierra Tarahumara, with its territory of approximately 50,000 km², thus became my broader and largely imaginary terrain, as I had no idea what, if any, indigenous communities I would eventually reach.

I spent the first few weeks in Creel and the surrounding area in the company of a Czech emigrant, Jiří (George) Lev, and his girlfriend at that time, the Canadian, Cynthia Brown, whom I met almost immediately after my arrival. The couple had just been on a trip of several months to the United States and especially to Mexico, and they settled in Creel for a while after Cynthia had been volunteering for a local hotel. For a while we became a group that explored together, especially the canyons and mountain valleys. Thus, at this early stage of my stay, I was nothing more than an ordinary tourist, driving around places of interest with my camera, taking pictures and returning to base. Both of my companions, however, were aware of my ethnographic ambitions, and in retrospect, I must note that they indirectly helped me to reach key people who opened the way to Tarahumara communities. Thanks to them, I was able to meet several local people during this phase, or those who had settled here some time ago. One of these people was none other than the American composer Romayne Wheeler, whom I had met three years earlier in the Votive Church in Vienna. Our first meeting took place shortly before his concert at the "House of Culture" (*Casa de la cultura*) in Creel, where I was invited together with George and Cynthia. Of course, he did not remember our meeting in Vienna, but he invited me to a friendly meeting the very next day at his apartment, which was part of the house to which the audience from the wider area of the region flocked. Among them were often Tarahumara Indians in their typical costumes. One was a young Tarahumara man, Patricio Gutiérrez Luna, from the Mesa Yerba Buena community, the second key figure on the path to my rite of passage, as I would now professionally refer to my own process of *becoming an anthropologist*.[13]

Romayne and Patricio were not the only ones, but in the context of a few weeks of chaotic movement in an area that was reserved more for settled mestizos, tourists, missionaries or various people coming to the sierra for business than for the native Tarahumara, these two were key gatekeepers for me, making it possible for me to go there for the first time. This was the Tarahumara community of Yerba Buena, where

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[13] I mention this episode also because Cynthia Brown later stayed in northern Mexico for a longer period of time, winning the hearts of the people of a Tarahumara family with whom she lived for some time and whom she still cares for to some extent in her role as godmother to one of the daughters of this family, in which I myself lived briefly in 1992 and 1996.

I arrived to do "real" field research in mid-November 1992. If I used the analogy of a rite of passage, I probably initiated its *liminal phase*, since I became for a time part of several Tarahumara *communitas*,[14] in which that power asymmetry between the researcher, who is usually described as a white man from the middle social class, and the indigenous man, who is considered to be from the lower rungs of the social ladder, was lost or even overturned.[15] This asymmetry may be due, for example, to the fact that the ethnographer is aware of his or her economic superiority and comes to the field knowing that he or she is financially able to do so, but psychologically it may be the other way around. This is how I felt during my first practice in ethnography in Tarahumara communities. I was aware that I could help my host family out a little, for example, by buying food to stock up, but at the same time I could not be sure that it would not be hinted, or directly said, that I should leave their company for some reason. These doubts were justified on my part, since a frequent question I heard was: *"How long are you staying?"* or *"When are you leaving?"*[16]

So who was I, or who did I feel I was during this first, "initiatory" field research? I probably did not think of it that way at the time, I was happy to be among the Indians without any visible outside element and I probably did not care about who the locals probably thought I was. Perhaps I thought I was at least on the verge of becoming an anthropologist with a focus on non-European areas, but in fact I was getting into rather different roles. Thomas Eriksen, in his textbook on social and cultural anthropology, in the chapter on methodology and field research, says it fully:

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[14] By the metaphor of *rite of passage* and *liminality*, I am of course referring to the well-known concepts of Arnold van Gennep see idem, *Les rites de passage: étude systématique des rites*, Paris 1909 (first edition in Czech under the title of *Přechodové rituály: systematické studium rituálů*, Praha 1996) and Victor Turner, *The Ritual Process: Structure and Antistructure*, Rochester – New York – Chicago 1969, in Czech under the title of *Průběh rituálu*, Brno 2004. Since both books are now considered classics in social anthropology, I will not explain them further in this text.

[15] Of course, this is far from being the case today, as anthropology has rapidly spread to former colonial countries in Latin America, Africa, Asia and Oceania. However, in some fields and situations this simplistic and overdone dichotomy may still be present.

[16] *"Cuánto tiempo te quedas aquí?"* or *"Cuándo te vas?"* The communication between Tarahumara and me was in Spanish, as I had not managed to learn the Rarámuri language at a communicative level. I only began to learn the native language in Creel from a Jesuit missionary's textbook see José A. LLAGUNO, S. J., *Tarahumar (Adaptación del Método "ASSIMIL")*, México, D. F. 1984, but it was written on the basis of the ethnolect in Norogachi, Upper Tarahumara, and moreover, it was not very useful for daily contact with the natives in the other areas. I can simply state that I was using basic communicative phrases corresponding to so-called phatic or social communication. See Bronislaw MALINOWSKI, "The Problem of Meaning in Primitive Languages", in: Charles Kay Ogden – Ivor Armstrong Richards, *The Meaning of Meaning. A Study of the Influence of Language upon Thought and of the Science of Symbolism*, New York 1923, pp. 296–336; or Ernest GELLNER, *Language and Solitude. Wittgenstein, Malinowski and the Habsburg Dilemma*, Brno 2003, pp. 178–184. I quickly learned these phrases after arriving in Yerba Buena and Coyachique, but after using them I spontaneously switched to Spanish, which especially the men of the younger and middle generations already knew very well. At the same time, in this text I do not discuss the results of my three ethnographic researches among the Tarahumara. These are described, interpreted and analysed in detail in my book HALBICH, *Ztraceni v kaňonech*.

> Many anthropologists unintentionally take on the role of buffoons while in the field: they speak with strange accents and make grammatical errors, ask surprising and sometimes tactless questions, break many rules of conduct […]. Each of us is perceived as more or less a buffoon in a foreign environment […].[17]

I did not find this self-reflection in my old field diary, I was probably very self-centred, perhaps in an attempt to be "objective", i.e., to keep a certain distance from the field I was studying and the people I was observing, interviewing and, at least according to my criteria and ideas at the time, scientifically researching.

4. Returns, epistemological progress, but methodological dilletantism

After returning from my first study trip to the Tarahumara, I realized that if I wanted to write a thesis on these North Mexican Indians, I would have to go back to them. At the time, I came away with many observations and notes, but they were so vague that I could hardly write a serious qualifying thesis based on them.[18] Returning to Oberg's concept of culture shock, I would in retrospect describe my entire first research visit to the Sierra Tarahumara as a *honeymoon phase* consisting of a somewhat distorted and simplified view of the foreign environment and especially of the local people. The fact that I did not get to the next phase, described by Oberg as a *crisis phase*,[19] was probably due to the fact that my stay was relatively short.[20] Briefly, in two months I did not leave the initial euphoria and excitement of my first steps in the field, which was not at all easy. The Tarahumara family of Patricio Gutiérrez Luna and Patrocinio López welcomed me very warmly, I became part of their community for a few weeks and as far as I know the only serious crisis was my stomach problems.[21]

I returned to the Tarahumara after almost four years, having previously travelled for about three months in all the Central American countries except Honduras.[22]

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[17] Thomas Hylland ERIKSEN, *Sociální a kulturní antropologie. Příbuzenství, národnostní příslušnost, rituál* [Social and cultural anthropology. Kinship, nationality, ritual], Praha 2008, p. 39.

[18] From my first trip I at least extracted a few articles in popular magazines and one shorter, more technical text see Marek HALBICH, "Los tarahumares: un fenómeno etnológico del norte de México", *Ibero-Americana Pragensia* 27, Praga 1993, pp. 203–205.

[19] OBERG, "Cultural shock", p. 178.

[20] Some anthropologists report that the honeymoon phase lasts from a few days to several months, which coincides exactly with my first field research. In general, tourists, traders, or seasonal workers seem to return home while still in this culture shock phase, whereas anthropologists, aid workers, or gastarbeiters go through multiple phases during their stay in a foreign environment see Rachel IRWIN, "Negotiating feelings in the field: Analyzing the cultural shock", *Revista Brasileira de Sociologia da Emoção* 8/3, João Pessoa 2009, p. 348.

[21] During my first trip to Mexico, I suffered from traveller's diarrhoea several times, known locally as Montezuma's revenge or Aztec two-step. If I can believe various global statistics, I was among the 30–80% of travellers to tropical and subtropical areas who suffered from travel diarrhoea see Vladimír ŠERÝ – Ondřej BÁLINT, *Tropická a cestovní medicína* [Tropical and Travel Medicine], Praha 1998, p. 227. I remember that I was helped by a small medicine called Lomotil, similar to the well-known Imodium.

[22] At that time, Honduras did not like to issue tourist visas to independent travellers from the former Czechoslovakia, as it was still considered a Communist country in the eyes of politicians at that time.

I therefore arrived in the field with a certain confidence and the knowledge that I had managed to overcome the various pitfalls and dangers arising from, for example, armed conflicts, the existence of guerrilla groups, narcotraffickers hiding in the forests and jungles of the cities, etc.[23] However, I was still studying ethnology, in the meantime travelling extensively as a guide, especially to southern Europe, where I was strengthening my symbolic capital as a "connoisseur" of the Hispanophone environment and continuously preparing for this first return to the Tarahumara. While I undoubtedly made significant progress on the epistemological plane during that four-year period, methodologically I returned to the field again almost untouched. In retrospect I blame myself for this unpreparedness, despite the fact that my second research was relatively successful, in that two years later I was able to write and defend my master's thesis.[24] This, from my present perspective, was somewhat uncritically praised by my teacher Oldřich Kašpar.[25] Kašpar, in a paper which dealt with the theses of students of ethnology at the Charles University Faculty of Arts based on research in non-European areas, highlighted my efforts to take a comparative approach in which I tried to cover, even if only very selectively, some social and cultural aspects of other contemporary ethnic groups of northwestern Mexico.

Leopold Pospíšil, in his previously-mentioned research, also applied the comparative perspective, although on a much broader scale. While his aim was to arrive at a universal theory of law that was not Eurocentric,[26] my own achievement was far more modest. Pospíšil included in his comparative analysis of law some sixty other ethnic groups from all continents, in addition to the Kapauku, Nunamiut Eskimos, Tyrolean peasants and Hopi-Tewa among whom he conducted long-term and shorter fieldwork, whereas my comparisons focused exclusively on the Yutonahuas of Chihuahua and Sonora, and partially on the Hokas Serie (Konka'ak) living in Sonora. I would rather describe my approach as a micro-comparison; I have tried to

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[23] Travel in Central American countries and Mexico was not very safe in the spring and summer of 1996. In many places I encountered the remnants of the long civil wars, and in El Salvador in particular I felt at times as if the conflict was continuing. In southern Mexico (Chiapas) and in Guatemala, again, the Mexican army checked trains, buses and other means of transport at every turn, since at that time various paramilitary groups were hiding in the area, the most famous of which were the Zapatistas of the EZLN (Ejército Zapatista de Liberación Nacional), whose activities resonated even in the two thousand kilometres of Mexico's north-west. In all countries I visited at that time I conducted ethnographic microprobes focused on *ad hoc* selected aspects of a particular ethnic groups (e.g. Emberá and Wounaan in Darién, Panama, Bribri in Talamanca, Costa Rica, Subtiava in Nicaragua, etc.).

[24] Marek HALBICH, *Charakteristika vybraných fenoménů tarahumarské kultury se stručnou komparací s ostatními nativními skupinami severozápadního Mexika* [Characteristics of selected phenomena of the Tarahumara culture with a brief comparison with other native ethnic groups of northwestern Mexico], Praha 1998.

[25] Oldřich KAŠPAR, "Diplomové práce z oboru mimoevropské etnografie v Ústavu etnologie Filozofické fakulty Univerzity Karlovy v Praze v letech 1989–1999" ["Diploma Theses in Non-European Ethnography at the Institute of Ethnology, Faculty of Arts, Charles University in Prague, 1989–1999"], *Český lid* 87/1, Praha 2000, pp. 61–66.

[26] Leopold POSPÍŠIL, *Etnologie práva* [Ethnology of Law], Praha 1997, pp. 111–113.

reveal, at least superficially, the relationship of each group to modernity and their degree of acculturation and assimilation into mainstream Mexican society.

But in fact, even during my second stay, I did not cross the shadow of stationary field research, as for two months I again stayed mostly in the Tarahumara communities of Coyachique and Retosachi in the Ejido Munerachi area and made comparisons only on the basis of a thorough study of the available anthropological and other literature on the northern Tepehuan (Ódami), the two Pima (O'odham) groups, the Varojío (Macurawe), the Yaqui (Yoeme) and others. My mobility was limited to the two settlements mentioned above, which did not allow me to analyse, for example, the social relations between some Tarahumara and mestizos from the district centre in Batopilas, but at least I could register major differences in social, cultural, economic, religious or ecological terms within the same ejido.[27] This second stay, which I spent mainly in the family home of Patrocinio López in Coyachique and in the home of Romayne Wheeler in Retosachi, was important for my future direction in that I realized the necessity of stepping out of one relatively limited research field, which for me was mainly the several communities within the ejido, or the ejido as a whole.[28] I had set my sights on more modern research methods that transcend the long-term staying in one community (e.g. Yerba Buena) or one area (e.g. Ejido Munerachi or Batopilas district). Although, despite a certain methodological dilettantism, I did not equate stationary research with mere data collection, as was and perhaps still is common in certain circles in the Czech academic environment.[29]

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[27] It is a very complicated form of small or mini-fund ownership, a specific non-capitalist type of relations of production that is a product of the Mexican Revolution of 1910–1917 see Roger BARTRA, *Estructura agraria y clases sociales en México*, Ciudad de México 1974, pp. 129–130.

[28] In this case, I borrow the term *field* from Pierre Bourdieu: "I describe the global social space as a field: by this I mean a field of forces whose pressure is necessarily subjected to the actors involved in that field, and at the same time a field of struggle in which the actors clash by various means and for various ends – depending on their position in the structure of the force field – and thus contribute to its preservation or change […]" see Pierre BOURDIEU, *Teorie jednání* [Theory of Action], Praha 1998, p. 38. Like Bourdieu, I understand by the term field the social space within which individual social actors create, maintain or expand their social position. Bourdieu meant, for example, the scientific, or academic, political, artistic, or sports field. In the context of my field research, I believe I am expanding or modifying this original concept a little. Thus, the social space under investigation for me was not only the ejido, i.e., the bounded political, economic and ecological entity, but all the space in which the social, political, religious and other interactions of all actors whose mobility goes beyond the boundaries of the ejido take place. An example of such a wider social field of the Munerachi's Tarahumara may be the *ritual compadrazgo*, which in some cases may, among other things, reinforce the power field of some persons socially related to the momentary political leaders of the district centre see Marek HALBICH, "Ritual compadrazgo as an Instrument of Interethnic and Social Adaptation among the Rarámuri in Northwestern Mexico and its Possible Correlations to Local Political Events", *Urban People* 8/1, Prague 2010, pp. 331–384; HALBICH, *Lost in the Canyons*, pp. 246–280. This field of power is, or can be, highly unstable and changeable for various reasons, as I was able to see during a short probing (pre-research) trip to Ejido Munerachi carried out in 2021, exactly twenty years after my last field research among the Tarahumara so far see: Marek HALBICH, "Report from a Preliminary Research Trip to North-Western Mexico", *Ibero-Americana Pragensia* 49/1, Praha 2023, pp. 103–109.

[29] Tereza STÖCKELOVÁ – Yasar ABU GHOSH (eds.), *Etnografie: improvizace v teorii a terénní praxi* [Ethnography: improvisation in theory and field practice], Praha 2013, p. 8.

Prolonged presence in one place was in several ways of considerable importance to me. I was obviously convinced that, above all, in this way I would obtain the most accurate data for the analysis of sociocultural configurations. Subconsciously, I accepted what was taking place before my eyes as an "objective" fact with which I could subsequently work. I was hardly thinking in the way that Stöckelová and Abu Ghosh so aptly state:

> […] the very question of the ethnographer's stay in the field, on which the notion of stationary research places a one-sided emphasis, is only one position of a process of inquiry, moving constantly in a diverse field of theoretical and epistemological, as well as biographical and historical powerlines […].[30]

I had not yet understood ethnography as a constant negotiation and reflexivity not only in the context of participant (or even non-participant) observation and conversation in the field, but also in the social space that often remains hidden to the discerning eye and judgment of the ethnographer. It was precisely this uncovering of hidden, or at first sight difficult to see and understand, phenomena and patterns that was to be the focus of my further pivotal research.

If I were to enter the native communities of northern Mexico today, I would of course be more instructive in many ways, since even in the Czech (and Slovak) environment we already have several anthropological or sociological studies that reflect the position of the researcher in the field, emphasizing the intersubjectivity of research, i.e. the interconnectedness and influence of the ethnographer and research partners, and pointing out that the field evolves and changes with each ethnographer's stay. For example, Milan Kováč, an ethnologist and founder of Maya studies in Slovakia who conducted several months of research among the Lacandon Indians in Chiapas, southern Mexico, argues that:

> Simply as soon as we start a stream of light so that the phenomena we intend to investigate emerge from the darkness, they are already somewhat different from what they were before. They are different because their vehicles are living people, and they respond to our presence just as we respond to them. It would be an illusion to suppose that our imaginary nature is a kind of scientific translucence, the ability to be invisible, or at least as invisible as one can be. We are pretty much physical objects, full of mysteries and surprises, that become objects of intense interest and study to our potential hosts. The subjects of our explorations usually watch us with as much, if not more, interest than we watch them […].[31]

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[30] Ibidem, p. 8.

[31] "Jednoducho ako náhle spustíme prúd svetla, aby sa javy, ktoré hodláme skúmať, vynorili z tmy, už sú trochu iné ako boli predtím. Sú iné, pretože ich nositeľmi sú živí ľudia a ti reagujú na našu prítomnosť práve tak ako my reagujeme na nich. Bolo by ilúziou domnievať sa, že našou imaginárnou prirodzenosťou je akási vedecká priesvitnosť, schopnosť byť neviditeľným alebo aspoň najneviditeľnejším ako sa len dá. Sme vcelku dost velké fyzické objekty, plné záhad a prekvapení, ktoré sa stávajú predmetom intenzívneho záujmu a štúdia našich potenciálnych hostiteľov. Subjekty našich vyskumov nás zvyčajne sledujú s rovnakym záujmom, ak nie vetším, ako my ich […]." (Translation from Slovak by the author). Milan KOVÁČ, "Antropológ ako sociálny inžinier: Prípad Lacandóncov"

In his text, Kováč primarily reflects on the dynamics of change in Lacandon communities brought about by his distinguished predecessors, especially Gertrude (Trudi) Blom, Robert Bruce, and the recently deceased German anthropologist Christian Rätsch, and points out that with each successive anthropologist, the phenomena he examines are already slightly different. I agree, but at the same time it is important to note that this is especially true in communities with smaller populations, such as those of the Lacandon, which have a population of approximately 1,000 people. Moreover, the Lacandon Indians differ from virtually all other Mexican Indian groups in that they have resisted Christianization, which has understandably attracted anthropologists to stay among them for longer periods of time. However, this probably created a paradoxical situation. While the Lacandons rejected missionaries of all kinds, they did allow people like the aforementioned Trudi Blom or, later, her protégé Robert Bruce into their world. The former, in her good faith to clothe the Lacandons in clothing made of more durable material, set in motion a process of irreversible cultural and social change consisting today, for example, of the strong involvement of many Lacandon individuals in local and regional tourism. The linguistic anthropologist Robert Bruce, despite being one of the few to learn the Lacandon language perfectly, and succeeding his mentor Trudi Blom, was apparently the first to introduce hard liquor into Lacandon households, which he drank at length with them, leading to many deaths, and eventually his own. Religious resistance notwithstanding, today's Lacandons, unlike several isolated Amazonian groups, are thus part of a global world that, especially through tourists heading to Palenque, Bonampak, Yaxchilan, and other archaeological sites in whose vicinity they live, continues to transform their lifestyle and increasingly integrate them into Mexican society.

The social engineering among the Lacandons, which is very suggestively described by Kováč, also shows a certain form of dominance with which the anthropologist claims the terrain he manages to enter first and establish friendly ties with the natives. Some anthropologists then regard such terrain as their "privileged waters" and keep any other research aspirant at a distance, as Trudi Blom did with the Lacandons for several decades. However, this approach is virtually impossible among those ethnic groups that are much more numerous or live scattered in tens or hundreds of villages, as is the case with "my" Tarahumara. Of course, many well-known anthropologists have worked among them, even in places where I have done my own research, but I have not come across a scenario similar to the one described by Milan Kováč. This is not to say that the anthropologist had no influence on the transformation of the Tarahumara lifestyle in, say, Munerachi, but the effectiveness of social engineering (and anthropologists may may be far from being the only ones responsible) is, perhaps somewhat paradoxically, weaker here than in the Lacandon communities, which were not contaminated by other religious systems or other external factors.

["The anthropologist as social engineer: the case of the Lacandons"], *Cargo: Journal for Cultural/Social Anthropology* 4/1–2, Praha 2001, p. 24.

Reflexivity, political correctness and indigenous peoples in the glocal perspective of the Czech Republic and Slovakia have recently been addressed in some texts, especially by the Czech social anthropologist Lívia Šavelková. This author, through a visual representation of the encounter of people during a lacrosse tournament in the Czech Republic, analysed the mutual construction of "exotic others", on the one hand by Indian players, on the other by Czech actors who tried to capture this process on film camera. Šavelková acknowledges her insider status as a lacrosse player and her co-authorship of this ethnographic film, which aimed to be honest and transparent in an effort both to show participation and (self-)reflexivity in the whole process, and to open up questions such as: "Who is the 'exotic other' and from whose perspective?" etc. Thus, one of the purposes of this collective visual representation was to bring in a Saidian Orientalist discourse concerning the production of knowledge by "non-Western" authors about marginalised peoples in the West. Šavelková is, however, well aware of the ambiguity or ambivalence of such exoticizing or orientalizing of the "others" and also points out that Said's notion of orientalization can be applied not only to the exoticization and stereotyping of "indigenous peoples" by members of the Euro-American space, but also, for various reasons, to the exoticization of people from "Eastern Europe" by members of the indigenous population.[32] This observation is apt, and in retrospect I am clearly aware that I have been in similar situations where I have been viewed by local people in Amerindian or later Madagascan communities as an "exotic other" having strange customs, or I have been stereotyped as someone who comes from the "other side", as is often said in Mexico about newcomers from the United States, considered without any doubt to be anyone with fair skin, light hair and blue eyes. I have gradually learned to live with this reverse exoticization and stereotyping in my field research, and in some cases it has even helped me to coexist more easily with the indigenous people.

5. From chaos to method and order

In this section, I will attempt to briefly summarize and reflect on my dissertation research among the Tarahumara in 2001 and outline my own epistemological transformation towards returnable fieldwork in the intended methodological context. This, as is evident from what has been presented so far, I have neglected, or approached with dilettantish ignorance in the past. I will focus in particular on so-called, global and glocal ethnography and the method of the extended case and multi-sited ethnography, especially in the context of environmental anthropology, and in conclusion I will reflect on the ethical issues involved in doing fieldwork in post-colonial countries and in areas sometimes referred to with some inertia as the *Fourth World* or more technically as *regiones de refugio*.[33]

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[32] Lívia ŠAVELKOVÁ, "Cross-Cultural Filmmaking as a Process of Self-Reflection: Filming Native Americans within Central European Space's Prevailing Imagery of the 'Noble Savage'", *Ethnologia Actualis* 17/1, Trnava 2017, pp. 133–154; idem, "Úvod [The Introduction]", in: Lívia Šavelková – Jana Jetmarová – Tomáš Boukal (eds.), *Původní obyvatelé a globalizace* [Indigenous peoples and globalisation], Praha 2021, pp. 46–47.

[33] The concept of the *Fourth World* is somewhat vague. It usually refers to areas where groups that have not yet been contacted live, or groups that are considered marginal even within a single country.

In 2001, I came to Tarahumara as a graduate student in ethnology and a post-graduate student in this field, in order to work on my dissertation. Although another five years had passed since my last stay, I was arriving in a relatively familiar environment and therefore without much concern about whether I would be able to live and work in ejido communities again for some time. This time I was no longer experiencing culture shock, but I do recall experiencing a sudden change in climate during my acclimatization stay in Batopilas, when sometime in early April it warmed up considerably from one day to the next, which delayed my departure for the ejido Munerachi communities. So the culture shock suddenly became more of an *environmental stress*,[34] which I could have expected, but certainly was not fully prepared for.[35]

In Latin America, this may include, for example, some Amazonian groups in Brazil, Peru, Ecuador or Bolivia, but also the more numerous Amerindian ethnic groups that are still largely marginal to the majority population in their countries. This is still the case for the majority of the Tarahumara population, although thousands of individuals now live in larger cities where they are more easily integrated into the state structure. Despite this marginality, the notion of the Fourth World is rather abandoned in anthropology and probably in other social sciences as well, as marginal groups are now often part of a wider regional, national or even global network. On the other hand, to a large extent, the characterization of these groups that George Manuel, one of the leaders of Canadian indigenous groups, and Michael Posluns, a Canadian journalist and activist, came up with almost fifty years ago is still valid: "We are the fourth world, a forgotten world, the world of aboriginal peoples locked into independent states but without an adequate voice or say in the decisions which affect our lives […]. The 'Fourth World' is thus a collective 'host' world that much of the states of the First, Second, and even Third Worlds have come to claim legally as their own […]." See Glen Sean COULTHARD, "Introduction: A Fourth World Resurgent", in: George Manuel – Michael Posluns, *The Fourth World: An Indian Reality*, Minneapolis – London 1974, p. xii. This characteristic is still valid in different areas, only some of the actors have changed. It is no longer only the state that enters the living space of these groups, but increasingly various multinational corporations seeking and then plundering natural wealth. The concept of *regiones de refugio* coined by the Mexican ethnohistorian Gonzalo Aguirre Beltrán can be seen as an analytical refinement of the notion of the fourth world: "these are mainly underdeveloped areas that have largely inherited their structure from the colonial period and in which archaic (pre-industrial) cultures found shelter from the onslaught of modern civilization"; see Gonzalo AGUIRRE BELTRÁN, *Regiones de refugio. El desarrollo de la comunidad y el proceso dominical en Mestizoamérica (Obra antropológica IX)*, Ciudad de México 1991, p. 31; in more detail HALBICH, *Lost in the Canyons*, pp. 314–352.

[34] In this case, I am at least partly referring to the thoroughly elaborated concept of environmental stress used in environmental psychology, in which "can be defined as the emotional, cognitive and behavioral responses to an environmental stimulus (or stressor) […]"; see Birgitta GATERSLEBEN – Isabelle GRIFFIN, "Environmental Stress", in: Ghozlane Fleury-Bahi – Enric Pol-Oscar Navarro (eds.), *Handbook of Environmental Psychology and Quality of Life Research*, Berlin – Heidelberg 2017, p. 469. The dramatic rise in temperature affected me so much that I preferred to continue my work in Batopilas and postpone the 20-kilometer transfer to Munerachi, which I had chosen as my primary terrain, for a few days.

[35] I had a good memory of an incident that happened during my field research in 1996. During the trek from Coyachique to Retosachi (about 20 km in very rugged terrain and with daytime temperatures well over 40ºC in the shade at the time of my stay), I got lost several times while climbing in the mountainous terrain, quickly ran out of physical strength, and of fluid supplies, and only thanks to a Tarahumara young man returning home with a few lemons and mangoes to share with me was I able to continue my journey. This incident, caused by my misjudgement of my physical strength, and during which my life was actually at stake, is deeply etched in my memory and has probably

This time, I chose as my base for several months of research the central community of the ejido of the same name, Munerachi, where key situations, interactions and encounters took place not only with its native inhabitants, but also with mestizos from Batopilas and other centres outside the ejido. Looking again at this, my most extensive fieldwork to date, from a retrospective perspective, I see this period as to some extent the first step towards the completion of my process of becoming an anthropologist. The formative period was long over, but I still felt as though I was in a kind of *liminal phase* of my professional anthropologizing in the field. In fact, I remained in that state for several years after I returned to academia at the Faculty of Humanities, where I began to lecture professionally on anthropology. Perhaps I would not be far from the truth if I now state that it was only the complicated process of writing my dissertation that established me as an anthropologist. One of the biggest complications was that several years passed between this last research and the writing of the dissertation, later adapted into book form, and my field diary proved to be rather "leaky". As I wrote, I realised that the notes I had taken during my field research were only partially usable. In an environment such as the communities in Ejido Munerachi, it is virtually impossible to conduct observations and interviews in the "classical" way, i.e., writing down in a notebook what was happening in front of my eyes, or simply arranging an interview and meeting at an agreed location with a tape recorder. To carry out ethnography in this setting is a much more complex process; it cannot be purely mechanical in the sense that the researcher follows the methodological and ethical principles outlined in the best handbooks. Implicitly, I have come to believe that the demands of the work of the field ethnographer are more complex, and necessarily require, in addition to the ability to intuit, the art of improvisation and experimentation in the sense of Burawoyan *robust ethnography* combining "classical" anthropological methods with research in archival documents, the study of minutes of indigenous council meetings, documents of various associations, interest groups, or petitions of environmental activists obtained directly from native communities, etc. With regard to my own field experience, I also include here various types of conversations on the *move*, by which I mean in particular the numerous dialogues carried out on the move while accompanying locals to, for example, urban centres, during movements between communities, during leisure activities, etc. complementing concentrated participant observation.[36] This is not to say that the field diary is not important or even meaningless, quite the contrary. The field diary, or field note-taking, certainly remains a symbol of the anthropologist's professional identity.[37] What is being modified in the context of changing and evolving anthropological paradigms is the mode and

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become an environmental psychological stressor that kicks in as soon as I am threatened with finding myself in a similar situation again. I am thus aware of the possible limits of my ethnography in such challenging geographical conditions.

[36] STÖCKELOVÁ – ABU GHOSH (eds.), *Ethnography*, pp. 22–23; Michael BURAWOY, *The Extended Case Method. Four Countries, Four Decades, Four Great Transformations, and One Theoretical Tradition*, Berkeley – Los Angeles – London 2009, p. 76.

[37] Jean E. JACKSON, "I Am a Fieldnote: Fieldnotes as a Symbol of Professional Identity", in: Roger Sanjek (ed.), *Fieldnotes. The Makings of Anthropology*, Ithaca – London 1990, p. 3.

content of note-taking. Not only should this be aimed at separating soft (e.g., statements, attitudes, opinions of our research partners) and hard (mostly statistical, "objective", countable) data, or different types of observations, but the researcher should also strive to capture as many fluid events as possible, including short episodes that at first glance do not seem relevant to the ethnographer's analysis, or whose significance becomes apparent only after these micro-events are brought together. Such an approach, however, requires a virtually permanent capacity for improvisation and experimentation, and an awareness that new and novel situations are created in the interaction between the ethnographer and the research partners – as I would probably best name all the actors who are part of our research network today, who constantly transform the articulation of the rules and expectations with which we enter the field. However, this fluid situatedness need not be a hindrance in research, but on the contrary, it can be an opportunity to continuously fine-tune research questions and thus ultimately improve the quality of the researcher's conclusions.[38]

6. Walking out of the stationary field research

By briefly indicating the gradual transformation from adherence to a more classical approach to field research to a more robust anthropological research, I do not want categorically to claim that stationary research (single-site oriented) is completely outdated or irrelevant. Indeed, the complete abandonment of this type of field research among the Tarahumara would probably be a methodological error, as the greater part of the total number of about 100,000 Tarahumara still remain in their native communities, particularly in the Lower Tarahumara (Tarahumara Baja) in the districts of Batopilas, Urique and Guachochi.[39] At the same time, however, it is quite evident that more and more of these Indians are for various reasons moving outside their native habitat to large cities, where more and more of them are settling permanently. Another and probably more important reason for the change in research scale is the increasing involvement of the Tarahumara in regional, national and even global social, political, economic or environmental networks. Thus, internal and external migration and the stronger penetration of globalisation effects into the heart of Tarahumara territory are, in my view, the two most important impulses for methodological and epistemological change.

This transformation has led to the necessity in many anthropological departments to overcome the long-standing idea of an ethnographic trilogy consisting of *one researcher, one (limited) time and one place*,[40] exactly along the lines of my past research. *Global transformations* have rapidly directed anthropological research towards a *methodological turn* in which this classical trilogy, going back in its purest form at least to Malinowski and his long-standing research on Trobriand society, has ceased to be sufficient for the effectiveness of such research. Today's field research is therefore much more a matter of a research team, often international and

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[38] STÖCKELOVÁ – ABU-GHOSH (eds.), *Ethnography*, p. 24.
[39] HALBICH, *Lost in the Canyons*, p. 196.
[40] Michel-Rolph TROUILLOT, *Global Transformations. Anthropology and the Modern World*, New York 2003, p. 104.

interdisciplinary, consisting of perhaps five or six or more researchers from different social and natural science disciplines. Although teamwork is quite common in the natural sciences and much more widespread than in the social sciences and humanities, this current apparent trend is not entirely new even in social anthropological research. In the second half of the 1940s, Julian Haynes Steward led a research team of budding social anthropologists. Such later renowned anthropologists as Eric Wolf and Sidney Mintz were among the group of these young scholars sent by Steward to various regions of Puerto Rico. Even before this team research, which was essentially the birth of community studies, at least a few researchers had worked in the same area, such as Wendell Clark Bennett and Robert Mowry Zingg in the Samachique region on the border of the Upper and Lower Tarahumara in the early 1930s, the former a specialist in material culture and archaeology, the latter in spiritual culture and social organization. Their nine-month research resulted in the most complete ethnographic monograph of the Tarahumara to date. Although it was mostly descriptive, it is still extremely valuable for contemporary anthropologists conducting their research in the region nearly a hundred years after its first publication (1935).[41]

While Bennett's and Zingg's monographs were more or less based on classical stationary research, and their conclusions could not be generalized and applied to other Tarahumara regions, the collective monograph published under Steward's direction already went beyond this background. The fact that the ambitions of this research to some extent went beyond one community, one region, or in the case of Puerto Rico, the entire island, was clearly indicated by Steward at the beginning of the extensive Introduction:

> The substantive results of the study are seen as exemplifications of processes which are now occurring also in other world areas, and this volume concludes with some hypothetical regularities of change which appear to operate in different cultures elsewhere.[42]

This Puerto Rican anthropological monograph is, it seems to me, somewhat forgotten and, in my opinion, perhaps somewhat unduly underappreciated. Indeed, Steward, and by implication his younger colleagues, probably anticipated what, many decades later, came to be called *globalisation* in the sociological and political science lexicon, and what anthropology moved towards in its research. Thus, in the 1990s, new research methods emerged in response to the change in scale. The roots of global and glocal ethnography, the extended case method, multi-sited ethnography, or transnational approaches to the study of migration processes, etc., thus reach back at least to this collective Puerto Rican ethnography methodologically linking and containing what Michael Burawoy characterized half a century later as the *extended case method*:

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[41] Wendell Clark BENNETT – Robert Mowry ZINGG, *The Tarahumara: An Indian Tribe of Northern Mexico*, Chicago 1935; HALBICH, *Lost in the Canyons*, pp. 67–73.

[42] Julian H. STEWARD, *The People of Puerto Rico*, Champaign (Illinois) 1956, p. 1.

The extended case method applies reflexive science to ethnography in order to extract the general from the unique, to move from the "micro" to the "macro" to connect the present to the past in anticipation of the future, all by building on preexisting theory [...].[43]

Although Gluckman et al. still introduced *situational analysis* into their research, these anthropologists did not go beyond the shadow of what was then the majority approach based on the aforementioned Trouillot trilogy. One of those in social anthropology who responded to the rapid transformation of the world by radically shifting their research from seeing the terrain as a *micro-environment* in the sense of *small is beautiful* (e.g., a village, a football stadium, an urban neighborhood, a luxury hotel, a small nomadic tribe, etc.) to a global or *"extended"* field as a *macro-environment* linking at least two but usually more separate spatial and material entities, it was Michael Burawoy who, using ethnographic participant observation, studied everyday life in its extra-local and historical context. In doing so, he started from a reflexive model of science based on the assumption of the *intersubjectivity* of the scientist and the object of study. Burawoy, however, did not arrive at this transformation and subject-object shift until forty years later, as his original fieldwork in the Copperbelt of Zambia was not yet very intersubjective and reflexive. But he was fortunate to be able to *restudy* his original conclusions and, on a methodological level, to shift his research back towards a global ethnography that places local settings and their actors on the global stage as pointed out by Gille and Ó Riain, who, together with Burawoy, helped to formulate the concept:

By locating themselves firmly within the time and space of social actors living the global, ethnographers can reveal the socioscapes that people collectively construct of global processes, thus demonstrating how globalization is grounded in the local.[44]

This new challenge for the anthropologist reflecting the *global situation*,[45] however, also presents a complication in that the uneven separation of the spatial and

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[43] Michael BURAWOY, "The Extended Case Method", *Sociological Theory* 16/1, Thousand Oaks, California 1998, p. 5; BURAWOY, *The Extended Case*, p. 21. Their aim was to develop a method for practical ethnography that would lead, according to Max Gluckman, to the study of colonial and colonized society as one social system with two racial groups nevertheless forming the basis of its structural unity see Max GLUCKMAN, *Custom and Conflict in Africa*, Glencoe 1955.

[44] Zsuzsa GILLE – Seán Ó RIAIN, "Global Ethnography", *Annual Review of Sociology* 28, 2002, p. 271; Michael BURAWOY – Joseph A. BLUM – Sheba GEORGE et al., *Global Ethnography: Forces, Connections, and Imaginations in a Postmodern World*, Berkeley – Los Angeles – London 2000; Martin ALBROW, "Travelling beyond local cultures. Socioscapes in a global city", in: John Eade (ed.), *Living the global city. Globalization as a local process*, London – New York 1997, pp. 35–52.

[45] As far as I know, virtually no anthropologist thinks that the global future will lead to cultural homogeneity. Rather, the opposite is the case: much ethnographic research on the impact of globalization on the local environment concludes that this impact tends more towards an increasingly "local" cultural diversity see Anna TSING, "The Global Situation", *Cultural Anthropology* 15/3, Arlington 2000, p. 339. However, the reality of the resistance of native societies to globalization has been pointed out

the social undermines or potentially severely weakens the claim that ethnographic research has always made, namely, to understand the social relations studied *in situ* and *in vivo*.

I see global ethnography more as a macro-sociological method in which the main emphasis is on *global (extra-local) actorhood*, on wider networks of relations based on a more general concept of globalization,[46] while local actors are to some extent neglected. Thus, *global ethnography* places more emphasis on the study and analysis of *ex situ* phenomena, focusing more on societies, institutions, corporations, NGOs, etc., rather than where these entities are headquartered.[47] As a kind of reaction to the somewhat "stretched" method of global ethnography, the social anthropologist Noël Salazar introduced in 2006 the methodological concept of *glocal ethnography*, drawing on several theoretical sources. The term *glocal* itself is a borrowing from the sociological concept of *glocalization* first introduced by Roland Robertson in his research on Japanese society. The term was therefore first applied to the Japanese word *dochakuka*, which originally described a situation in which farming techniques were adapted to local conditions. The idea of applying the term to "global localisation" was adopted especially in the business world.[48]

This shift of attention from the global to the local was initially much studied, especially in the context of travel and tourism. James Clifford speaks of a *rerouting* of field research from relatively bounded terrains to spatially unbounded places:

> Generally speaking, the localization of "natives" meant that intensive interactive research was done in spatially delimited fields and not, for example, in hotels or capital cities, on ships, in mission schools or universities, in kitchens and factories, in

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before by Marshall Sahlins in particular with his concept of the *indigenization of modernity* see Marshall SAHLINS, "Goodby to Tristes Tropes: Ethnography in the Context of Modern World History", *The Journal of Modern History* 65/1, Chicago 1993, pp. 3–5.

[46] There is no room in this text for a detailed interpretation of globalisation in an anthropological context. The very concept of globalisation, global flows and their impact on specific local environments is dealt with in more detail in another article see Marek HALBICH, "Local Reactions of Village Communities of the East Coast of Madagascar to Globalisation", in: Lívia Šavelková – Jana Jetmarová – Tomáš Boukal (eds.), *Indigenous Peoples and Globalisation*, Prague 2021, pp. 283–314.

[47] An example of a somewhat opposite approach is one of the most cited books in the field of transnationalism and migration studies today, which focuses on people at their place of origin in sub-Saharan Africa and their complex migration trajectory to their final destinations in the European Union. Research by global institutions, in this case for example Frontex (the European Border and Coast Guard Agency) based in Warsaw, is also analytically rigorous, but the primary focus is on "following people", their actions, their reactions to various bureaucratic and other obstacles in situ and *in vivo*. From a methodological point of view, such research can be considered a combination of global ethnography, the extended case method, glocal ethnography and multi-sited ethnography see Ruben ANDERSON, *Illegality, Inc. Clandestine Migration and the Business of Bordering Europe*, Oakland, California 2014.

[48] Roland ROBERTSON, *Globalization: Social Theory and Global Culture*, London – Thousand Oaks – New Delhi 1992, pp. 173–174; Habibul KHONDKER, "Glocalization as Globalization: Evolution of a Sociological Concept", *Bangladesh e-Journal of Sociology* 1/2, Dhaka 2004, pp. 3–4.

refugee camps, in diasporic neighbourhoods, on pilgrimage buses, or at a variety of cross-cultural sites of encounter […].[49]

Salazar later applied this rather theoretical (meta-anthropological) assumption by Clifford in the context of glocal ethnography to his research on tourist sites in Indonesia and elsewhere on the planet, and set out its rather precise methodological framework, which included a clear statement of objectives, key research questions, data collection and analysis, and most importantly, an outline of the final product, which was to be an understanding of the complex interconnections, disconnections and reconnections between local and global phenomena and processes.[50]

On a more theoretical level, this spatial methodological reorientation of field research was elaborated by George Marcus in the so-called *multi-sited ethnography*, which consists not only of an attempt to step out of one limited research site, but above all of a thorough observation of people, things, metaphors, stories, conflicts, etc. Probably the most common multi-sited observation is the tracking of human actors and things (objects) not only in their home sites, but especially in their "excursions" beyond those localities.[51] Malinowski's tracking of people and sacred objects on kula ring cruises consisting of the ritual exchange of gifts, however, was still effectively taking place within one relatively circumscribed area of the Trobriand archipelago. Yet we might see hints of the later extended case method in his approach, both in the original Gluckmanian and in the later Burawoyian sense. We might see parallels between Gluckman and Malinowski, for example, in the attempt to arrive at the general through careful observation of the dynamically changing peculiarities of the case. Gluckman, for example, sought to understand and explain the colonial system as a single system by studying social conflict in South African Zululand,[52] while Malinowski, by studying the meaning of the *kula* ritual, explained the social

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[49] James CLIFFORD, "Spatial Practices: Fieldwork, Travel, and Disciplining of Anthropology", in: Akhil Gupta – James Ferguson (eds.), *Anthropological Locations. Boundaries and Grounds of a Field Science*, Berkeley – Los Angeles – London 1997, p. 207.

[50] Noël SALAZAR, "Experimenting with 'glocal ethnography' as a methodology to study tourism in Asia and beyond", (conference paper), *Asia Research Graduate Workshop, Questions of Methodology; Researching Tourism in Asia*, (International conference organized by Asia Research Institute, National University of Singapore & Department of Tourism, University of Otago, New Zealand on 5–6 September 2006). Thus, for example, a typical research question emerging from glocal ethnography may be an attempt to understand how people's thinking and behavior are shaped by local and global influences.

[51] The ethnographer's participation in extralocal mobilities is not new in social anthropology. An archetypal example of a similar technique, according to Marcus, is the book *Argonauts of Western Pacific* of Bronislaw Malinowski, who participated in the circumnavigational trade routes and ritual cycles of specific groups of people in the Trobriand Islands see George MARCUS, "Ethnography in/of the World System: The Emergence of Multi-Sited Ethnography", *Annual Review of Anthropology* 24, San Mateo (California) 1995, p. 106.

[52] Max GLUCKMAN, "Analysis of a Social Situation in Modern Zululand", *Bantu Studies* 14/1, Thames, Oxfordshire United Kingdom 1940, pp. 147–174; Richard WERBNER, *Anthropology after Gluckman: The Manchester School, Colonial and Postcolonial Transformations*, Manchester 2020, p. 1; Terrence M. S. EVENS – Don HANDELMAN (eds.), *The Manchester School: Practice and Ethnographic Praxis in Anthropology*, New York – Oxford 2006.

cohesion of the inhabitants of the scattered islands. Some parallels between Burawoy and Malinowski can be seen, for example, in the geographical spread of the case from one focal point or centre to a larger area. Although Burawoy's elaboration of the extended case method is much more complex, one of his main premises is the observation of everyday life not only in a local but also increasingly in an extra-local context.[53] Malinowski's research, however, was still conducted in a reasonably clearly bounded space, so I would rather describe his approach as a regionally constrained extended case method, which took him from a central location (such as the islands of Toulon or Kiriwina) to a wider archipelagic space, but virtually not to other places in the Pacific area. His approach thus more closely resembled a limited, extended case method that took him from a central location (such as the islands of Toulon or Kiriwina) to a wider archipelagic space, but virtually not to other parts of the Pacific area.[54]

Nevertheless, this "classical" research and a number of later anthropological investigations foreshadowed a future methodological turn along the lines I have outlined above. Perhaps the most important work in this regard was Wolf's book on *nations without history* and their European conquerors,[55] often compared to the work of major world systems theorists, notably Immanuel Wallerstein or Andre Gunder Frank. Wolf's book, however, differs qualitatively from the approach of these two great theorists in that, unlike them, his focus is on research into the interrelationship of the global environment and local worlds, which represent a sometimes latent, sometimes evident symbiosis, and is thus irreducibly "glocal". These "glocal" worlds are sometimes dominated by transnational economic processes, but flexibly adapt by either maintaining, adopting or transforming cultural traits. In principle, I agree with some contemporary social anthropologists who regard Wolf's book as an unrivalled study of *comparative global anthropology*. Wolf has analyzed not only the dynamics of culture and identity, but also the relations between power and economic processes, pointing out how anthropologists can learn from world history and, conversely, why historians should be interested in the methods and perspectives of anthropological research. Long before it became commonplace, Wolf thus pointed to the necessity of *multi-sited* anthropological fieldwork and argued persuasively that global processes must be studied in local contexts.[56] In contrast to world systems theory, which does not concern itself much with peripheries, Wolf focused specifically on the inhabitants of marginal areas on a global scale and how these peoples were active participants in shaping the new cultural and social forms

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[53] Michael BURAWOY, *The Extended*, p. 90.

[54] As far as I know, the kula exchange system, which has been the main subject of research in the *Argonauts*, has been recorded, apart from the Trobriand Islands, in Port Moresby in Papua New Guinea, for example, but has not extended significantly into that part of Melanesia see Bronislaw MALINOWSKI, *Argonauts of the Western Pacific*, London 1922.

[55] Eric WOLF, *Europe and the People without History*, Berkeley – Los Angeles – London 1982.

[56] Thomas Hylland ERIKSEN, "Forward to the 2010 Edition", in: Eric Wolf, *Europe and the People without History*, Berkeley – Los Angeles – London 1982, pp. ix–xviii.

that emerged in the context of trading empires.[57] Wolf advocates a type of history written on a global scale that acknowledges the major structural transformations of world history tracing the connections between specific communities, regions, ethnic groups, and peoples that anthropologists often separate and examine and reify as separate entities.[58]

7. *Being there* after twenty years: a new beginning, new challenges

Last year (2021), I began the process of returning to Tarahumara communities to conduct new, larger-scale anthropological research focused primarily on *glocalizing local knowledge of biodiversity conservation* in broader environmental, social, political, and other contexts. Such a project necessarily requires a change of approach reflecting the transcendence of the "sacred ethnographic trilogy" suggested by Trouillot. I went on a lightning pre-research sounding trip in late September/ early October last year with the freasonably clear aim of preparing the best possible conditions for overcoming this classic research triad. Although officially I will probably be the only researcher in charge of the new project, I will overcome the horizon of one researcher by having as co-authors not only local and other experts in, for example, biodiversity and environmental protection in general,[59] but also some individuals from the native population[60] who, thanks to their erudition, knowledge and partnership during the research, have a great deal to contribute to its final form. Among the Tarahumara, North American evolutionary biologist Daniel Lieberman has recently conducted interdisciplinary research with significant native participation, seeking to explain as fully as possible the often superhuman endurance

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[57] William ROSEBERRY, "European History and the Construction of Anthropological Subjects", in idem, *Anthropologies and Histories. Essays in Culture, History, and Political Economy*, New Brunswick – London 1989, p. 130.

[58] Ibidem, p. 125.

[59] Here I am thinking, for example, of the Mexican biologist and environmentalist Manuel Chávez Díaz, who led the development project *Tarahumara Sustentable* (*Sustainable Tarahumara*) from 2014–2019, which inspired my new research in the Sierra Tarahumara. Chávez Díaz now works for the state organization CONAFOR (*Comisión Nacional Forestal*) with the main objective of mapping and protecting the biodiversity of Chihuahua's diverse ecosystems. I can have the archival documentation and results of the already completed Tarahumara Sustentable project, which will later allow me to study the history of processes and conflicts related to local and regional biodiversity conservation.

[60] This development is also not entirely new in anthropological research and goes back at least to Maurice Leenhardt and his key informant, the grand chef (chiefman) of the New Caledonian tribe Houailou Mindia Néja, or to Franz Boas and his lifelong native (Kwakiutl) collaborator George Hunt see James CLIFFORD, "Fieldwork, Reciprocity, and the Making of Ethnographic Texts: The Example of Maurice Leenhardt", *Man* 15/3, London 1980, p. 525. Although neither acknowledged co-authorship of the ethnographic texts produced in their titles, without the ability of the two natives to translate the text from their native languages into French and English respectively, the final product would have been far more inaccurate. Probably the first anthropologist to explicitly call for a *reversed anthropology* based on reciprocal ethnography, during which both the ethnographer and the native observe and interpret, was Roy Wagner, who saw field research as, among other things, a cumulative or parallel culture shock expressing the changing feelings and reciprocal gradual adaptation of the researcher to the local people and vice versa see Roy WAGNER, *The Invention of Culture*, Chicago – London 1981, pp. 22–33.

capabilities of some Tarahumara individuals. Lieberman has powerfully involved in his research one of the most famous contemporary Tarahumara runners, Silvino Cubesare Quimare, from the Huisuchi mountain community. The result of their collaboration was, among other things, a particular deconstruction of the stereotypical view of Tarahumara runners as "mythical Stone Age super-athletes" suggested and globally disseminated by the famous book *Born to Run*.[61]

Research focused on local responses to climate and environmental change also necessarily requires a change in the temporal orientation of such research. A kind of immobile long durée (however long the stay in the field), is replaced by an exploration of the *processuality* of such problems. Through robustly focused research, the researcher must go back in time and try to identify and explain the causes of the current state of the environment and, in interdisciplinary collaboration and interaction with indigenous governments, at least outline ways to protect biodiversity not only from external influences but also from progressivists among local people.[62] Finally, the third and perhaps most important shift is the approach to research location. As I indicated in the previous section the new research project in the Sierra Tarahumara will be a combination of multiple methods or research techniques and strategies, alternating a global ethnographic approach with glocal ethnography and multi-sited observation tracking people's movements over a larger area, the circulation of things, tracking local, regional and larger conflicts,[63] events etc. with a stay at the main research base, depending on the current situation.[64]

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[61] See Christopher MCDOUGALL, *Born to Run: A Hidden Tribe, Superathletes, and the Greatest Race the World Has Never Seen*, New York 2009. In this book, for example, he incorrectly explains the running abilities of the Tarahumara by saying, among other things, that heart disease, high blood pressure, diabetes, carbon dioxide emissions, etc. did not exist among them see Daniel E. LIEBERMAN – Mickey MAHAFFEY – Silvino CUBESARE QUIMARE et al., "Running in Tarahumara (Rarámuri) Culture. Persistence Hunting, Footracing, Dancing, and the Fallacy of the Athletic Savage", *Current Anthropology* 8/3, Chicago 2020, p. 358. However, this worldwide bestselling book has caused many people, including those interested in endurance running, to have very skewed information about the Tarahumara.

[62] In this case, I mean by progressivists, for example, those of the local people who agree to clear forests faster for logging companies in order to generate immediate income for such a community.

[63] In this text, I do not explain in detail what I mean by local or global, the relationship between local vs. global, etc., although I am aware that both terms are often treated in a very light and vague way, and especially that they do not include the notions of regional and national as important spatial intermediaries that link the local and global environments, at least to some extent. In this context, as early as the 1980s, some social geographers pioneered the thesis that environmental relations at the local level cannot be understood without research and clarification of relations at the regional level úrovni see Yancey ORR – J. Stephen LANSING – Michael R. DOVE, "Environmental Anthropology: Systemic Perspectives", *Annual Review of Anthropology* 44, San Mateo (California) 2015, p. 160.

[64] An example of a global ethnographic research can be the analysis of the documents of the global organization *Conservation International* and their impact and resonance in indigenous communities. An example of glocal ethnography might be the analysis of a document (or verbal agreement) issued by local authorities towards extra-local actors. In September 2021, I witnessed an indigenous local government meeting in the community of Retosachi (Ejido Munerachi) regarding the protection of pine forests and how to protect them from the increasing pressures of timber corporations, but also from climate change or weather fluctuations. In many ways, pine is an integral part (kin unit) of Tarahumara families in many places in the Sierra Tarahumara see Alejandro FUJIGAKI LARES,

I thus see glocal ethnography and similar methodological innovations as a new research challenge and ask whether detailed research conducted in specific places and combining single- and multi-sited observations can shed light on wider (global) networks. Is there indeed a complex process leading to the interconnectedness or even convergence of local and global environmental space, or is it merely a manifestation of cultural imperialism in which certain cultural forms and social institutions spread unidirectionally? One of the questions I would like to pose as a provisional answer is, how have people living in a recently colonized world adapted to new social conditions? In other words, how did they develop the mechanisms of resistance and adaptation by which natives control their environment, which are said to be (perhaps necessarily) glocal?[65] If I am referring specifically to the new ways of doing ethnography among the Tarahumara and in spaces associated with them, then I will try to answer questions such as the following: can a better understanding of local biodiversity, the knowledge of which indigenous peoples have accumulated over many millennia, help build a sustainable and appropriate future? Can the Tarahumara relationship to the environment lead to some more general ontological or existential turn that acknowledges or even questions the relatedness of human and non-human actors and contributes its part to the global discourse on saving the planet? These and many other questions, however, will be more precisely and purposefully generated only by my return field research to the Sierra Tarahumara and other places reflecting some of the methodological inspirations I have outlined.

(Written in English by the author)

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[65] ERIKSEN, *Social and Cultural Anthropology*, p. 362.

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