Ships passing in the night? Interdisciplinarity, East-West relations and commoditization of knowledge in anthropology of post-socialism

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Abstract: Debates on 'native anthropology', 'anthropology with an accent' and so forth have usually focused on colonialism as the main culprit of asymmetric relations between anthropological knowledges. By bringing the recent dispute between Western and 'native' anthropologist of post-socialism into the 'world anthropologies' debate, I seek to highlight those aspects of current epistemic inequalities that are not post-colonial in nature, but result from global commoditization of knowledge. I ponder why Western anthropologists who started visiting Eastern Europe from the 1970s, concluded that 'native' academic knowledge is inferior to their own output. This was not due to a prejudice brought from afar, I argue, but rather was a result of their field experiences. I discuss how three types of native 'captive minds' (communist, nationalist, and neo-liberal) emerged, and how encountering (or learning about) them made Western anthropologist uninterested in (and distrustful of) local epistemic production. I focus on the putative nationalist 'captive mind', and argue that the straw man of East European 'positivist' science (as opposed to the superior 'theory-oriented' Western anthropology) emerged due to recent changes in the political economy of the academia. I show how the 'theoretical turn' was experienced differently in Western and Polish academia, and how these changes, explained by the different regimes of value, show that there has been an increase only in 'ritual' exchange between parochial and metropolitan anthropology rather than meaningful communication.

Keywords: commoditization of knowledge, Eastern Europe, history of science, nation-state, positivism, post-colonialism, post-socialism, world anthropologies, world-systems analysis.

As the good old anthropological tradition disallows making general statements without invoking to the particular, I would like to take up Eduardo Restrepo and Arturo Escobar's invitation to carry out an 'exercise of double critique (critique of power over and power within)' (2005: 101) on world anthropologies by discussing the hierarchies of anthropological knowledge in a region unmapped in this debate so far, i.e. East and Central Europe. This is especially relevant as a similar discussion has recently emerged between Western and 'native' scholars of what is called 'post-socialism' (Benovska 2007, Bošković 2007, Buchowski 2004, 2005, 2007, Collier at al. 2003, Durand 1995, Hann 2005, 2007a, 2007b, Kalb 2007, Kelertas 2006, Moore 2001, Otiou 2003, Podoba 2007, Scheffel 2007, Skalník 2007, Stenning 2005, Stewart 2007, Uherek 2007, Verdery 2002, 2004, 2007). It seems that confronting this debate with what had been said about 'world anthropologies' from mainly the vantage point of Latin American anthropology may help us separate the wheat from the chaff in both discussions and push them, hopefully together, forward.

Unlike most previous voices in this ongoing controversy, published recently in *Critique of Anthropology* and written earlier on the subject of 'native anthropology', 'anthropology with an accent' and so forth, anthropology of post-socialism has no colonial heritage to struggle with. Although analogies to post-colonialism have been made (cf. Buchowski 2004, Verdery 2002, Hann, Humphrey and Verdery 2002), and one may argue that the region had been subject to German and/or Russian colonialism in the XIX and XX centuries, the Anglo-American anthropology of (post)socialism started in the 1970s. This unusual condition of anthropology of post-socialism may help highlight such aspects of current hierarchies of anthropological knowledge that are result of the way anthropology is structured and practiced today, rather than are due to what had occurred a century ago. Tracing synchronic links between today's anthropology and the global political economy rather than focusing on the diachronic links with colonialism (and hence eschewing the notorious 'natives should talk back' approach) was, I believe, the direction that Restrepo and Escobar were advocating for in their opening article. Most responses to their text, however, hinged upon arguments confined to the domination-resistance metaphor and aimed primarily at 'decolonising the field' (Degregori 2006: 469). The various authors only mentioned in passing issues such as 'really existing globalisation' (Ribeiro 2006: 370), the worldwide 'intellectual/academic market' (Ribeiro, p. 365), and the 'free trade agreement' prevailing in academia today (Degregori 2006: 465). If post-colonial anthropology was a brighter future to strive for thirty or so years ago, today 'international circulation of ideas' (Riberio 2006: 372) exercised by a 'global community of anthropologists' (Ribeiro 2006: 380) is a fact hard to deny. Like some decades ago, today's anthropology is plagued by inequalities, but they are not result of the (post)colonial institutions but rather are the workings of the global market for ideas and commoditisation of knowledge. I agree with Pina-Cabral that 'we need the mechanisms of present-day imperialist academic silencing to be exposed' (2006: 468) and with Ribeiro that 'heteroglossia in anthropological production should start with the recognition of an enormous production in different world locales' (2006: 371). The world anthropologies debate, however, has been so far surprisingly general, and did not scrutinise the 'watery realm of interconnectedness' (Pina-Cabral 2006: 469) between current metropolitan and parochial academic bodies of knowledge. By putting the (post)socialist case on board, I will show how 'positivism' was the central notion (surprisingly absent in the debate!) that

structured the recent 'terms and conditions of anthropological conversability worldwide' (Restepo and Escobar 2006: 486). We are, as Restepo and Escobar stressed, beyond the liberal and Marxist moments, and indeed 'world anthropologies' are part and parcel of the post-structuralist movement. Post-structuralism (post-modernism, post-colonialism etc.), however, has had many faces, and only some of its versions, in Gavin Smith's phase, 'gained purchase' (2006: 472) on the global market for academic goods. This global market has 'dialogue' (a nicer word for market exchange) as its basis, and therefore we cannot speak really of 'asymmetric ignorance' any longer but rather of 'reciprocal disinterest' between various actors operating on the global knowledge market, or rather on its many and only partially overlapping niches. What I have in mind is not the process of 'Othering', but rather the practice of manufacturing straw men that are utilised in waging academic battles and in, as Clifford Geertz once put it, 'mutual exchange of intellectual insults' (1973: 10).

Manufacturing straw men

Let us first trace how such 'reciprocal disinterest' was achieved by the metropolitan scholars of (post)socialism. Unlike Malinowski and the colonial lot, Western anthropologists who came to Central and Eastern Europe in the 1970s and later had a vast body of written knowledge, some of which could pass for an equivalent of 'their anthropology', to confront. They generally, however, to this very day bracket it. This was the concern of Michał Buchowski (2004, 2005), who pondered how was it possible for anthropologists fully proficient in 'native anthropology' debates and well attuned for 'subaltern voices' to engender what he dubbed a neo-positivist and neo-Orientalist science and assume that objects of their analysis themselves have nothing interesting to say. It was, not due to a prejudice brought from afar, but was rather an outcome of encounters in the field. Individual efforts to explore the local epistemic production – that have certainly been made – were made in vain, because even those who had spend months in local libraries – as one 'culprit' has recently told me – had a difficult time incorporating outcomes of such research into their anthropological work. Somehow Malinowskian fieldwork turned out to be the best (and the most reliable) research method of socialism and of 'what came after'. In other words, various local bodies of knowledge have become discredited, distrusted, deemed irrelevant or incommensurable in a contingent process. It had three critical moments.

First and foremost, Western anthropologists coming to the region before 1989 learned from the Cold War propaganda as well as from the natives they meet, that the official (communist) sources were not worth a brass farthing, and hardly bothered to explore it. Instead, they turned to the voices of 'opposition' - as anthropologists are inclined to do. '[T]he orientation I am advocating' Katherine Verdery wrote, 'would give voice to the 'natives' as analysts of their own condition. Although it is not yet clear who would be the Franz Fanon of this corpus, his or her forerunners surely include the CEE dissidents and other scholars – people like Rudolph Bahro, Pavel Câcpeanu, Györgi Konrád, István Rév, Jadwiga Staniszkis and Iván Szelényi – whose writing spurred us to seek an understanding of socialism different from that offered by Cold War categories' (in Buchowski 2004: 6). These figures (sociologist and political economists exclusively) have published in English in the 1980s and the 1990s, and are indeed read and discussed with. They are not, however, full-fledged East

European Franz Fanons, because they turned out to be politically and/or epistemologically entangled in the building of the post-socialist order, and hence imbued with another ideology: this time neo-liberal or neo-conservative. Finally, such 'Fanons' were not even sought among local anthropologist, because these were discredited as *Volkskundists* (ethnographers or ethnologists) – working within an outdated paradigm (positivism), and/or serving the nationalist project. As John Davies put it already in 1977: 'a contemporary ethnographer from France and England or America, carrying the very latest lightweight intellectual machine gun in his pack, may be suddenly confronted by a Taylorian or Frazerian professor appearing like a Japanese corporal from the jungle to wage a battle only he knows is still on' (in Buchowski 2004: 10). So, in short, confronted with an object of analysis that seemed unique (socialism) or wholly new (the allegedly clean slate of post-socialism) and disillusioned with the existing bodies of knowledge, Western anthropologists were left to themselves, as lonely Malinowskian riders surrounded in the field by 'captive minds' of three sorts: communist, neo-liberal/neo-conservative and positivist/nationalist.

I use the term 'the captive mind' on purpose – it is an *emic* notion, popularised by Czesław Miłosz's bestseller (1953). Anthropological neo-positivism was, therefore, achieved partially by assuming the 'native point of view'. Natives were perhaps even more distrustful first towards the communist 'propaganda', and second towards the eulogists of neo-liberalism (who were under an avalanche of criticism from the grassroots in the 1990s) than the foreign anthropologists themselves. The motives behind bracketing of the local anthropological production are more complex and more relevant to our discussion. A standard Western opinion about how anthropology was practiced behind the Iron Curtain reads as follows: 'scholars in CEE countries tended to share a traditional, nationalist preoccupation with peasant traditions, and their work had little theoretical content or comparative range' (Adam Kuper in Buchowski 2004: 10). On one hand this stems from a classic Cold-War myth holding that state socialism is merely a smokescreen that mystifies lurking nationalisms, hence the only alternative to communist propaganda is epistemic nationalism. On the other, this is a standard critique of 'positivism', this time displaced onto a geographical 'Other' (Buchowski 2004, 2006) and 'incarcerated' (Appadurai 1988) in particular locales.

Kuper's point rests upon the distinction between anthropologies of nation-building and anthropologies of empire-building (Ribeiro, p. 375), but it goes a step further, since he argues that the former is inferior because it has no theoretical value. This lack of theoretical insights is equalled here with lack of reflexivity – East European anthropologists are seen as happily emerged in the everlasting now of naïve positivist anthropology, and waiting for their turn to undergo the critical revolution that came in the West after the 1960s. One may wonder how Kuper arrived at this conclusion without knowing any of the local vernaculars (Buchowski 2005: 10), but the issue is more serious, since many scholars who do speak local languages share such views. For example Chris Hann, in a bitter reply to Buchowski's 'open letter', admitted that he simply does not find local anthropology useful, and when for example two Polish anthropology students visited him at his research site in a Carpatian village in the 1980s, they 'could not really understand my concern to document the prevailing economic and political structures, just as I could muster little intellectual interest to photograph and catalogue the roadside religious monuments of the region' (Hann

2005: 195). He goes on, instructing Buchowski and other locals that if they 'wish to be as widely read as some of the outsiders who write about CEE [Central Eastern Europe – K.P.], then they need to put in the field time and write monographs of equivalent depth and sophistication' (Hann, 2005: 195). That is, they should do 'proper' fieldwork, and proper fieldwork is, according to Hann, staying in a foreign country for twelve months. In fact writing about their home countries ought not to be 'the only career option' for CEE scholars, and they would actually be better off if they do not 'try harder to write books about CEE that compete with the products of foreign scholars for publication by Cornell, Cambridge, or some other prestigious player in the market that is dominated by the Anglo-Americans' (Hann, 2005: 196). Instead of 'doing fieldwork at home and confining [their] foreign trips to academic institutions', he continues, they should 'take advantage of post-socialist freedoms and embark on anthropological projects outside their home countries'. They have failed to do so, and therefore 'the CEE etnographers/anthropologists are still confining themselves to their national frames', just like their academic forefathers did. Nearly half a century of state socialism changed that not a jot, and even fossilised the local academic world, since the communist power holders had a bizarre penchant for folk culture (Hann 2007: 25). Only the experience of doing fieldwork abroad, Hann insists, would free CEE scholars from their nationalist mindset and place them 'in a better position to undertake similar in-depth work in [their] own country, and thus to compete more effectively in that sector of the market' – if they so much insist on doing so. Hann also provides a positive hero story of an CEE scholar who, thanks to his research in a foreign (albeit post-socialist) country 'have contributed the concept of *sovkhoism* to the anthropological tool-kit' and whose work is much more valuable than the local 'intellectual pyrotechnics' (Hann, 2005: 196). Since CEE anthropologists' work still boils down to 'document[ing] the vanishing folk culture by making two-week excursions to the countryside, just as [their] predecessors have done since the nineteenth century' (Hann, 2005: 196), they also lack the critical distance towards their own intellectual endeavours. In other words, they are ignorant of the many post-structuralist debates that became cathartic for the colonial anthropology and that exposed the power-laden process of doing fieldwork and social science more generally and that are indispensable for doing enlightened anthropology today. For Hann, CEE anthropologists are still naively Orientalist towards their subjects, and this is why they romanticise and aestheticise the peasantry just as the first *Volkskundists* did over a century ago and think that taking pictures of the rustic lifestyle is still what anthropology is all about. Such positivist/Orientalist anthropologists cannot produce innovative theoretical output, 'theoretical' being the very word that replaced 'critical' and 'objective' as the main criterion for separating good from bad science. Theory (or ' useful concepts') in such an approach is a 'thing' extracted from the raw data, put preferably in the plainest way possible (so it is not confused with 'some*thing*' else) and marketed. It is only the post-colonial self-aware epistemic tool-kit that guarantees the right balance between epistemic distance and proximity of the object to the subject, and hence facilitates a successful production of an intellectual commodity – no matter by whom and where (hence the positive hero can be a Bulgarian working in Siberian hinterland). Therefore Hann's reply was simple turning the tables on Buchowski by retorting: 'it's not us, but you' who is Orientalist.

Of course there is a kernel (or more) of truth in Hann's points, and one could find many examples that fit such argument. The point it, however, that there are also

examples that counter it. I will limit myself to the Polish case that I know best (for other countries, see: Benovska 2007, Bošković 2005, 2007, Podoba 2007, Skalník 2007, Uherek 2007). The reason why there is no single recent volume in CEE anthropology comparable to Edward Said's *Orientalism* could be simply because such debate had occurred long time before. Even literature, that is in theory more susceptible to romanticising, has ridiculed the intellectuals' passion for rustic lifestyle for example in a play from 1901 by Stanisław Wyspiański titled *The Wedding* – one of the classics of Polish literature. High-school, or even primary school curricula, are ripe with such works – for example Stanisław Mrożek's 1959 short-story *A Wedding in Atomville* that mocks the socialist attempts to modernise the countryside and adore its folklore at the same time is a compulsory read for fourteen-year olds to this very day! Even the monumental Nobel winning novel *The Peasants* by Władysław Reymont (written between 1904 and 1909) has nothing to do with nostalgia for the lost *Gemeinschaft*, but provides, amongst other things, a suggestive analysis of how capitalist property relations encroached Polish countryside in the late XIX century. Although (or maybe because) the very village where *The Peasants* unfold had become one of the icons of folklore after 1945, there is a very thorough monograph on it written by an anthropologist coming from the area (Jarecka-Kimlowska 1989). Such examples are abundant. To show that local anthropologists criticised the nationalist project and even positioned their own endeavours in opposition to it, I could mention that one of the towering figures of Polish anthropology Jan Stanisław Bystroń published in 1935 a book with a telling title *National Megalomania*.

Theory fetishism and reification of knowledge

One could go on, but it not the content, but the *form* of Hann's criticism that is most instructive. And it is beyond doubt that just as one could find most recent works released by prestigious American publishers that have deficiencies enlisted by Hann, one can find 'local' authors who place themselves in intellectual strands germane to the Anglo-American social anthropology. These are bracketed not because Hann or any other scholar are ignorant of it (in fact Hann's empirical work (1985) and Verdery (2004) reflected how doing library research in socialist Romania helped her to reach beyond Cold War categories), but rather because of the argumentative template they use. Today's most widespread *genres* of academic writing are very much structured and dominated by theory fetishism, and hence offer many easy 'totalising' traps to fall in. Only 'research that demonstrates a clear link to anthropological theory and debates, and promises to make a solid contribution to advancing these ideas', as Wenner-Gren Foundation (2007) guidelines for example make it clear, is worth supporting, and scholars are pushed by the global knowledge market to *show* they produce ever newer 'ideas' and 'theories'. This is why, when asked to justify the *raison d'être* of Western social anthropology of (post)socialism in CEE, it was more convenient for Hann to make the case that the market niche that he, together with other Anglo-American anthropologist of (post)socialism, occupies is novel and doing what they do has not been done in the region before – if it was not 'new', then it would be of little (market) value. In other words, today's academic world rewards making enemies over making friends and just as literature reviews a few decades ago tended to stress intellectual continuity and 'standing on the shoulders of giants' (often one's own teachers), today's academic publications tend to be more confrontational and in the most extreme cases are written in the

'they-all-got-it-wrong-but-here-I-come' vein. Of course it would be naïve to assume that CEE anthropology as a 'pristine precipitate' of national anthropology was spared from such 'theoretical revolution' that commoditises knowledge.

Those who so easily incarcerated blunt positivism in CEE anthropology hardly noticed that their peers behind the Iron Curtain were busy waging an anti-positivist battle too – only by slightly different means. The 1970s and 1980s gave birth to a 'writing culture' turn in Polish anthropology that eventually lead to a practice of anthropology as a 'purely theoretical, almost philosophical enterprise' (Buchowski 2004: 8), closely related to, if not even more extreme than, the Western post-modern cultural anthropology. Such anti-positivist, anti-communist and anti-materialist efforts (all these being each other's metonyms) elevated anthropology as the 'European science par excellence' (Kołakowski 1984, 1990) and cherished it as the most 'comprehensive' and all-encompassing of all possible bodies of knowledge. Its mission was to free humanism from the 'determinisms' of positivist Marxism and free the individual, now understood thanks to anthropology in his or her 'totality', for action. This was close to the spirit of the 'Solidarność' trade union, and coincided with the post-Vaticanum II evolution of the Catholic Church, which turned in the mid 1970s its discursive interest towards the 'human being as such' and placed him/her at the very centre of its rhetoric. Anthropology started to mean simply a 'science of the human', a science that took over humanism and optimism from the increasingly 'deterministic' Marxism, even though it was closer to arts (performative arts especially, and inspired some of the 'alternative cultures' of the 1980s), rather than to sciences. Just as the 'neo-positivist' social anthropology of (post)socialism, it was an ontological project, hostile towards the (communist) captive minds, and interested in clearing the grounds for generating a new kind of knowledge that would be adequate for the new era, or even for the New Age (Domańska 2005: 276-288). This is why it kept to the letter rather than to the spirit of *anthropo*-logy by being particularly interested in exploring the cultural and metaphysical origins of humankind, and revolved around issues such as myth, gift, ritual and sacrifice. Such 'theological anthropology' had its moments of glory, when for example anthropology-inspired theatre groups (most notably Jerzy Grotowski's *Laboratorium*, and more recently Włodzimierz Staniewski's *Gardzienice*) received world-wide acclaim, and especially when a version of it was made global in the multi-culti ecumenism of John Paul II (cf. Pobłocki 2004). Still today it is largely 'in the swim', and not only books, but even volume-long interviews with Western giants of that market niche such as René Girard (2006) are quickly translated into Polish and eagerly discussed.

To be sure, such theory-oriented local anthropology is ripe with shortcoming akin to any science that reifies 'theory'. A lot of it is, in Buchowski's words, a 'recycled good' (Buchowski 2004: 9), and many local papers, written also by scholars from other disciplines, typically take a foreign 'concept' and investigate whether or not it applies to the local reality. Just as locals compete in demonstrating proficiency in foreign 'theories', production of 'concepts' by metropolitan scholars usually does not require getting familiar with the local epistemic production. It could even impede it, since 'recycled goods' of often foreign provenience may 'corrupt' their ethnographic material (cf. Conklin 1997). They are also not so interesting, since their value on the global market jammed with such 'recycled' ideas is extremely low. 'Original' ideas, such as the concept of *sovkhoism* mentioned by Hann, or the 'dividual person' that allegedly captures the Polish idea of

personhood that is distinct from the Western notion of an 'individual' (Dunn 2004: 125-6) have a much better chance on the global market. Local anthropologists also hardly quote Western scholars writing on CEE, because if they borrow 'ideas' from Western scholars then they refer to 'tyrants' well-known for engendering 'ideas' such as Zygmunt Bauman, Richard Rorty, Clifford Geertz and James Clifford (Buchowski 2004: 9). So, we see that the 'reciprocal disinterest' between metropolitan and local anthropologist is a result of the different regimes of value structuring the academic markets they operate in. For that reason trips to foreign academic institutions, where a local anthropologist turns into 'a *flâneur* wondering in the library and searching for ideas' (Buchowski 2004: 9), do not have the magic quality, according to Hann, of healing CEE scholars of their nationalist mindset, but fieldtrips to countries that might be equally exotic executed according to the post-colonial cookbook do.

This peaceful co-existence in mutual ignorance was broken only recently. Perhaps because the Anglo-American anthropology of (post)socialism has established itself (in Western academic institutions) it can no longer be ignored by the locals. This, coupled with general weariness with the teleologies of 'transition' theory, as well as with a willingness to rejuvenate local anthropology that has became a little too 'cultural' and have somehow lost the 'social', sparked the recent native interest in Anglo-American anthropology of post-socialism (e.g. see Gdula 2006). Also metropolitan scholars realized that 'history matters' and even embarked on collaborative projects with local scholars and learned that their erstwhile 'arrogant disdain for native ethnography' that they regarded to be merely 'antiquarian' and their belief that their use of theory 'marked [their] own work as superior' was unfair, as Katherine Verdery (2007: 204) admitted in a 'self-criticism'. The bone of contention is now how to institutionalise this 'new' Anglo-American-styled social anthropology in CEE, and locals who had been previously deeply engaged in theoretical explorations are now being chided for not doing 'theory' at all, or at best for getting it wrong (Buchowski 2004: 11). To be sure, this is only partially the Anglo-American's intention, since the insistence that a 'new' sort of Western science needs to be imported to CEE comes partially, if not largely, from local scholars. As Katherine Verdery (2007) argued, we cannot speak of a neo-colonialism here, because the initiative comes from the local end, and those who claim epistemic superiority are actually used quite instrumentally by the locals who are astonishingly happy to accept the 'positivist/nationalist' label. This is hardly surprising: in order to compete on the local market, parochial anthropologist have to seek theories and paradigms outside of their own realm. External voices can be utilised in waging local battles against colleagues who can now be labelled 'positivists', 'idealists', 'culturalists' and so forth. Value of any theory is higher if it is new, therefore it is sometimes better to swallow a straw man because it can turn into a more powerful 'intellectual machine gun'. This is so especially since straw men are usually swallowed with a pinch of salt. Natives usually conclude (hidden transcripts!) that the cosmopolitans have only a 'shallow' understanding of the subject matter and that the they themselves know it better anyway (Buchowski 2004: 10).

Such straw men are key element of the academic merry-go-around – 'new' theories can be born only after other ones are flogged to death. It is perhaps time to draw conclusions from the post-Fordist restructuring of institutions – quite well described with regard to numerous industries, but still remaining largely a mystery as far as the centres of knowledge production are concerned.

For one thing, post-Fordism in the academia means increased competition and commodification of knowledge production, manifested on one hand by interdisciplinarity and one the other by theory-reification. If the Weberian pyramid-like organisational model, often used to describe the Fordist enterprise, allowed in the academia for a peaceful co-existence of various branches of knowledge immersed in their own (seemingly) independent research agendas, today's typical organisational model resembles, writes Richard Sennett, 'a circle with a dot in the centre. At the centre, a small number of managers make decisions, set tasks, judge results. … The teams working on the periphery of the circle are left free to respond to output targets set by the centre, free to devise means of executing tasks in competition with one another … In the Weberian pyramid of bureaucracy, rewards came for doing your job as best you could. In the dotted circle, rewards come to teams winning over other teams' (Sennet 2001). In the academia, this increased competition is euphemistically referred to as 'interdisciplinary'. This is why militant literature reviews with an increasingly short life-span are a must in today's publications, authors have to put as much effort into arguing that they have something interesting to say as into actually saying it, and academic audiences are much more impatient today and much less forgiving than they used to be. Huge conferences where speakers have up to 10 minutes for marketing their academic product in a flashy Powerpoint presentation is a relatively novel phenomenon. The accelerated competition between the metropolitan and parochial anthropologies is only a small offshoot of this larger process.

Towards a post-socialist anthropology

Our task, of course, is not to lament the current situation, but to find in it what can be utilised for envisioning future world anthropologies. And that we need a new cosmopolitan theory is beyond doubt. The fate of socialism – one of the most powerful cosmopolitan theories ever – that became something local, incarcerated in particular spaces, places or even people (Pobłocki, in print), is instructive. A pre-1914 socialist would find it wholly hilarious that socialism can be studied, and even more so that it was studied by anthropologists. I do not, however, advocate for a metropolitan appreciation of the 'theological anthropology', or other examples of 'native' theoretical reflection, but rather for a post-socialist theory that would not be a theory of post-socialism, but rather a new cosmopolitan theory that would grow out of both parochial and metropolitan efforts to effectively combat commoditisation of knowledge.

I see the recent evolutions within anthropology and the 'world anthropologies' debate as an excellent opportunity to revisit the little remembered criticism of Marx by Bakunin (1972). Bakunin argued that Marx differed little from Bismarck as he did not desire workers' emancipation, but hoped to grasp the increasing state power by mobilizing labour aristocracy that would lift the ascending group of 'socialist scientists' to became the fourth ruling class and establish 'reign of scientific intelligence, the most aristocratic, despotic, arrogant, and elitist of all regimes' (Bakunin 1972: 319). In fact it was a Bismarkian minister Johann Karl Rodbertus who coined the notion of 'state socialism' and Marx was often accused if not of plagiarising him (Kautsky and Engels put a considerable effort into defending Marx's authenticity as an author, Shatz 1989: 86), then at least, in Wacław Machajski's phrase, 'lending weight of pure science' to Rodbertus basic positions (Shatz 1989: 87). Machajski, following Bakunin, argued that Marx

wished to replace 'private capitalism' by a statist order that would guarantee the 'perpetual existence of national capital' and hence refine the method's of workers' exploitation. Marx's real enemy, Bakunin and Machajski argued, were not the capitalists, but what Bakunin called the 'flower of the proletariat', the uneducated, the unskilled, the 'dregs of society' disdained as 'Lumpenproletariat'. Machajski suggested that 'socialism' was a movement of the new bourgeoisie that sought to combat its 'elder brother', and argued that the Paris June Days of 1848 proved that struggling for political goals such as universal suffrage actually impedes workers' real emancipation (the chamber of deputes that was elected thanks to it shut national workshops for the unemployed that were according to Machajski the prime achievement of 1848), hence 'struggle for universal freedom is a bourgeois deception' (in Shatz 1989: 59), and so was the International. Before he was expelled from it, Bakunin argued that the International ought not to promote political solutions to economic problems, because workers of all countries cannot have a single, common 'interest', and that only a considerable degree of decentralisation and allowing workers in various world locales to develop their own political agendas and to struggle for their own *particular* interests, would create real unity in diversity in the workers' movement. Anthropology's early recognition that there is no single working class, but rather 'working classes' that vary in gender, ethnicity, race, age and so forth (Wolf 1982: 277, 358-60), its preference for an economic rather than political explanation, its embracement of the subaltern, and above all its recognition of the enormous and signifying differences in various world localities that by no means can be simply reduced to a single common denominator, bears a striking resemblance to the Bakuninian argument. Only for that reason to argue that CEE anthropology or ethnology was serving the nation-state project is wholly unfair, because even if it was utilised for mustering national allegiances, then still as a body of knowledge, unlike all other 'positivist' sciences, it was based upon this key awareness of the often radical differences between the many (geographical, ethnic, religious etc.) singularities that the nation-state comprised of.

After the roll-back of nation-state swept state socialism into the annals history, we may find Bakunin's points helpful. So far, however, a Bakuninian critique of Marxism/statism, came mostly from the right. Bakunin's most outspoken disciple was Fredrich von Hayek, who – in a famous exchange with Oskar Lange – condemned the state planners' arrogance in their belief that they would be able to first aggregate and then process all relevant human knowledge. Hayek is really worth quoting at length: 'today it is almost heresy to suggest that scientific knowledge is not the sum of all knowledge. But a little reflection will show that there is beyond question a body of very important but unorganized knowledge which cannot possibly be called scientific in the sense of knowledge of general rules: the knowledge of the particular circumstances of time and place' (Hayek 1945: 521-2). Hayek rebels against reification of knowledge into things as 'statistical aggregates' and turned economists' attention towards what anthropologist later called 'tacit' or 'local' knowledge. He offered two solutions to the problem of reification of knowledge by bureaucratic science: planners should be replaced by 'experts' who do not reduce knowledge to numbers-things, and planning should be replaced by the price system. How price system can express 'tacit knowledge'? Hayek relies here on a pre-20th century understanding of number, and just as 'statistical aggregates' had become for him clearly reified, he argues that the price system is primarily symbolic. Lorraine Daston (1995) showed that quantification never necessarily implied reification, or even reliance

on numbers. She described the various phases of quantification and showed that e.g. it employed geometrical figures as well as numbers, and it did not seek accuracy, only 'rough' precision. Daston argued that the movement towards quantification was in fact driven by the desire to enhance scholarly communication. Leibnitz put it this way: '[w]e need not to be surprised then that most disputes arose from the lack of clarity in things, that is, from the failure to reduce them to numbers' (in Daston 1995: 9). In other words, quantification was a result of accelerating communication and it evolved into hegemony of reified number-things due to the separation of thought and emotion. She draws here on the ideas of Ludwik Fleck, who argued in the 1930s that: 'the uniform agreement in the emotions of a society is … called freedom from emotions. This permits a type of thinking that is formal and schematic, and that can be couched in words and sentences and hence communicated without major deformation. The power of establishing independent existences is conceded to it emotively. Such thinking is called rational. (Fleck 1979: 49). Disillusioned by the hegemony of statistics that may have been good on 'clarity', but reified as an 'independent existence' was epistemologically poor and useless for social practice, Hayek pointed to an alternative 'mechanism for communicating information' (Hayek 1945: 526) with the expressive potential statistics had lost. What the price system could, and statistics could not, do, is to facilitate communication between people who otherwise have nothing else in common. His closing remarks are telling: 'the problem which we meet here is by no means peculiar to economics but arises in connection with nearly all truly social phenomena, with language and most of our cultural inheritance, and constitutes really the central theoretical problem of all social science… We make constant use of formulas, symbols, and rules whose meaning we do not understand and through the use of which we avail ourselves of the assistance of knowledge which individually we do not possess. We have developed these practices and institutions by building upon habits and institutions which have proved successful in their own sphere and which have in turn become the foundation of the civilization we have build up' (Hayek 1945: 528). The price system is hence an archaic and nearly unconscious institution, a common tradition to rely on, and so flexible that any local idiosyncrasy can be voiced through it. I believe that we are in a similar moment as Hayek, although it is not statistics that we need to dethrone. Hayek's project for localising economics ended in the blind lane of Nash equilibrium, that, according to Philip Mirowski, is the pinnacle of 'autistic' approach to science that wholly divorces it from reality (Mirowski 2002: 331-49). 'Local knowledge' also became 'big business' and today no expert panel can do without a discussion on how to 'tap it' (Kalb 2006: 579). In anthropology itself, the sheer volume of increased communication in the recent years inevitably lead to a process germane to quantification as described by Fleck and Daston, i.e. the hegemony of ideas-as-things, reified knowledge, only that if the hegemony of statistics was largely a result of the rising bureaucratic state apparatus (Desrosières 1998, MacKenzie 1981), then today's epistemic ills stem from the post-Fordist global knowledge commoditisation. Unfortunately, because 'theory' has been the major battlefield (or the nexus of exchange) on which the metropolitan and parochial anthropologies meet, intellectual communication has been limited to mere rituals of empty gestures (i.e. quoting without understanding of the real differences in respective research agendas, Buchowski 2004: 9). Therefore it is not lack of communication between anthropologies practiced in various world locales ('asymmetric ignorance'), but coping with its increased volume and transforming the *mode of communication* that is our main problem. For such Bakuninian

cosmopolitan theory that I advocate for not to share the fate of Hayek's attempt to eliminate 'things' from human thought and establish a democratic, universal and above all effective medium of communication, it may have to cease to be 'theory' the way it is generally understood today.

Anthropologies of peripheral capitalism

If anthropology today is largely a 'study how capitalism or globalisation, or violence etc. is *experienced*' (Smith, p. 472), and if anthropology of (post)socialism is a study of how the CEE places and spaces fit into larger processes of capitalist restructuring, then Frazer or Malinowski (as Hann (2007: 15-18) insisted) are not the best departure points for envisioning intellectual joint-ventures between metropolitan and CEE anthropologists. And it is wholly irrelevant that Malinowski was of Polish decent (Hann 2007: 17). If the notion of 'capitalism' (with all its shortcomings) is to be if not central then at least significant for a cosmopolitan world anthropology, then of course it is much better to start with Eric Wolf – as Don Kalb (2007: 167) suggested. Although Wolf as well comes from the region, and apparently his understanding of capitalism was largely shaped during his youth spend in antebellum Czechoslovakia (Ghani 1987: 349), his work is preoccupied with other regions that became 'peripheral' to European capitalism, and belongs much more to the Latin American tradition. Is therefore finding the CEE Franz Fanon indeed 'a distant dream and maybe as complicated as finding a Panchen Lama' as Buchowski (2004: 6) ironically concluded? Perhaps, but this is mostly because somebody had already found him/her long time ago.

My first positive hero is Immanuel Wallerstein, who to his own testimony – fascinated by the decolonisation movements of the 1960, the writings of Franz Fanon that he enthusiastically reviewed – coined his world-systems theory largely as a result of his encounter of Polish and Hungarian economic history (Wallerstein 2002: 360-3). Wallerstein's work to this very day is one of the best examples of most fruitful blend of metropolitan and parochial intellectual achievements, and if one wishes to build bridges between Anglo-American and CEE science, then Wallerstein is a much better starting point than Frazer. Wallerstein cannot really be called an anthropologist, but this is not because he has been uninterested in non-Western societies and how capitalism has been experienced there, but because he never privileged the strictly anthropological research method. But it was precisely for this reason that Wallerstein turned out to be a better anthropologist of the region that scholars with a strict anthropological training. It is remarkable that only Anglo-American historians, and not anthropologists, were ever accused of 'going native'. The most spectacular case was Norman Davies', whose *Europe. A History* (1996) has been rightly criticised for providing a strictly romantic Polish vision of European and world history. This is probably because historical training, unlike its anthropological counterpart, is not obsessed with achieving the enlightened balance between proximity and distance of the subject to the object of study, and promotes engagement with local epistemic production that is longer than the twelve-month-long fieldwork so much advertised by Hann (2005: 195). But this was also because there are many overlaps between history and anthropology they way they had been practiced in Eastern Europe (cf. Buchowski 2007).

This relationship is precisely the subject matter of Hann's longer piece (2007). He speculates, however, on the possible relationship between the historically-oriented 'new' social anthropology and CEE ethnology, instead of focusing on the relationship between history and anthropology at the CEE universities over the past decades. Hann mentions that, unlike in '[m]odern universities' that 'typically have a clear line of demarcation between faculties of social science and faculties of history' (Hann 2007: 24), in CEE ethnography institutes most often part of the faculties of history (Hann 2007: 25), but then he takes virtue for vice, since this is for him a proof for the *Volkskunde*'s positivism as well as the reason why local anthropologists were not interested in researching 'now' and hence did not do 'proper' fieldwork (Hann 2007: 25). But why not appreciate the fact that historical research done by local anthropologists is great to tap? The tradition to draw on is indeed rich: not only history and ethnology departments were somehow interrelated, but often people referred to as ethnographers worked at history departments, or historians published in ethnology journals and volumes and so on. The marginal important of historical anthropology in the Polish academia is a result of to the success of the non-positivist anthropology-inspired history (Piasek 2004: 5). Just a quick glance at the anthropology quarterly *Konteksty* confirms that this is still the case: of out the 40 issues in the last 10 years, 9 issues were devoted solely to history (of which 4 were on memory, 1 on biography). History was the third, next to visual anthropology (8 issues) and performative arts (8 issues), 'big topic' in this journal of 'cultural anthropology' still subtitled 'Polish folk art'. Not only ethnographers, but also historians and literary scholars, contributed to the debates on anthropological history, or historical anthropology. Needless to say, just as such debates were never discussed by Hann and others, articled in *Konteksty* also referred to its own history-anthropology canon, and hence shows that even when addressing an identical issue, the metropolitan and local discourses hardly overlap. Although such interest in history is also a recent fashion (for some older attempts to find a theoretical perspective of combining history and anthropology, both Western and non-Western, see Kochanowicz 1992), if only for its limited size, the Polish academic world has always been quite interdisciplinary. Moreover, both history and ethnology departments have always been somehow localised, i.e. focused mainly (but not exclusively) on the region they were placed in. In fact the 'unfinished project' of Wallerstein's theory lacks classic anthropological insights: it seems that he draw too hasty conclusions from the work of Marian Małowist that he knew best and failed to reincorporate local criticism of his own argument. Małowist's data was pooled mainly from the coastal Gdańsk area, where – because of the city's role as node of export of grain – the manorial economy was most connected to Western European markets. This was not, however, the case with other regions, where internal markets were equally (if not more) important for the manorial economy. So this weakness of Wallerstein's theory lies in his inability to tap from research done in regional centres (in this case Poznań most notably, cf. Topolski 2000: 41-45).

This is why I suggest that a study of CEE capitalism (that by no means started in 1989) should engage with Wallerstein's work, but not by 'importing' world-systems approach as a 'superior theory' (this has already been done by both Anglo-Americans and locals), but rather by re-enacting the type of intellectual adventure Wallerstein once embarked on. Wallerstein argues that '[t]he term theory … denotes the end of a process of generalisation and therefore of closure, if only provisional' (2002: 358) and warns against theory fetishism by giving an

'itinerary to resist becoming a theory' for what he sees primarily as a *method of analysis*. We should follow his advises and try at least to bracket the commoditisation of intellectual production, if it is not realistic to advocate for a frontal assault on it. Straw men, who populate today's intellectual landscape, should be made redundant. This can be done only after theory fetishism – the main tool for manufacturing positivist straw men – had been rejected. Such approach necessarily separates theoretical and empirical contributions, and values the former higher, since only such can be 'sold' on academic market. Since only 'theoretical' input, contrasted to contributions carrying merely 'empirical value', can be communicable to 'outsiders', only such are deemed relevant for cosmopolitan community. And since volumes published before the 1990s seldom work within such clear-cut theory/empirical data dichotomy, they can be very easily labelled positivist, and their *raison d'être* is reduced to the poor by today's standards 'filling in the gap'. But this is a problem posed even by some of the Western classics that were hardly ever labelled 'positivist'. For example E.P. Thompson's *The Making of the English Working Class*, perhaps the most brilliant piece of native anthropology written by an Anglo-Saxon author, would seem obscure, lengthy, and overly detailed for such contemporary reader looking for new theoretical insights.

We should not seek 'convergence' of the erstwhile national anthropology with post-colonial *theory*, or with the post-colonial *research method* (as Hann insists), but by resurrecting the dead horses that are being flogged to this very day by cosmopolitans and natives alike, we should recuperate what is inspiring in the former 'anthropology of nation-building' and forge a locally-anchored anthropology 'without a nation state' that is oriented towards talking to other 'world anthropologies'. My second positive hero is Józef Bursza. I am sure that countless examples like this can be found among local anthropologists both dead and alive. His book (Burszta 1950) on the role of vodka in making of the Polish peasantry and the manorial class relations does not feature a literature review with a discussion of recent fads in consumption studies. It emerged as an attempt to account for how vodka became an integral part of the peasant everyday life and folklore, and it describes the various ways Polish gentry forced their peasants to consume vodka since the XVI century. It is as innovative and inspiring as Sidney Mintz's *Sweetness and Power* written three decades later, and features some 'concepts' that could be fished out and sold on the academic commodity market. For example his notion of 'double moral bookkeeping' [*podwójna buchalteria moralna*]. It captures the divergent moral, legal, political and economic standards used by the gentry with regard to the economy at large and to the very villages and peasants they owned and administered. The complete freedom of trade the gentry enjoyed (no import duties) was null and void with regard to their own villages – and there monopolies were imposed, and peasants were forced to buy vodka only in the inns owned by their legal master. This double moral bookkeeping was particularly visible in constant efforts of local gentry to attract peasants from adjacent villages to drink in their inns, and hence to undermine the monopoly of vodka marketing of their own neighbours. Burszta describes at length how the nobility and the Catholic church used moral preaching in trying to turn manorial peasants into docile labourers and consumers, while undermined this very 'moral bookkeeping' by their own economic practises, and sees the origins of the notorious 'immorality' of the manorial peasant in this. This notion bears some similarity to Thompson's 'moral economy', and could have made equally global career. The similarity lies also in

the fact that neither Thompson nor Burszta run around flagging their 'concepts' and arguing that they are useful for cosmopolitan theory, nor they considered their work to have primarily theoretical value. Instead, their value was *both* empirical and theoretical: Burszta's credits are in the way he explained the growing importance of production and consumption of vodka for the manorial economy in Poland. By today's standards, such 'filling in the gap' is not enough. The point is, however, that this filling in the gap was significant for theory that Burszta operated within – trying to explain the rise, the nature and the demise of the manorial economy in Poland, the classes it produced and the heritage it had left. It could seem 'so little' by today's standards, but if we are interested in how capitalism has been experienced at the grassroots in the *long durée* , books like Burszta's are indispensable.

Hence flirting with the 'captive minds' is my recipe against theory fetishism. A brilliant proof that dangerous liaisons with the 'captive minds' could turn out very fruitful is provided by the my last positive hero – the historian Stephen Kotkin. His compelling *Magnetic Mountain: Stalinism as Civilization*, describing the emergence of the Soviet steel-town Magnitogorsk in the 1930s, is based almost exclusively on official sources, mainly local newspapers. In theory Kotkin embarked on a mad venture – he decided to find clues about everyday life by reading official newspapers from the 1930s – the heyday of Stalinist terror – issued in one of the icons of Soviet industrialisation. It turned out, however, that he could find a lot of insightful information in such sources, and his volume (Kotkin 1995) is one of the very few books that successfully escapes Cold War dichotomies of socialism/capitalism, totalitarianism/democracy, state/market economy and so forth. In fact, Kotkin (1991) also published a monograph where he described how Magnitogorsk urbanites lived socialism of the late 1980s, based upon a classic anthropological fieldwork that he could do every day after the local library was shut. These two volumes brilliantly feed one into another and demonstrate how one may most successfully blend historical and anthropological research methods under contemporary twelve-month research time constraints. In fact Kotkin did what Buchowski was advocating for: he eschewed the Cold War epistemic traps by altering his units of analysis – in short, instead of thinking in terms of socialism vs. capitalism he put on the urban-rural lenses. Of course all dichotomies are problematic by nature, and Kotkin actually shows how the industrialising Soviet society remained surprisingly rural. But had the Anglo-American anthropologist admitted that they were not really on an adventurous trip in the foreign land of socialism or (post)socialism, but actually researching a *rural* locale, then it would have been much more difficult for them to ignore the vast body of literature produced by locals on their own countryside. Hence the controversy between Buchowski and Hann really boils down to placing the Anglo-Saxon anthropology of (post)socialism in a different tradition. For Buchowski these are works that essentially are part and parcel of the studies of the CEE hinterland – of which the vast majority was written in vernaculars and that foreign authors usually fail to refer to. For Hann, however, these works are part if not of a unique intellectual strand of 'transitology', then at least of Machester-school inspired global social anthropology, and if perceived so, then it is no wonder why local ethnographic work is of little use – after all authors like Burszta did not write on socialism, let alone on post-socialism, they did not do the typically British-styled fieldwork, so why bother about reading them? It is another issue that only Anglo-American historians of Polish socialism noticed that urbanisation was one of its most salient features of post-war Eastern Europe

(such as Padraic Kenney, whose volume on Wrocław and Łódź (1997) offers to this day one of the most inspiring comparative set-ups and also profits from incorporating indigenous academic production), and Anglo-American anthropologists usually went to the most 'backward' parts of the country. By first creating, and then defending, a largely bogus unit of analysis such as 'post-socialism', Anglo-American scholars essentially did the same what Western managers did to Polish firms and economies – create 'new' market niches and pretend that they had not existed before – as it was brilliantly described by Elizabeth Dunn (2004, chapter 3). Of course, just like the managers, they were more than often induced by the locals to do so. But such niche marketing strategies necessarily lead to an increased fragmentation of world anthropologies into academic 'camps' entrenched in their discourses with distinct and hardly overlapping cannons that relate to one another only by reified theories, straw men, and really pass each other like ships at night while fighting to grab often common resources. Therefore it would be useful, as Don Kalb argued, for metropolitan and local scholars alike, to 'help to lift the region out of its obsession with its putatively singular post-socialist predicament and its singular orientation on a putative West' (Kalb 2007: 173), and only after this has been done, we may contribute to building world anthropologies that represent a comprehensive (albeit not homogeneous) theory, or – better – a method of analysis.

Beyond 'anthropology of anthropology'

Finally, it is overly arrogant to assume that 'an anthropology of anthropology' is sufficient a methodology for unveiling the current hierarchies in knowledge production and consumption. In order to lift the discipline from its 'putatively singular predicaments', relationships to other bodies of knowledge, and especially to 'hard' sciences, should be scrutinised. And this is still largely a virgin land. The only anthropological work that would tackle this issue is Michael Adas' *Machines as the Measure of Men*. Written in a truly Wolfian spirit, this books became a classic in science and technology studies, but has not sprung so much interest in anthropology itself. This is a gross pity, since Adas' analysis of how technology replaced religion as the central criterion for measuring 'worth' of human beings (and societies at large), how machines became the central meeting grounds between European travellers and the non-Europeans they meet, and how gradually the idea of one universal material world that is measurable and can be accounted for by European machines (and physics), is ripe with clues that could rejuvenate even the most classic anthropological themes, such as the conundrum of fetishism. Adas shows in detail how European travellers thought they alone were skilled enough to use Western machines, and how they thought the natives were either unable to use even the simplest Western tools, or they used them for other purposes and hints that the notion of fetishism could have emerged as the Western misconception of the alleged native misuse (i.e. worship) of the machines Europeans brought with them (cf. Adas 1989: 126, 158-161, 224-5, 237, 380). And that unmasking the most basic believes about hard sciences may enrich anthropology can be seen by the acclaim for Eduardo Viveiros de Castro's (1998) brilliant essay *Cosmological Deixis and Amerindian Perspectivism* that was a tribute to Bruno Latour's *We Have Never Been Modern*, in the illuminating discussion of Durkheim and African religion in Anthony Appiah's *In My Father's House* (1992, chapter 6), or in the

compelling volume edited by Laura Nader (1996) where also 'native science' is taken seriously.

In fact such move has already been initiated from another end – Donna Haraway's *Primate Visions* is one notable example, and more recently *Einstein's Clocks, Poincaré's Maps* by Peter Galison (2003) is another. Galison shows that not only the central issues of Einstein's theory of relativity were the sort of questions that he encountered as a clerk in Bern patent office, but that the questions of simultaneity of time in various spaces (i.e. synchronising distant clocks) were a general and burning concern in Einstein's time, for example for co-ordinating railroad schedules. He shows that Einstein's thinking of time and space run smack into projects undertook by Henri Poincaré at the French Bureau of Longitude such as mapmaking with the use of submarine cables (establishing relative distances between places by sending electromagnetic signals through them), agreeing on where to locate the prime meridian, decimalisation of time, and the long-fought battle to measure the exact distance between Paris and London. Following Lorentz, Einstein rejected the principle of ether, arguing that only relative speeds of moving objects could be determined, and the speed at which Earth moves through the ether is impossible to measure, and hence abandoned the last objective, absolute frame of reference (vestige of the Christian and Newtonian worldview). Poincaré, unlike Einstein, never fully rejected the principle of ether, perhaps because the idea that there is no unifying substance that our world is immersed in was too horrifying to accept. Sure, he thought that time, space and frames of reference were mere 'conventions' subject to arbitrary decision. Yet, unlike Einstein the deliberate exile, he was struggling (as during the campaign to establish the prime meridian in the vicinity of Paris and not in Greenwich) to keep the tools for co-coordinating the global panoply of clocks in his, or rather in France's, hands. He was a loyal subject, a French patriot, respectable engineer and a clerk. Einstein, on the contrary, 'was never out to repair and uphold any empire – neither the French, nor the Prussian, nor the Newtonian" (Galison 2003: 310). For Poincaré there was 'local time' as contrasted to 'true time'. For Einstein there was only a multiplicity of local times (Galison 2003: 317), no centre but only a plurality of time-spaces (Galison 2003: 292-3). Therefore it is an irony that, as Galison argues, Einstein continued the nationalist project of General von Moltke, who inspired by his quick military victory over France thanks to his dextrous use of precision-synchronized trains, set out to unify the German hodgepodge of mechanical and electrical time systems as early as 1889 and use time as the unifying tool of the hodgepodge of the German peoples (Galison 2003: 156-159). Of course, he drew on the older military tradition of mastering the methods of 'keeping together in time' (McNeill 1995) and did it while *Volskundists* were working on the concept of 'culture' for essentially the same purposes, but only with the use of humanities.

I believe that such turn from physic to hermeneutics in both Einstein and Poincaré was taken a step further by Malinowski, who set out to map in greater detail the local time/spaces when the colonial Empire was already on the wane. The link is direct here – Einstein was inspired by the writings of Ernst Mach, who criticised the Newtonian notions of absolute space and time as medieval and naïve, and called Mach the 'forerunner of theory of relativity' (Galison 2003: 236-7). It was Mach's philosophy of science that Malinowski wrote his dissertation on (Flis 1988), and followed his advice to examine the Western science in its 'lower phases of maturity', what he dubbed later as 'ethnoscience' of the primitive

peoples (Gonzalez, Nader and Ou 1995: 867-9), and just like Ludwik Fleck working in the nearby Lviv, he was well aware of the 'conventionalist' trends in philosophy of science, and both befriended Leon Chwistek, whose idea of the 'multitude of realities,' which all exist with equal rights, contiguous to one another and based upon mutually incompatible axiomatic systems, was inspired chiefly by Poincaré and Duhem (Gonzalez, Nader and Ou 1995: 867). The lack of political allegiance to any nation-state nor empire and Einstein's idea of the plurality of time-spaces was leaning towards anarchism (although because of his belief in the power and universality of science, he argued that economic socialism is undoubtedly superior to 'capitalist anarchy', and actually cheered for socialism), and it were in fact the anarchists who set out to destroy in a bomb assault on Greenwich the symbol of time uniformisation and control promoted by state officials such as Poincaré. The bogeyman of 'anarcho-clockism' was well-known in Europe of the time (e.g. the Jura watchmakers were one of the strongest footholds of anarchism in Europe, and e.g. Kropotkin's stay there sealed his rejection of socialism in favour of anarchism – Galison 2003: 226), and it was in the world of electrocoordinated clocks organized by the modern state that was to free humanity from it, as e.g. Albert Favarger, whose mentor the clock-constructor Mathias Hipp was arrested for consorting with anarchists, vehemently argued (Galison: 226-7). Although Malinowski was even further from anarchism than Einstein, he must have been aware of such time/space struggles, only if from reading Joseph Conrad's *The Secret Agent* that describes the anarchists' assault on Greenwich. Still we have a vast discussion of Malinowski's anthropology, a volume on Malinowski the Pole (Gellner 1988), some works on Malinowski the artist (e.g. Clifford 1988, chapter 3., Wright 1991), but only one short article that merely drafts Malinowski's portrait as a natural scientist (Gonzalez, Nader and Ou 1995). And there are mulitple paths to take: one could compare Malinowski's fieldtrips to earlier cartographic ventures such as Poincaré's Quito Mission (Galison 2003: 191-198), or trace, for example, if there are any links between the Malinowskian idea of 'fieldwork' and natural sciences. Malinowski's commitment to empirical research was heir to Mach (Flis 1988: 123-125), but the notion of 'fields', as well as of 'local time', was developed by Hendrik Lorentz, and 'work' too is a basic concept of nineteenth century physics (Rabinbach 1992). And that looking at 'philosophers' or 'social thinkers' as hard scientists is possible, and above all fully legitimate, can be seen by the fate of Hobbes' physical treaties that were first translated from Latin in… 1985, as an appendix to Schaffer and Shapin's brilliant study (1985) that effectively buried the dichotomy between hard sciences and humanities. It is crucial for effective 'anthropology of anthropology' to bridge that gaps, also because physics has been long replaced by cybernetics as the 'first' ontological science (Bowker 1993, Mirowski 2002: 57-61), and its interest in information, symbols, language etc. made the 'hardness' of hard sciences even more problematic, and in fact anthropologists keenly participated in such developments, for example by taking part in the Macy Conferences that were a crucial step for shaping post-war cybernetics (Edwards 1996: 189). There is a compelling study on the relationship between economics and both physic (Mirowski 1989) and cybernetics (Mirowski 2002), but although the increasing number multi-sited ethnographies of anthropology and development and volumes such as the one on audit cultures (Strathern 2000) are very illuminating, the critique of anthropology as a body of knowledge embedded in global politics is still not much more than patchwork and we still have to await a synthesis showing the recent history of our discipline in a fully comprehensive and critical manner.

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