

what, in this book's sub-title, is the meaning of 'empirically-grounded'.<sup>60</sup> Half a century ago I received a very long and intensive training as an empirical social scientist – seven years full-time before even the Drs examination, with a similar duration then standard, at least in the Netherlands, for the attainment of the doctorate. The self-evidences (*i.e.* the blinkered sociological presuppositions, if you want) of the social sciences were inescapably ingrained in me during that period, and I admit I cannot and will not get rid of them at this late hour. In the last analysis, this means that I have learned to doggedly take the hard-earned, yet fragmentary and partly mal-observed and mis-understood data from the field as the ultimate criteria against which my scientific pronouncements are to be tested in order to make sense to me as an anthropologist – *call it a naïve empiricist point of departure that ties the anthropologist's hand to the point of making her or him almost unfit for free discursive thought*. It is not lack of thinking power that brings me to adopt this awkward position. It is awareness that in the last analysis the conceptual and interpretative initiative lies, not with the anthropologist, but with the competent local socio-cultural actor whose life and thought ethnography and ethnohistory are to represent... *vicariously*. Thus reduced to a humble secondary position, to dependence, the anthropologist tends to reduce, in the light of the primary field data, all philosophical reflection to a subordinate level – to an embellishment, a footnote, a literary trope (meant to grant a semblance of bibliographical and socio-political topicality, conceptual sophistication and erudition to one's ethnographic texts). As I have argued elsewhere (2003b: 498 *f.*), the anthropologist is used to 'rough it' – and rather than departing from an explicit theory and seeking the data to substantiate or explode it, usually prefers to let herself or himself to be guided by the flow of personal inspiration produced at the local actors' initiative by field data at hand, spending only so much time and effort on conceptualisation and theory as seems needed to make the resulting ethnographic argument more or less presentable as a scientific text.

C. MAKING ETHNOGRAPHIC CLAIMS THE EASY WAY – BUT SPURIOUSLY. This humble, secondary, dependent and empiricist orientation was ingrained in me from my earliest years reading anthropology. My principal teacher then, André Köbben, had gained his PhD with field-work among the Agni and Bete of Ivory Coast (1955), and during his lectures on field-work method in the mid-1960s he would fulminate about a team of 'Culture and Personality' researchers, who had come to ascertain, during a prohibitively short stay at Köbben's West African field site, the prevalence of Freud's Oedipus complex there, without bothering to construct ethnographic authority for themselves by making such considerable professional investments in residence, language acquisition, and cul-

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<sup>60</sup> It is the inspiration of my participation in the Symposium 'Chaos in the Contact Zone', University of Rostock, Germany, August-September 2015 (although most participants were not philosophers but historians and literature scholars) that drove home to me the backgrounds and implications of the loss of authority characteristic of the social sciences in post-modern academic constellations. I wish to express my gratitude to the convener Prof. Stephanie Wodianka, and to the organiser Dr Andrea Zittlau, for inviting and accommodating me, and for patiently contributing to my self-reflection.

tural learning through participant observation as Köbben had himself made and as he was to require his students to make. The easy approach he was chiding was not uncommon among anthropologising psychoanalysts. A few decades earlier (1925), the iconic Carl Gustav Jung (Burlinson 2005, 2008; Collins n.d.), then already 50 years old, had made an ethnographic sally to the Elgonyi people of Mt Elgon, one of the highest mountains of Africa, at the Ugandan-Kenyan border, and was inevitably but unreliably confirmed in his Lévybruhlian and essentialising ideas (Wilmsen 1993 / 1995). Jung had had no ethnographic method or procedure to speak of, nor any local cultural and linguistic competence – and by his own admittance got practically no information out of the Elgonyi. The unmethodic eagerness with which armchair anthropologists (and, in this case, historians of religions) would jump to conclusions as to the proposed universality of the Freud Oedipus Complex is also emphatically clear throughout a minor classic as Fokke Sierksma's *Religie, Sexualiteit & Agressie* ('Religion, Sexuality and Aggression' – 1979 / 1962). Another psychologising ethnographer of the first half of the 20<sup>th</sup> c. CE, Margaret Mead, fared only slightly better than Jung – after worldwide success with her studies of South childhoods, also her field-work methods were found to be defective (Mead 1928, 1930, 1935; cf. Freeman 1983). Testing theories in the field has been almost anathema among professional anthropologists; instead, they have tended to rely on crude, naïve induction: *let the empirical facts speak for themselves, or at least* (since even anthropologists now realise that apparent facts are preconditioned by the researcher's mind set and paradigms) *let them have the first and the last word*.

Much as, in later days, I have had to realise the considerable limitations of my brilliant teacher's anthropology (a fixation on social relations, lack of statistical sophistication, no room for art or material culture, little room for material and economic relations, for myth, for meaning, for continuities in space and time, for history, for library research, and finally a naïvely social-democratic political outlook), there was yet great value in what I was given to learn. I have never been able to shed Köbben's lessons as to the necessary underpinning of anthropological thought by prolonged field-work experience, and as to the secondary nature of theory (easy to formulate, to play with, to impose, to criticise, to replace) as compared to what a local society had to offer through direct and time-consuming participant observation *from the typically humble, locally powerless and reticent stance of the ethnographic field-worker*, where not the researcher but the local actors call the tune and determine what is interesting though harmless enough (!) to share with the inquisitive outsider. Köbben's period as a leading anthropologist was mainly the 1960s, and preceded the counter-hegemonic discourse of the late 20<sup>th</sup> c. by decades. But the title of one of his books, *Van Primitieven tot Medeburgers* ('From Primitives to Fellow-Citizens' – 1964), brings out very clearly the *counter-hegemonic stance* that was, *avant la lettre*, the essence of his anthropology.

Building an anthropology that is vicarious in the sense that it ignores the perceptions and conceptualisations of the people we write about, is Faustian (cf. von Goethe 1981 / c. 1800 CE), and objectifying in the Sartrean sense of *dehumanising* (Sartre 1943). I have often both admired and chided the unlimited freedom my philosophical colleagues allowed themselves to 'think through' or, preferably still, 'think *beyond*', not only aspects of the present-day social experience in the North-Atlantic region, but also extending their appropriative gaze to the West African Dogon, to African proverbs, shamanism, the African atti-