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INTRODUCTION:
FIVE THESES ON ETHNOGRAPHY AS COLONIAL PRACTICE[1]

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Despite the influence of Thomas Kuhn on critical assessments of anthropology, disciplinary histories written by anthropologists still tend to be self-serving. To this day, it seems evident to look upon the great thinkers of anthropology, those whom we think revolutionized its theories and methods, as the main carriers of the history of anthropology. Our own research projects, concerning localized, contextual histories of ethnographic practices in colonial Tanganyika (Pels) and Vietnam (Salemink), made us consider the history of anthropology from another angle. We feel that the emphasis on the "big men" of anthropology in disciplinary histories obscures the way in which ethnography was linked to the construction of colonial and neo-colonial societies. In the following text we elaborate some theses on the historical relevance of ethnographic practice, understood in relation to the anthropological discipline and to its respective local and historical contexts. These theses are obviously not the definitive outcome of a rewriting of anthropology's history, but we consider them to be necessary steps toward a critical reflection on the relations between ethnography and colonialism.

First Thesis: Disciplinary history obscures the way in which academic anthropology was linked to the construction of colonial and neo-colonial societies through ethnographic practice.

When the collection of essays on *Anthropology and the Colonial Encounter* (Asad 1973) was published, its message was drowned in heated arguments. Despite Asad's statement that "it is a mistake to view social anthropology

in the colonial era as primarily an aid to colonial administration, or as the simple reflection of colonial ideology", a contemporary effort of critical anthropologists (Hymes 1974) resulted in debate about the way in which British anthropologists had been engaged in "aiding and abetting British colonial policy in Africa" (Scholte 1975: 45).[2] Indeed, in that context, the volume edited by Asad was understood as making precisely that point (cf. Loizos 1977: 137; Ortner 1984: 138). Similarly, the claims that anthropology provided an ideology legitimating European feelings of superiority (cf. Lewis 1973) was countered by professions of left-wing sympathies (Gluckman 1974; but see Brown 1979) or the one-sided statement that "the only inferiority which most social anthropologists have ever stressed has been a technical one" (Firth 1977: 152).

The issue of the practical or ideological complicity of anthropologists in the construction of colonial (or neo-colonial – Horowitz 1967; Wolf and Jorgensen 1970) power was crucial for debates accompanying a radical shift of claims to anthropological authority from classical anthropology to the more politicized perspectives of the 1970s (see Pels and Nencel 1991). It was often accompanied by denunciations of the opponent's lack of historical consciousness made by both parties.[3] Yet, history was a remote concern for most participants in the debate. Forster's balanced overview of the New Left critique shows that the critics focussed on the theoretical limitations, or the lack of social responsibility, of classical anthropology (Forster 1973: 24), not on its history. Similarly, defenders of classical claims were also not inclined to study history very closely: Leach presumed the existence of a "sociology of *colonialism*" in functionalism (Leach 1974: 34; our emphasis), ignoring that it was usually called "culture contact" (Leclerc 1972: 55). Gluckman's formal denial that as an anthropologist he was never a member of His Majesty's Government (1974: 43) of course does not imply that he abstained from doing the work of government (see Scholte 1975; Brown 1979). In contrast, many contributions to *Anthropology and the Colonial Encounter* provided some of the first studies of anthropology in its historical, colonial context.[4]

More recent assessments of the impact of the critique to which Asad's volume was a contribution show a similar lack of historical consciousness. *Anthropology and the Colonial Encounter*, despite occasional praise (Clifford 1986: 9), is usually included in disparaging assessments of the critique of the 1960s and 1970s. The critical anthropologists' negative portrait of the anthropologist has, for the new generation, "hardened into caricature" (Clifford 1986: 9); the critique "merely scratched the surface" (Ortner 1984: 138) and its overall effort "was too immoderate and ungrounded in practice to have much effect" (Marcus and Fischer 1986: 35). Yet, the critique went sufficiently deep to have the effect of inverting and unsettling anthropologists' claims to academic authority (Pels and Nencel 1991). It remains relevant for so long as much of postcolonial anthropology is still based on

raw materials delivered by "informants" which are processed into "cultural" identities that can be sold on Western academic markets (cf. Galtung 1967; Lewis 1973).

Such attempts to "write off" academic debates usually go together with attempts to inscribe oneself in the discipline (Pels and Nencel 1991: 17), for instance, by claiming the American culturalist experience as the rationale of all anthropology (Marcus and Fischer 1986: 22), or by setting up one's own standard by calling the other's "provincial" (Leach 1974: 33; cf. Diamond 1974: 37–38). It is a rhetorical absolutism which hides a "parochialism of the present" (Levenson, quoted in Stocking 1968: 5) by formulating its own claim to authority in terms of the rationality of the discipline as a whole. This can only lead to what George Stocking called a "presentist" attitude towards history (1968: 1–12), evident from a large number of efforts of anthropologists to write the history of their discipline.[5] However, despite the influence of Thomas Kuhn's call for a historicizing disciplinary history (1970) on critical debates within anthropology (cf. Scholte 1966, 1978, 1983), its use was mostly restricted to a strategic use of the paradigm concept, amounting to a proliferation of "Whiggish histories". As Regna Darnell noted:

> A great deal of purported history of anthropology [...] is far from contextually accurate or historically sophisticated. Practitioners as quasi-historians frequently use history to argue for present theoretical concerns. (1982: 268)

In spite of attempts both before and after the publication of *Anthropology and the Colonial Encounter*, it can be doubted that the call for a historical contextualization of anthropology has been sufficiently "historicist", taken as the ideal to "understand the past for the sake of the past" (Stocking 1968: 9).[6] It is remarkable that the relevance of anthropology for the colonial encounter has hardly been a subject for historical study (but see Brown 1979; Cell 1989; Johnson 1982; Kuklick 1978; 1991) until the latest issue of the *History of Anthropology* series (Stocking 1991a) – although this relevance has by now been accepted by the majority of anthropologists as a "commonplace" (Stocking 1991a: 4). We feel that this is – at least partly – due to the fact that the status of anthropology as an academic discipline is too much taken for granted, even in much scholarship of professed "historicist" character. Therefore, an essential methodological move in the study of the history of anthropology needs to be made: the dialectical one of accounting for the extra-academic and extra-disciplinary influences on the constitution of the discipline.

In 1953, Meyer Fortes wrote that "[i]t is characteristic and important that anthropological studies owe a great deal to enthusiasts from outside the academic world, to officers of the Crown, to missionaries, traders and

travellers". In France, Maurice Leenhardt was an important missionary presence among anthropologists (Clifford 1982), while in Austria and Germany, Father Wilhelm Schmidt and his journal *Anthropos* made an important impact on behalf of his congregation, the missionary Society of the Divine Word (Brandewie 1990). Yet, histories of anthropology do not usually consider missionary anthropologists and when they do, their missionary background is thought to be of no importance.[7] The importance of the administrative background is illustrated by the facts that in Britain, Cambridge anthropology owed a lot to the Orientalist and administrator Sir Richard Temple, and that as late as 1953 Meyer Fortes succeeded to a Cambridge professorship which was handed down by two former members of the Indian Colonial Service, T. C. Hodson and J. H. Hutton (Fortes 1974: 427; see West, this issue). Malinowskian functionalism could not have established itself without the support of missionaries like J. H. Oldham or administrators like Lord Lugard (see Cell 1989; Stocking 1985; 1991b). Events like the Protestant missionary conferences at High Leigh (1924) and Le Zoute (1926) tied up extra-academic missionary anthropology with the network of Oldham and Lugard (Forster 1989: 27). These cases indicate the importance of nonacademic influences on the establishment of academic anthropology.

In a sense, there is a seductive logic to the focus on disciplinary histories, for it seems evident to look upon those who revolutionized the theories and methods of anthropology as the main carriers of the history of the discipline. Yet, we should take account of the fact that "disciplinary history does not exist until its view of the past is ratified by members of the discipline" (Darnell 1971: 87). Disciplinary history holds on, for example, to the legend that Malinowski "invented" modern fieldwork methods (Kuper 1983: 13). In fact, Malinowski managed to produce this impression by consistent "self-fashioning" (Clifford 1985), a propaganda which concealed the fact that he drew upon earlier examples (Stocking 1983). To a large extent, the professionalization of fieldwork in British anthropology depended on the tactical denigration of both missionary and administrative ethnographies (Pels 1990; 1991; Thomas 1989: 69 ff.). One has to study, not accept, the way in which *Argonauts of the Western Pacific* set up the boundaries between the academic "Ethnographer" and his rivals (cf. Malinowski 1922: 1–25). Moreover, the concentration on the intellectual giants of the discipline obscures the links between anthropology and colonial work, for the simple reason that the giants were occupied with a purely academic career, whereas lesser figures often operated outside of the academy. Lastly, the discussion about anthropology and colonialism almost completely ignores the pre-professional fieldwork phase, and consequently, the impact of Indian Civil Servants like Herbert Risley and Richard Temple on academic anthropology.[8]

Foucault argued that one can only understand a discipline through the ways in which it fixes its limits (1972: 224). This implies that one has to move beyond academic anthropology to understand its emergence and reproduction. *Anthropology and the Colonial Encounter* embodied that realization, through, among other things, a number of papers on administrative ethnographic practices (Lackner 1973, Clammer 1973, Owen 1973), and through Asad's argument that not the complicity of anthropologists with colonialism, but the location of anthropology in the colonial context, was the crucial issue (1973: 18–19) .

Second Thesis: In order to understand the historical relationship between anthropology and colonialism, it is better to regard academic anthropology as a specific instance of ethnographic practice than the other way around.

Although the recent studies of ethnography from a literary perspective have brought to light previously unacknowledged relations between text and (colonial) context (Clifford and Marcus 1986), we have seen above that they do not tend to broaden their scope beyond the confines of the discipline and that they rewrite anthropological history to suit their present demands (but see Fabian 1983, Pratt 1985). The call for experimentation with ethnography is in itself a new claim to academic authority, and a weak one at that, because the problems it identifies (power inequalities in ethnographic representation) are not solved by the solution it proposes (new representations, cf. Fabian 1991: 193). "Dialogic" experiments make much of a seemingly democratic encounter with the interlocutor, but tend to ignore, as Said puts it, that "this kind of scrubbed, disinfected interlocutor is a laboratory creation with suppressed, and therefore falsified, connections to the urgent situation of crisis and conflict that brought him or her to attention in the first place" (1989: 210). Moreover, these approaches tend to reify the ethnographic genre, and consequently, its exoticism and its "subsumption" of theory (Thomas 1991a). Thus, the literary turn in anthropology can be interpreted as part of a "process of domestication" of the crises of the 1970s in which the attempts to change power relationships are substituted by the reading of hegemonic texts (Stocking 1991a: 4).

However, if we resist the temptation to read from the text outwards (or "reading-back" into history – cf. Boon 1989), and, instead, read the history of its production *into* the text, the analysis of the literary means of producing ethnography remains important. Marie Louise Pratt has pointed out the continuities between academic ethnography and the "manners and customs" genres that preceded it (1985). She argues that the ethnographic authority claimed in the academic sphere (Pratt 1986; see also Clifford 1983a, Rosaldo 1986) is continuous with its non-academic predecessors. Without engaging

in historical research proper, Johannes Fabian has come to similar conclusions (1983). He shows how important the manipulation of temporal dimensions has been for the construction of the objects of anthropology, both theoretically and ethnographically. In particular, he found that ethnography, by rhetorically denying the contact between anthropologists and informants (their "coevalness"), has an inbuilt tendency to ignore its historical context – a conclusion confirmed by Pratt (1985: 121; see also Thomas 1991a; 1991b: 3). Most important for our present purposes is that he locates the emergence of the ethnographic genre in the premodern shift from sacred to secular time and the transformation of European practices of *travel* (1983: 2–11).

Justin Stagl, in a series of papers crucial for understanding the history of anthropology (1974, 1980, 1990), has shown that from the sixteenth century onwards, a discourse on travel took shape in Europe. It drew upon earlier genres, directions for pilgrims in particular (1990: 317), but adapted these to changing conceptions of time and space. The "incorporating" cosmology characteristic of crusade, pilgrimage and mission, which was essentially directed inwards at a centre (Rome, Jerusalem), gave way to a "distancing" cosmology of exploration which started out from the now and here to discover the then and there (Fabian 1983: 27). We cannot do justice to the full range of historical possibilities suggested by Stagl, but will restrict ourselves to two elements of the history of anthropology which are crucial for our argument: the importance of the technology of writing, and the close link to European state-formation.[9]

Stagl shows how the *ars apodemica*, the art of travel, transformed "implicit cultural patterns of travelling" presented orally or in handwriting into a "formally codified" manual disseminated in print to the reading public (1990: 319). Under the influence of the philosopher Petrus Ramus (1515–72) and his "natural method" of the organization of all knowledge, the *ars apodemica* developed an encyclopedic manual of travel (1990: 303), a paradigm for later, more strictly anthropological manuals such as Degérando's *Considérations sur les méthodes à suivre dans l'observation des peuples sauvages* (1800; Moore 1969) and the Royal Anthropological Institute's *Notes and Queries on Anthropology* (1874). As Fabian argues, the Ramist method of storing, reproducing and disseminating knowledge acquired, through the technology of printing, general acceptance of a conception of knowledge in visual terms, a "diagrammatic reduction of the contents of thought" (1983: 116). These manuals were characteristic of a period in European history when knowledge of others was acquired "on the road", during travel, and provided a classification of knowledge that made it transferable and exchangeable from one context to the other, just as commodities are made exchangeable on the market.[10] Interestingly, Stagl remarks upon the fact that the manuals tended to enumerate the "singular phenomena" to be observed, creating reports "far removed from the

original experience of the traveller" (1990: 322) – a "denial of coevalness" which was carried over into ethnography (Fabian 1983). As we shall see, the term "ethnography" emerged in the context of this organization of knowledge.

The *ars apodemica* was also intimately linked to processes of state-formation in early modern Europe. Each manual included descriptions of the main nations of Europe, indicating "the close link between the *ars apodemica* cosmographies and the descriptions of polities" (Stagl 1990: 319). In the seventeenth century, the *ars apodemica* lost much of its former goal of improving the traveller's personality (institutionalized in the "Grand Tour" of young European noblemen) and concentrated more exclusively on the gathering of knowledge – "a transition to the methodology of expeditions" (1990: 324). This shift "from the centre to the periphery" resulted in a number of specializations: The instructions for copying inscriptions and using libraries and collections developed into an auxiliary discipline of history; the collection and conservation of minerals, fossils, plants and animals became important for "natural history", one of Linnaeus' students drawing up a *Instructio Peregrinatoris* (1759); and the questionnaires, basic to the art of travel from its beginning, were systematically applied by academicians like Robert Boyle to guide the collection and verification of knowledge by travellers (Stagl 1990: 324).

Specialization also resulted in the giving of very specific political instructions: the prince who financed a traveller often added a secret set of instructions, connected with the commercial and political aims of the voyage, to the official ones (1990: 325). The relationship with the state is also evident from the resurgence, in Göttingen, of the by then "moribund" *ars apodemica* in the second half of the eighteenth century, in a movement that gained a European reputation through scholars like August Ludwig Schlözer and Johann Christoph Gatterer (Stagl 1974: 73–91; 1990: 327). The art of travel became associated with the discipline of *Statistik*, destined to educate capable servants of the state – a concept later appropriated by those who only wanted to gather quantitative knowledge (1980: 375). It is in this context that the concept of *Ethnographie* was first mentioned as early as 1771 (Vermeulen 1992: 6). In the 1780s it acquired common usage among German scholars (Stagl 1974: 79–80; see also Fischer 1970).

The *Oxford English Dictionary* puts the date of the first mention of the term "ethnography" in the English language at 1834, in a source which states that "the term ethnography (nation-description) is sometimes used by German writers in the sense which we have given to anthropography".[11] According to the *Grand Robert*, the French term *ethnographie* was first used in 1819, when the Napoleonic wars, which prevented the further development of the expeditions and methodologies of Bougainville, Lapérouse and Dégerando, had come to an end (Stagl 1990: 326).[12] Restricting

ourselves to the English use of the term, we see that according to the OED, the complex of terms (ethnography, ethnographic(al), ethnology) appears rather late, in the 1830s and early 1840s. Its meanings are negotiated until the terms are defined in the *Encyclopedia Brittanica* in 1878 as follows: "Ethnography embraces the descriptive details, and ethnology the rational exposition of the human aggregates and organizations".[13]

In Britain, too, the words crop up in a context which ties "ethnography" firmly to the practice of travel – expeditions in particular – and the taxonomic organization of knowledge derived from Ramism and summarized by the term "natural history" (cf. Fabian 1983: 8; Foucault 1970: 125 ff.). A number of expeditions had already been sent into West Africa since the late eighteenth century, but the initiative lost momentum and merged with the Royal Geographical Association in 1831 (Curtin 1964: 17, 151; Voget 1975: 105). Shortly afterwards, T. F. Buxton formed a House Committee for the protection of aborigenes, for which a professor of anatomy, Thomas Hodgkin, acted as informal advisor. Hodgkin and his friend and colleague J. C. Prichard founded the Aborigenes Protection Society in 1837, to save indigenous peoples from possible extinction and study them at the same time. Meanwhile, Buxton had given the impetus towards the Niger Expedition, which combined the fight against the slave-trade, the promotion of African commerce and industry, and missionizing, with observation and exploration (1964: 298–303). From this context of merged humanitarian, commercial and scientific concerns, the first ethnological associations began to emerge. On the suggestion of Hodgkin, W. Edwards founded the *Société Ethnologique* in Paris in 1839. A lecture by Prichard led the British Association for the Advancement of Science to commission three medical doctors (Hodgkin, Prichard, and Richard Owen) to draw up a questionnaire for the study of native races threatened by extinction, which they did on the basis of a model provided by Edwards (Curtin 1964: 330–332). This questionnaire became the basis of the 1874 *Notes and Queries* (Voget 1975: 105). In 1843, Prichard published his *Natural History of Man*, which he called an "ethnographic outline", and in which he defined ethnology as "the history of nations". In the same year, Hodgkin and Prichard decided, for organizational reasons, to meet separately from the APS as the Ethnological Society of London, which became, after a troubled history of debates, separations and a merger between monogenists and polygenists, the Anthropological Institute in 1871 (Curtin 1964: 331; Reining 1962; Stocking 1971; 1987).[14] The connection with the state is again evident from the fact that Prichard, among others, drew up the "ethnology" section of the Admiralty's questionnaire in 1849 (Curtin 1964: 334).

Of course, during the second half of the nineteenth century, scientific racism and the debates between monogenists and polygenists had changed

the intellectual orientation of ethnologists and anthropologists to such an extent that taxonomic "history" was now replaced by a "naturalization of time" in terms of evolution (Fabian 1983: 11ff.) It should be pointed out, however, that Victorian anthropology was still characterized by a method of gathering knowledge "on the road", by the *travellers* that provided Tylor and Frazer, among others, with their data (Stocking 1987: 78–102). Ethnography continued to be understood as the collection of "manners and customs", an activity for which the current questionnaires provided the model, even when the taxonomy of the questionnaire had now been transposed to an evolutionary taxonomy of "stages" in the development of mankind. Still, ethnographic knowledge took the form of bits and pieces of knowledge that, by being classified in a questionnaire, could be transferred to another realm of thought.

This situation changed when imperial domination reached the stage where administrators, missionaries and others could start to "settle in". Its consequences for the history of ethnography cannot be fully spelled out here (but see the elaboration of the following thesis). A few important aspects, however, should be noted in the context of this thesis. The colonial situation may partly account for differences in the process of professionalization of ethnography. We ought to consider, for instance, the influence on Malinowski's "invention" of modern fieldwork of the fact that he, because of the lack of cooperation of the missionary Savile and the suspicion of local authorities during the First World War, was forced to rely much more on his own devices than was common at the time. The introduction to *Argonauts* can be read as a charter for a certain form of fieldwork – participant observation – which could be executed by the professional anthropologist on his own. With the Malinowskian "revolution" in ethnography, the questionnaire became obsolete. Not only did *Argonauts* successfully propagate a change in the ethnographic genre, it was also the culmination of a change in the conceptions of research – initiated by, among others, Haddon and Rivers – which was now conceived of as a methodology that, in contrast with the questionnaire, could not be easily mastered by laymen. Within Anglo-Saxon anthropology, questionnaires were gradually replaced by courses in methodology; the last edition of the *Notes and Queries* (1951), composed by professional British anthropologists, had the format of an introduction to anthropology rather than a questionnaire. As is often the case, the professionalization of ethnographic practice was achieved by the exclusion of other ethnographic methods and genres (like the questionnaire or the glossary – see Amin, this issue), and possible rival ethnographers like missionaries and administrators (Pels 1990, 1991; Thomas 1989: 69 ff.). Thus, ethnographic knowledge, constructed on the basis of an extended period of fieldwork by a trained anthropologist,

constituted a claim to authority that enhanced the anthropologists' monopoly of this kind of knowledge. Fieldwork became the hegemonic form of ethnography for most anthropologists.

Yet, trajectories of professionalization were not always the same: in France, for instance, the influence of administrators like Delafosse, missionaries like Leenhardt and the development of Griaule's work from expeditionary to initiatory fieldwork suggest extra-academic sources for the French emphasis on a "documentary" ethnography (Clifford 1982: 138–141; 1983b). Ethnographic questionnaires were common in France until well after World War II (e.g. Mauss 1967). To this day there is, comparatively speaking, more room in France for lay ethnographers in scientific journals and forums. Boas introduced a similar emphasis on the collection of documents by laymen – in his case, native American informants – to American anthropology, which was clearly related to his historical orientation brought from Germany (see Stocking 1974: 85–86), even though Boas' pupils later tended to embrace British-style fieldwork. Our hypothesis is that European "continental" traditions of scholarship – including those brought to the USA – were largely overwhelmed by the British domination of ethnographic discourse, but again, more historical research is needed here.

Such an emphasis on ethnographic traditions may well counter the overemphasis on the present "experimental moment" in anthropology (Marcus and Fischer 1986). It should not come as a surprise that some recent attempts at ethnographic experimentation take their cue from national traditions that resisted British hegemony in ethnography. James Clifford's essay on Marcel Griaule is revealing in this sense, because Griaule's example shows how anthropology could be characterized by a continuous process of experimentation with ethnographic forms (1983b). Moreover, Malinowski's *Argonauts* was clearly a literary experiment (Thornton 1985); other examples should include Evans-Pritchard's *The Nuer* and Bateson's *Naven* (cf. Kuper 1983: 74; Marcus 1985), Bateson and Mead's *Balinese Character* (1942: xi) or Condominas' ethnography of the Mnong Gar in diary form (1957). This does not exclude the existence of a hegemonic form of ethnographic authority; on the contrary, this hegemony may account for the fact that certain ethnographic experiments, such as Audrey Richards' *Chisungu* (1954) or Zora Neal Hurston's work (Gordon 1990) were largely ignored in established, academic anthropology.[15]

Thus, the reversal of priority from academic anthropology to ethnographic practice leads us to question the common assumption that academic anthropology, and fieldwork methodology in particular, is the *telos* to which all ethnography strives. The rest of our argument, therefore, concentrates more exclusively on the way in which a study of the colonial context of ethnography frees it from academic prejudices.

Third Thesis: Ethnographic holism, cultural relativism and functionalism are as much products of colonial practice as they are theoretical innovations of academic anthropology.

When colonial domination necessitated the "settling in" by administrators and missionaries, the attitudes characteristic of ethnography "on the road" changed and eventually led to the trajectories of professionalization sketched above. The relevance of ethnography for the development of both colonial society and academic anthropology during this stage of "settling in" is apparent from the way ethnographic holism developed from the colonial situation. Moreover, recent studies suggest that there were local "ethnographic traditions" in which the academic anthropologist participated on arrival in the field.

In this context, the historical validity of literary approaches becomes again evident. James Clifford has argued that all fieldworkers have worked on the assumption that social wholes can be understood and described by concentrating on certain significant elements of society: key institutions like the Kula, Azande witchcraft, or initiation, or methodological constructs like genealogy or social structure. Such synecdoches are necessary for the representation of "relatively short-term professional fieldwork" (Clifford 1983b: 129–130); without the idea that a specific part of society can stand for the whole, fieldwork would be questionable because "social wholes cannot be directly perceived by a single human observer" (Thornton 1988: 288). Clifford argues that this emphasis on social wholes is a reflection of a nihilistic world in which, from Yeats to Achebe, "things fall apart" (1983b: 130). However, there is reason to suppose that ethnographic holism was not merely fostered by the European's yearning for an integrated society, but by the practical demands of the colonial situation.

French missionaries in pre-colonial Indochina professed a proto-holism in their ethnographic writing early on. Forced by a precarious existence among the Bahnar "savages" to accomodate to their way of life, a situation reminiscent of fieldwork, Father Jean Guerlach realized as early as 1887 that the different spheres of life were intimately tied together:

> Among all the primitive peoples, the religious system and the political system, the cult ceremonies and the domestic habits are so intimately interrelated, that, in order to understand their history and national organization, the knowledge of the traditions and of the religious doctrines is indispensable. (Guerlach 1887: 441)

This ethnographic holism *avant la lettre* was not formalized in a theoretical statement on the organization of society, perhaps because his "only goal was to know well the religious beliefs of the savages, in order to better demonstrate [...] the absurdity of the superstitions" (1887: 441). For this

practical reason, his description focussed on religion, and on the role of the sorcerers in particular, who – as "accomplices of the devil" – were the main obstacles to conversion. But Guerlach did not take religion as a synecdoche for understanding the whole of Bahnar society. Some of his successors, however, with more theoretical feedback and in a changed colonial situation, tended to use "religion" as the key to all social affairs – a rhetoric announced by Guerlach in the quote above. The synecdoche "religion" was common to many missionaries, especially in the latter half of this century, but in other cases missionary practice produced less holist, "selective" ethnographies (see Pels, this issue).

In *Victorian Anthropology*, George Stocking pointed out that the ethnographies by early missionaries and administrators foreshadowed "a more intensive ethnographic style whose data would in fact sustain a more holistic interpretation" (1987: 104). The synecdoche that was most probably dominant in understanding social wholes in the political field was what Europeans perceived as "customary law". The development of Indirect Rule in Africa, for instance, shows that Frederick Lugard, whose primary worries were the conquest and control of Northern Nigeria, did not formulate a theory of local political institutions. When instituting Indirect Rule after 1898, he was mainly interested in delegating his own authority to the Fulani chiefs, not in incorporating theirs (Pels 1993a: 30). His former Chief Secretary, Donald Cameron, held a different view when he was Governor of Nigeria in 1934:

> [Indirect Administration], based on several principles, is designed to adapt for the purposes of local government the *tribal institutions* which the native peoples have evolved for themselves, so that the latter may develop in a *constitutional* manner from their own past, guided and restrained by the sanctions and traditions which they have inherited... (Cameron, in Kirk-Greene 1965: 193; our emphases)

This conception of Indirect Rule was developed during Cameron's tour in Tanganyika, where he promoted rule through "hereditary tribal chiefs". "Tribal institutions" were the customs of a tribe, and the customs of a tribe were the laws of hereditary succession. Thus, Cameron and his officers could shift conceptually from "institution" to "constitution", a practice reflecting the legalist interests of administration. For administrative ethnography in Tanganyika between 1925 and 1931, research implied looking for customary laws (Pels 1993a: 34–40). The customary law of hereditary succession was, for Cameron, needed to guarantee controlled political evolution in Tanganyika; if the African was not kept in touch which his own customary ways, he would become "detribalized", "leaderless and uncontrolled", in short, a "bad imitation" of the European.

It is important to realize that the ethnographic holism of colonial discourse – whether of a legalist, or any other kind – is often characterized by both proto-relativism and proto-functionalism, even if it sometimes was a "functionalism of the abhorrent" (Stocking 1987: 104). By "proto" we mean that such notions existed in an embryonic, often implicit form. Ethnographic holism was proto-relativist, in the sense that it called for a recognition of "the values set up by every society to guide its own life" (cf. Herskovits 1973: 76–77), even though it did not, as in Cameron's case, deny a complementary idea of political evolution. It was proto-functionalist to the extent that it premised the good functioning of social wholes under colonial rule on the integrity of a collective system of embodied legal strictures, often caught in the term "tribe" (or, in India, "caste"; Dirks 1992). This point was, indeed, made by Malinowski, when he said that Lugard's Indirect Rule (at a time when the latter had moved more closely to Cameron's perspectives) was "a complete Surrender to the Functional Point of View" (quoted in Cell 1989: 483). The so-called "legalist" character of functionalist anthropology, as noted by David Goddard (1972), and the influence of the Sudan Administration's legalist perspective on Evans-Pritchard's *The Nuer*, traced by Douglas Johnson (1982), suggest that there were even tighter relationships between colonial ethnographies and academic anthropology than just the general necessity of a holistic perspective.

This proto-relativism and proto-functionalism is well brought out in the career of the French administrator *cum* ethnographer Léopold Sabatier, from 1913 to 1926 head of Darlac province in Vietnam's Central Highlands. Sabatier was the first to appreciate the Rhadé group in Darlac as a well-functioning society and a valuable culture, basing his administration of Darlac on the composition of a *coutumier* — a record of customary law. This (re)construction of Montagnard tradition in the format of a *coutumier*, storing and modifying the "memory" of Montagnard sages in writing, became a model to be followed by both administrators and professional anthropologists in the French tradition up to the 1960s (Salemink 1991: 262). As in the case of Cameron and his administration, ethnography, embedded in an administrative practice, was a legalist act, meant to counter supposed detribalization processes. Aiming at a gradual development of the Rhadé group through a combination of paternalistic direct rule adapted to local culture, Sabatier resisted the gradual take-over of Darlac province by both ethnic Vietnamese and European settlers, supporting his resistance with a study of traditional Rhadé land tenure systems. In this context, the *coutumier* became a highly contested political document, because it staked a claim to tribal territory (Salemink 1991: 250). Sabatier's stubborn refusal to lease out concessions to European companies in tribal territory created a scandal

in colonial circles, but he was defended by metropolitan intellectuals like the ethnologist Lucien Lévy-Bruhl, co-founder of the Parisian *Institut d'Ethnologie* together with the former administrator Maurice Delafosse and Marcel Mauss. Lévy-Bruhl, who had gained fame as a functionalist ethnologist, was very much interested in Sabatier's Darlac, which he would like to preserve as a living laboratory of the "primitive mentality" in which he was interested. The two entertained a lively correspondence, from which Sabatier derived his arguments to preserve his Darlac as a human reserve on scientific grounds, while Lévy-Bruhl was interested in Sabatier's work as ethnographic source material to substantiate his arguments about the contextual rationality of the "primitive mind" and the functional organization of primitive societies. This example shows that administrative ethnography could create fertile ground for the relativist and functionalist theories of academic discourse.

This homology of colonial and academic discursive patterns is reinforced by the fact that an increasing number of studies show that there were local ethnographic traditions into which academic anthropologists fitted quite easily after arrival in the field. Fardon and others have pointed out that the "literary turn" in anthropology adopts an idea of ethnography as an encounter between a fieldworker and the "Other", and thereby ignores that most anthropologists work within "conventionalized regions of enquiry" and that "ethnographies are also reworked versions, inversions and revisions of previous accounts" (Fardon 1990: 22, 25). Yet, in the same volume David Parkin tries to account for a specific "East African" ethnography by focussing on academic institutions only, and in particular on the division of labour between the East African Institute of Social Research headed by Audrey Richards and the Rhodes-Livingstone Institute under Wilson, Gluckman and others (Parkin 1990: 187–190). He ignores that the title of Richards' collection on *East African Chiefs* (1959) cannot easily be understood without taking account of the history of Indirect Rule in Eastern Africa, with its predominant emphasis on "chiefs" ruling "tribes". Work in Uganda and Tanzania was very much determined by ethnographic insights shared by administrators and anthropologists and embodied by people like Philip Gulliver, John Beattie and Henry Fosbrooke, who became academic anthropologists after having served in the Tanganyikan administration. Fardon, too, seems to dismiss missionaries, administrators and explorers from the history of local ethnographic strategies (1990: 3).

In other words, even when a history of ethnographic traditions is considered, it tends to ignore what Georges Condominas – speaking from experience – called the *préterrain*, the local colonial milieu from which the academic ethnographer departed and to which he returned in times of

"surfeit of native" (the phrase is Malinowski's, 1922: 6; Condominas 1973: 9–10). The *préterrain* influenced many an academic's ethnography. Raymond Firth pointed out that the "informal, often covert, constraints" of colonial society on anthropology were, paradoxically, "largely a function of the positive assistance that the anthropologist receives".

> To hold that cooperation and constraint may go hand in hand cannot be novel ... to anthropologists familiar with the theory of reciprocity. When an anthropologist has had help in the field from a local administrator, an agricultural officer, a storekeeper, a plantation manager, a missionary, who has not only lent him equipment and given him hospitality but also discussed with him pressing problems frankly, with a mixture of hope and despair, the anthropologist may feel constrained to reply in kind. ... [T]hough the anthropologists tends not to share some of his host's major assumptions about the colonial situation, he is often led to examine these assumptions more carefully than otherwise he would... (Firth 1977: 146–147)

Acknowledging the often subconscious influence of the colonial *préterrain* is more important for the study of anthropology's relation to colonial society than accusations of collaboration or complicity on the part of the anthropologist. Firth exaggerates the extent to which anthropologists could "examine" colonial assumptions, but brings out well that their contact with colonizers necessarily led to the sharing of discursive patterns. It is in this perspective that we would interpret the fact that the subjects and viewpoints of Evans-Pritchard or the Rhodes-Livingstone scholars were often taken from their respective colonial administrations, as were the classifications of tribes and states in Northern Nigeria of present-day scholars (Sharpe 1986), the images of Bali of Bateson and Mead (Schulte Nordholt, in this issue), the professional academics' image of Vietnamese Montagnards (Salemink 1991) and academic views of the lineage systems of Uluguru (Pels 1992: 40). The missionary Thomas Cullen Young's support for the Reverend Yesaya Chibambo's history of the Ngoni of Nyasaland, who in turn exerted, as expert guide, a major influence on the research by the anthropologist Margaret Read, suggests the importance of the missionary *préterrain* for academic ethnography (Forster 1991 and this issue). It shows the crucial influence of the mission school's spread of literacy, a practice which not only produced many an anthropologist's fieldwork assistant, but also taught Africans to represent themselves in ethnographic form (as was noted early by Balandier, 1962: 91; see also theses four and five). It can also be argued, however, that the missionary *préterrain* was sometimes less conducive for academic and administrative ethnographic concerns (Pels, this issue).

Fourth Thesis: A historical study of the production and consumption of ethnography necessarily implies an analysis of the ways in which these were materially mediated.

We have already remarked that ethnography obscures the historical mediations on which its production is based: the contact or "coevalness" of ethnographer and informants (Fabian 1983; Pratt 1985; Thomas 1991b: 3). This situation is reflected in the way in which the concept of "ethnography" frequently subsumes fieldwork practice, and as often equated it with "participant observation". From Tylor to Malinowski, ethnography was taken to be a "classificatory science" (Malinowski 1911: 25; Tylor 1871, I: 7). Therefore, if "ethnography" is taken literally – as the classification in writing of "cultures", "races" or "nations" – one should be careful to distinguish it from fieldwork as a research practice (cf. Thomas 1991a: note 3).[16] Malinowski distinguished "ethnographic fieldwork" from other forms of research such as "archaeological" or "zoological fieldwork" (1922: 24). Nowadays, however, ethnography is often called a "method", which includes dealing with problems of access to the field, field relations and interviewing techniques (f.e. Hammersley and Atkinson 1983: ix and *passim*), activities which are not specific to ethnography. Terms like "ethnographic collections" or "ethnographic observation" obscure that neither collecting artefacts nor observing acts necessarily lead to the classification of an *ethnos*. Conversely, the common use of the pleonasm "ethnographic writing" (f.e. by Clifford, 1986: 14, Pratt, 1986: 35) raises the question what else ethnography can be but writing (cf. Fabian 1990: 757). There is a peculiar see-saw historical movement here: whereas Malinowski tried to subsume ethnography under the professional's claim to intensive fieldwork as a scientific method, now fieldwork seems to be subsumed under the practice of writing culture. This subsumption needs to be resisted in order to understand the production and consumption of ethnography.

Put in another way: it is necessary to shift attention from the nature of ethnographic representations to the work of representing (cf. Fabian 1990). Recent studies of ethnography still partake of a discourse of representation that moves within the boundaries of the "truth" and "falsity" of its representations and the way they reflect reality: Marcus and Fischer aim at a more "accurate view and confident knowledge of the world" (1986: 14–15), and James Clifford at "partial", "dialogical" truths which represent "negotiated realities" (Clifford 1986: 15). These "aesthetic responses" to a situation of imperial contest can, according to Said, better be classified as "anesthetics" (1989: 211), because they tend to ignore the historical transformations of knowledge that were necessary to create the impression that others' realities *can be* represented by ethnography (cf. Fabian 1983,

1990). We therefore stress that the content of an ethnographic text needs to be understood through an analysis of the historical context of its production: the fieldwork process, which is itself a symptom of the accessibility of others created by European colonialism.[17]

Also, before assuming that the content of the ethnographic text is an example of intellectual colonization of "others", one first has to analyze the ways in which it was consumed by different audiences, both within and outside of the colonial situation. This argument is not meant to deny the importance of the kind of analysis pioneered by Said (1978), but to argue for an elaboration of it. Like Said, we insist on the blurring of the distinction between pure and political knowledge (1978: 9), and we acknowledge his effort as a methodological precondition of ours (cf. 1978: 15–16). Said is concerned with demonstrating that Orientalist representations can be analyzed "*as representations*, not as 'natural' depictions of the Orient" (1978: 21). Our intention is to add an intermediary instance: the fact that any representation of "others" is a *historical* depiction. Neither its "naturalness" – that is, its pretense to reflect "the Orient as such" – nor its character as representation – which excludes "any such *real thing* as 'the Orient'" (1978: 21) – sufficiently characterize the historical processes in which these representations were put to use. While agreeing in principle with Said that anthropology and empire were never separated (Said 1989: 214), we feel that he connects them too crudely by not operationalizing "anthropology" in terms of ethnographies and "empire" in terms of local colonial situations.

Although Said is at pains to relate discursive patterns back to the imperial context from which they derive (1989: 211), the approach toward representations of the colonized through textual analysis only, often fails to capture the historical mediations through which these representations were produced. A number of recent studies, for example, seems to be based on the assumption that the study of hegemonic imagery is identical to the study of the production of hegemony (cf. Mitchell 1988, Mudimbe 1988). Studies of the colonial "invention of tradition" seem to take the "ideological" function of invented traditions for granted, but fail to ask whether and how, if "invention" only means "made up by the colonizer", the colonized shared this invention; or conversely, when the "tradition" was an invention co-authored by colonizers and colonized, for whom it was an invention (a new idea) and for whom a tradition (an existing practice), and again, why it was adopted.[18] We feel that a notion of the "microphysics" of colonial power (cf. Foucault 1979: 26) is necessary to adequately capture the material mediations through which representations of others were made to mold or modify colonial relationships.

Two examples from the construction of colonial power through ethnography will show how a microphysics of power mediated the impact of

ethnography: the influence of writing (as technology) and secondly, the importance of the construction of ethnographic occasions (see also Pels, this issue). Writing as technology mediates writing as text. We have already argued that writing, and printing in particular, resulted in a visual conception of knowledge. This reduction of the contents of thought contributed to the creation of the tabulated lists of the traveller's manuals and the absence of the observer from the information stored. The argument can be generalized beyond the historical impact of printing technology, to writing as such. Information gathered in writing divorces content from its context of utterance and therefore emphasizes the referential aspect of language and ignores the performative ones (Fabian 1990b: 1–20; Washabaugh 1979: 32). This could have the result, for example, that the Tanganyikan administrator, in his urge to find out who was the "true" representative of a local Luguru polity in terms of hereditary right to office, ignored the fact that Waluguru rarely contradict a superior in his presence and that they often show their disagreement by staying away or not answering. By limiting his questions to the truth and falsity of claims to office (the referential aspect), the administrator could hardly be expected to find out that there were no chiefs at all before the government appointed them. In Uluguru, this led to the replacement of transitory "big man" positions by rigid bureaucratic hierarchies (Pels, this issue). Similar processes accompanied the reification in writing of Vietnamese Montagnards' "customary law" and suggest parallels with, for instance, the *adatrecht* studies in the Dutch East Indies (Salemink 1991: 251; Schulte Nordholt, this issue).

Another important material mediation of ethnography was the (gendered) construction of the ethnographic occasion. Johannes Fabian (personal communication) has suggested that some of the best early colonial ethnography was produced "on the pillow", between European travellers and their native concubines. But the issue of male-female relationships and gender constructions in colonial ethnography is far broader than this. The *baraza* (council meeting) of the Tanganyikan administrator, normally used as the occasion at which he outlined his desires and directives to the Native Authorities appointed by government, was also used as an instrument for gathering ethnographic knowledge. The *baraza* consisted of those native leaders already appointed, ignoring the big men who lacked a government position, but more importantly, the leading women who wielded a considerable amount of power within traditional society (Pels 1993a: 52–53, 145–147). As similar processes have taken place among Vietnamese Montagnards (Salemink 1991: 254) and in Sri Lanka (Risseeuw 1988), we suggest that this misconstruing of – especially matrilineal – discourses on gender and politics was widespread under colonial rule. Such distortions of local political process were not simply the result of consciously held sexist beliefs, but of the material practices – the ethnographic occasions –

in which these ideas were embodied. Colonial administrators hardly reflected consciously on the construction of these ethnographic occasions, thus creating "empirical proof" for the assumption that women were politically insignificant.[19]

It is important to realize that the two material mediations – writing and the construction of ethnographic occasions – often worked to reinforce one another. Bureaucracy was predicated upon the transmission of knowledge in writing. In the colonial situation, this often resulted in the uncritical reproduction of knowledge available in writing by subsequent administrators. This lent an inordinate weight to the original ethnographic occasion in which the knowledge thus passed on had been formulated (for an example from the Balinese administration, see Schulte Nordholt, in this issue). Tanganyikan administrators often only engaged in ethnography to check the knowledge gathered in writing by their predecessor, reproducing the same ethnographic occasion (the *baraza*) and consequently reproducing the same formulations of "traditional" customs (Pels, this issue). Needless to say, the validity of the synecdoche of "customary law" was, in that context, never questioned.

Fifth Thesis. Ethnography was mostly relevant for colonial society, not in terms of the truth or falsity of its representations, but because it instituted representation as such (both in the literary and political sense).

A common assumption of discourses on ethnography is that better knowledge of others leads to more legitimate control over them, an argument common to those who discussed the relevance of anthropology for colonial government (cf. Fortes and Evans-Pritchard 1940, Leroi-Gourhan and Poirier 1953, Malinowski 1929, Perham 1934). This can be contrasted with the view that representations of others produced from a position of power are inevitably ideological inventions of tradition (f.e. Graham 1976, Ranger 1983). The two positions show opposed attitudes towards the relationship between power and knowledge: the first argues that true knowledge of others' routines may ensure their participation in the colonial process on their own terms and thus lead to a decrease of power inequalities; the second, that visions of others constructed from a position of power are inevitably false and thus ensure the continuation of power inequalities.

We suggest that the focus on the truth and falsity of colonial representations obscures the importance of the institution of political representation as such, and that this institution of political representation in former colonies was often achieved through ethnography. As we argued in the elaboration of the previous thesis, the material mediations of the process of representation were often more important than the contents of

the representations themselves. It is worthwhile to refer to Marie Louise Pratt's view that "ethnographic texts are means by which Europeans represent to themselves their (usually subjugated) others" (Pratt 1992: 7). The catch lies in the clause "usually subjugated", because Pratt also shows that there are "auto-ethnographies": "instances in which colonized subjects undertake to represent themselves in ways that *engage with* the colonizer's own terms" (Pratt 1992: 7). While "auto-ethnography" implies a kind of self-determination through representation, it also presupposes a major degree of adaptation to the practices of colonial power.

In this context, it is crucial to acknowledge the administrative and military interest in the management of *territory*. The importance of ethnographic mapping is exemplified by the classical statement by Colonel (later Marshall) Galliéni on the connection between military control and an explicit ethnic policy:

> It is the study of the races who inhabit a region which determines the political organization to be imposed and the means to be employed for its pacification. An officer who succeeds in drawing a sufficiently exact, ethnographic map of the territory he commands, has almost reached its complete pacification, soon followed by the organization which suits him best.[...] Every agglomeration of individuals – a race, a people, a tribe or a family – represents a sum of shared or opposed interests. If there are habits and customs to respect, there are also rivalries which we have to untangle and utilize to our profit, by opposing the ones to the others, and by basing ourselves on the ones in order to defeat the others. (Galliéni 1941: 217; cf. Salemink 1991: 246)

This shows that one of the initial moves in colonial discourse is the ethnographic one: the fixing of an ethnic identity to a specific territory – the "geographical disposition" of ethnography (Said 1989: 218; Byrnes, this issue; Noyes, this issue). Definitions of ethnic identities and their corresponding territories have been carried over from the colonial situation in which they were formulated into academic spheres (cf. Pinney 1990; Sharpe 1986). The emphasis on territory of functionalist anthropologists (f.e. Fortes and Evans-Pritchard 1940: 6, 10) may well be related to discourses informed by administrative interests. In any case, it is clear that a certain management of spatial categories was an important tactic of colonial discourse (see Noyes 1992 and this issue; Byrnes, this issue).

This may be seen in terms of what Condominas called *tribalization* (1966: 168): a process by which fluid entities, with no fixed boundaries distinguishing them, became ethnic groups. The French colonial administration created "tribes" on the basis of an "ethno-linguistic" classification, despite the common opinion in early ethnographic sources that hardly any supra-village organizations existed in the Vietnamese Central Highlands at

the time of French penetration. Nowadays, the "tribes" are not only ethnic minorities in a nation-state, but take colonial classifications for granted in their own political organizations and in their own 'histories' of the Central Highlands (Salemink 1991: 272–276). This process of tribalization has been documented for colonial Tanganyika, too (Iliffe 1979: 318–341).

These uses of ethnographic classifications show the extent to which they were limiting devices in a double sense. Contrary to the expectations of those who defended the application of anthropology in the colonies because it would facilitate traffic between colonizers and colonized, ethnographic classifications functioned initially as a way to immobilize groups of people by drawing boundaries around "their territory" – a process against which some groups, like the Nuer, rebelled (Johnson 1979). Moreover, we have already seen that the selection of a "chief" in colonial Tanganyika under Donald Cameron depended on a theory about "tribes" which declared the chief to be the "representative" of "his" people. In Uluguru, the ethnographic occasions on which this kind of knowledge was gathered reduced the normally fairly democratic meetings of big men and women to rituals of bureaucratic command in which only the administrator and so-called "representative" chiefs, subchiefs and headmen, appointed by the British, took part.[20] Thus, the primary effect of ethnographic representation was to immobilize Waluguru and reduce their participation in colonial politics (Pels, this issue).

Several other staple concepts of ethnography show this interest in control through representation. We have already remarked upon the ubiquity of the synecdoche of "customary law" and the way in which it reflected the legalist interests of administration. Jan Breman (1987) and Jeremy Kemp (1987) have argued that the notion of the village community was an administrative construction of colonial rule in Asia, while Salemink (n.d.1) shows that recent debates on the motivation of peasant revolts (moral versus political economy) were closely connected to political practices for which the concept of "village" was deemed crucial. Feminist anthropology, in particular, has shown how problematic these holist notions are, by arguing that *every* group or collectivity can be divided by antagonistic male and female interests. Ethnographic holism is not only to be criticized on rhetorical and methodological grounds (Thornton 1988), but also politically.

Therefore, a critical view of "auto-ethnographic" practices is necessary. Tempting as it may seem to view auto-ethnographic expressions as approaching the ideal of a truly "democratic" representation, the fact that it is an ethnographic representation makes it as politically problematic as the "dialogic" ethnography championed by many anthropologists of the literary turn. More often than not, "auto-ethnographers" objectify the self

in terms borrowed from the colonial ethnographic imagination, reinventing tradition on the way. In different ways, Allan Hanson (1989) and Toon van Meyl (1990) show how auto-ethnographic practices by Maori anthropologists tend to convey the view and express the interests of local elites, just as the Reverend Yesaya Chibambo's history of the Ngoni legitimated the rule of the Ngoni over other ethnic groups in Nyasaland by arguing that theirs was a form of "Indirect Rule" akin to that of the British colonizers (Forster 1991 and this issue). Although we do not deny that "auto-ethnographies" can be genuine channels for political protest, the fact that they are representations of "ethnic groups" makes us wary of the legitimacy and the political consequences of the claims put forward.

Who needs ethnography?

We hope to have demonstrated that ethnography is, in many cases, both a product *of* and a product *for* colonial rule and that as such it mediates between colonialism and anthropology. However, this does not (yet) amount to a balanced assessment of its global significance. We think that there is ample reason to be suspicious of contemporary claims to social scientific authority on the basis of ethnography, but do not wish to dispense with ethnographic practice altogether. If we have questioned some assumptions long cherished by anthropologists and ended with holding a few of them upside down, this does not amount to a critical assessment of ethnography *in toto*. In line with the first thesis, that academic anthropology is not understood by studying academic anthropology only, we might say that one cannot understand ethnography by studying ethnography only. Nicholas Thomas has argued that there are reasons to be "against ethnography" (cf. 1991a). Thus, anthropologists may have to ask: "Who needs ethnography?", and consequently: "Do we?". Whatever the answers, the first question should be the *Leitmotiv* for a historian of anthropology.

Therefore, an inductive and historicizing approach is still needed, the more so because ethnography is located in widely diverging patterns of interest. We have located ethnography to a large extent in the administrator's desire to produce static, localized ethnic identities and to identify the central institutions or persons (like "chiefs") on which to apply the state's measures. This seems plausible, because of the long-standing historical link of ethnographic practice with nation-building and state-formation. This link emerged in a process of transformation of travel, a shift from the incorporating cosmologies of crusade, pilgrimage and mission to the outgoing orientations of explorers and *Statistiker*. Thus, our analysis implies that missionary ethnography may take up a different position, because mission is a different kind of travel: a practice of incorporation of

"pagans" into the church. The example of Father Guerlach (see p. 11) shows that while missionaries shared in the generic conventions of ethnography – holism – their interests were often also to oppose certain "customs" or "superstitions", creating a tension with ethnographic holism. Before we fall back upon anthropological stereotypes of missionaries who only come to "teach" (while anthropologists come to "learn"), the different orientations within missionary ethnography should be studied. In one case at least, missionary ethnographies were not as "ethnographic" as, for instance, administrative ones (Pels, this issue). In another, a missionary was willing to relinquish ethnographic authority in favour of his informants (Forster, this issue).

Not all colonizers were interested in ethnography. Both in Vietnam and in Tanganyika, there was considerable tension between missionaries, settlers and administrators. It seems as if ethnography belonged more to the political and religious or intellectual sectors of colonial society: missionary, administrator and anthropologist engaged in it while settlers did not produce much of the kind. One may ask the question why strictly economic enterprise was not fertile ground for ethnography while politics and religion were.[21] Usually, settlers were not interested – better: had no interest – in producing images of collective otherness.[22] The dispute between Donald Cameron's administration and the settler-dominated polity of Kenya showed how little the settlers were interested in tribal identities – after all, labour power does not come in tribal groups but in manageable individuals. In colonial Vietnam, settlers and aspiring planters were convinced that the Montagnards would disappear from the face of the earth, as predicted by the social-Darwinist formula of survival of the fittest; they were also aware that any serious ethnographic interest in indigenous groups could bring to light rival claims to the fertile soils of the Central Highlands (Salemink 1991). Nowadays, similar non-debates take place over the issue of logging and the disruption of indigenous groups in Amazonia and Southeast Asia. Conservationists extoll the environmental wisdom of traditional cultures, whereas logging industries and states justify their practices by blaming the "backward" shifting cultivators for destroying the forests. It is obvious that the first group would be inclined to engage in ethnography, and the latter group not, for the simple reason that the sheer *fact* of ethnography is contrary to the interests represented by loggers, in that it acknowledges the existence of a population with interests opposed to theirs.

In contrast, traders may have had an interest in ethnographic description, if only to try to improve upon their terms of trade. We know of no study of trader's literature, despite the fact that a considerable number of books exist. One of these, the autobiography of a British trader on the West-African coast (Taylor 1939), suggests that traders' knowledge of other

cultures was more fragmented and not held together by a holist concept: magic becomes a topic of study for the trader once it promises an exchange of ivory; love-potions become a topic for study when the trader discusses his native concubines; poison ordeals when he is confronted with a dispute between his agent and other Africans. One should acknowledge here that traders – unlike administrators and missionaries, many of whom had university-level training – may not have had the desire nor the publishing opportunities that motivated them to write at all.

In recent times, we can see how classical ethnographic descriptions are integrated in the "tourist gaze".[23] They serve as base material for new travelogues, and, more banal, for the tourist guides that promote visits to "exotic cultures" and promise "participant observation" in "traditional" ceremonies in "unspoilt" locations (see Volkman 1990). While ethnographic images are appropriated by the tourist industry, (auto-) ethnographic descriptions also play a role in the resistance of minorities to state power, whether they be native North or Latin Americans, Kikuyu or Luo against the Kenyan state, or "tribals" in India. These diverging patterns of interest need to be studied before an assessment of ethnography's global impact can be made, an assessment that leads us far beyond the "colonial" ethnographies to which we restricted ourselves in this essay.

This outline of the possible scope of analysis shows that the essays presented in this issue cannot but be isolated examples of the kind of reorientation to which a focus on colonial ethnographies can contribute. Five essays deal with Asia, three with Africa, and two with Oceania, a coverage that in itself shows major gaps; one only has to think of the internal colonialism that characterized the growth of nation-states in the Americas and of the Soviet empire to see the histories of anthropology that we have not been able to consider. The importance of the Indian *Raj* for the development of varying ethnographic genres and of academic anthropology is highlighted by two contributions. Shahid Amin puts Crooke's glossary of North India in the context of administrative attempts to monopolize ethnographic and political representation and their consequences for the image and self-image of Indian peasant society. Andrew West's outline of the Naga tribes' ethnographic tradition is an interesting addition to the recent work on Indian colonial ethnography (see, among others, Bayly 1994, Dirks 1992, Pinney 1990). An assessment of the impact of Indian ethnography on British academic anthropology is long overdue, and West provides us with a first example of an ethnographic tradition that developed in the field but gained a remarkable continuity in academic circles, and Cambridge University in particular. The importance of the Naga ethnographic tradition is disproportional to the importance of the Naga Hills for British colonial rule. West argues that it was precisely the peripheral status of the Naga Hills that fostered a special ethnographic interest on the part of colonial officers.

Writing about another part of Asia, Henk Schulte Nordholt shows how the academic ethnography of Bali, the work of Mead and Bateson in particular, was a continuation of the ethnographic tradition that developed when bureaucratic expediency seduced administrators to fall back upon a limited number of ethnographic exemplars written by their predecessors. Oscar Salemink's essay on the continuity between colonial ethnography and present-day accounts of a Vietnamese millenarian movement is yet another example of the processes that link colonial and postcolonial patterns of interests in ethnographic representation. Patrick Wolfe emphasizes other discursive continuities between colonial ethnography and contemporary academic practice. Wolfe's tracing of interconnections between evolutionist assumptions of "virgin birth" and Australian eugenetic policies toward Aboriginals is an example of the type of analysis that is needed to dispel the facile interpretation of nineteenth-century academic anthropology as an "armchair" activity which is only remotely in touch with colonial practice.

Giselle Byrnes provides a thought-provoking elaboration of the thesis that ethnographic production needs to be linked to its material mediations, by showing how the cartographic strategies employed by Edward Shortland for describing his New Zealand explorations were based on the tactical negotiations with his interlocutors during his travels. John Noyes also focuses on cartography by showing the continuities between the colonial mapping of German South West Africa and academic practices. Both papers give far more subtle interpretations of the geographic disposition of ethnography than we have been able to provide in this introduction. A related instrument for classifying and territorializing populations is the census. Vicente Rafael exposes the categorizations (gender, ethnicity, class etc.) in the 1903 census in the Philippines under American rule as tools for surveillance, and contrasts these with contemporary Filipino nationalist theatre, that tries to resist and escape from the colonial classifications advanced by the census. All three papers, therefore, address the use of non-narrative, diagrammatic forms of representation in ethnography.

Like this introduction, the essays deal predominantly with administrative ethnography. Given the nature of the debate about anthropology and colonialism, this may seem evident, but it should not obscure the fact that colonial rule comes in many more guises than just administration. Peter Forster, in the one essay exclusively devoted to missionary ethnography, shows the influence that missionary ethnography may have had, by focussing on the way in which Cullen Young's work introduced a "functionalist" tendency in African nationalist thought through his promotion of African authors. He argues that common anthropological stereotypes of missionary work are in need of revision and that the political impact of missionary ethnography and the auto-ethnographies

they promoted may be a major factor in the development of colonial and postcolonial identities. A similar demystification of missionary work is the result of the essay by Peter Pels, whose comparison between administrative and missionary ethnographies of Uluguru (Tanzania) is intended to counter the naive way in which colonial power and colonial prejudice are linked in many discussions of colonial discourse.

In our view, critical discussions of the way in which ethnographies have been produced and consumed in specific historical contexts should have an impact on anthropologists' self-awareness. One of the consequences which we have only addressed implicitly here is the possibility of assigning to ethnographic representations a less prominent place in anthropological discourse than before. To what extent do we, as anthropologists, need ethnography? A rival discourse – historiography – was important for the way in which we formulated our arguments and joins the present project with others that also address the often crude handling of history by anthropologists (e.g. Thomas 1989). That does not imply that historiography itself is not amenable to a critical historicizing discussion. Recent critiques of Said and Swedenburg show the extent to which historical contextualization itself can be used in attempts to blacklist one's opponent.[24] Historical analysis, therefore, is no easy way out. However, we hope to have shown that anthropology may have to reconsider its historical self-awareness. If that leads to some uncomfortable conclusions about the relationship between ethnography, colonialism and academic practice and to a debunking of exaggerated "Big Men" histories of anthropology, we feel these are long overdue.

Notes

1. This paper served as a pilot study for a seminar on "Colonial Ethnographies: Writing, Cultures and Historical Contexts", held at the Amsterdam School for Social Research, University of Amsterdam, in June, 1993, which was made possible by the generous support of the Amsterdam School for Social Research, the Royal Netherlands Academy for Arts and Sciences (KNAW), the Fund for Development Cooperation (FUOS) of the University of Amsterdam, the Directorate-General for International Cooperation (DGIS) of the Dutch Ministry of Foreign Affairs, the Society for Scientific Research in the Tropics (Treub-Maatschappij), and the Amsterdamse Universiteitsvereniging. The Fund for Development Cooperation (FUOS) of the University of Amsterdam also supported the preparation of the publication of the seminar proceedings. We thank the Centre for Asian Studies Amsterdam, in particular its director Jan Breman, and Iet de Groot, whose invaluable support has made the project possible. We also thank John Kleinen, Toon van Meijl, Peter van der Veer and Han Vermeulen for their critical comments on an earlier draft of this paper. Most other contributions to this issue have initially been presented at the seminar, the only exceptions being the articles by Shahid Amin and Giselle Byrnes. We thank Christopher Pinney, Nicholas Thomas and Lainee Wong L. N. for their encouragement and support in editing this collection.

2. The theme recurs in the discussion: see Diamond (1974), Gluckman (1974), Leach (1974), Scholte (1974).
3. See Diamond 1974: 37; Firth 1977: 145; Gluckman 1974: 43; Leach 1974: 33, 34; Loizos 1977: 141; Scholte 1974: 41.
4. See "Part 2: Case Studies" of Asad (1973). Some responses to it, in a special issue of *Anthropological Forum*, also paid attention to historical detail, be it in a more "personal" form (Loizos 1977).
5. See Evans-Pritchard (1981); Harris (1968); Hoebel (1960); Leaf (1979); for an example, see the critique of Kuper's mythification of Malinowski below.
6. Before 1973: Leiris (1950), Maquet (1960), Leclerc (1972), Lewis (1973), Stocking (1982 [1968]). After: Copans (1975), Copans and Jamin (1978), Diamond (1980), Fardon (1990) and Said (1978, 1989).
7. Wilhelm Schmidt is ignored by Evans-Pritchard (1981) and Leaf (1979); his missionary background is ignored by Honigmann (1976), Kuper (1983), Lowie (1937: 193), and Voget (1975). Harris (1968: 389) is an exception, but he is an adherent of a rival religion.
8. More studies are needed of the involvement of missionaries like Westermann or anthropologists like Mair in projects initiated by the colonial establishment (Cell 1989: 492, 498) and of the incorporation of former administrators like Risley, Temple, Beattie and Gulliver into academic anthropology (see Pinney 1990; Beattie 1963). Kuklick's thesis that administrators and anthropologists did not see eye to eye with each other does not touch the core of the issue and almost completely ignores the influence of the Indian Civil Service in the years before 1922 (1978; 1991: 182 ff.). Asad has pointed at "the ideological conditions of anthropology, and the implications of these conditions for its discourse" (Asad 1979). In a more general sense, Gérard Leclerc has traced the common suppositions underlying both sociology and anthropology back to the state's attempts at controlling ill-known and therefore potentially dangerous groups (Leclerc 1979).
9. We are aware that we are constructing a genealogy of ethnographic practice from the present, but we hope this historical detour allows us to escape writing "Whiggish history", by "suspending judgment as to present utility" (Stocking 1968: 12).
10. Cf. Tylor's comparison of civilizations with "stocks of merchandise" and of the "prehistoric archaeologist" with the "financier" (1871, I: 31, 55).
11. The use of the word "anthropography" seems to have been rather idiosyncratic: the OED gives as its original meaning a medical one (description of the human body) and its "ethnographic" meaning is restricted to the same source that mentioned the German origin of " ethnography " .
12. See also Copans and Jamin (1978), Moore (1969) and Stocking (1964).
13. However, these meanings were not firmly established: Tylor, for instance, sometimes uses "ethnography" where one would expect "ethnology" (1871, I: 22–23), which may be the result of the contemporary struggle between "ethnologists" and "anthropologists" (see Stocking 1971). Moreover, as we shall see below, the term ethnography has nowadays come to cover more than just the activity of describing.
14. Reining (1962: 593) argues that the academic faction of the APS split off because they were in disagreement with the rest over its missionizing intentions. Curtin (1964: 331) and Rainger (1980) show that this assertion cannot be substantiated.
15. The examples of Audrey Richards and Zora Neal Hurston also suggest that the lack of career opportunities for women partly determined the selection of monographs that were deemed important at the academy (cf. Gordon 1990: 148).
16. Yet, while we oppose Tyler's reduction of fieldwork to making fieldnotes (1987), we are also critical of his opponent's tendency to reduce fieldwork to dialogue (Tedlock 1987).

17. It is interesting that British colonial servants did not claim *writing about*, but *dealing with* "natives" as their professional competence. We might say that for them, fieldwork experience was the mark of professional competence, not ethnography (see Pels 1993a: 33, 34).
18. Ranger (1983: 229, 236) stresses the ideological function of invented traditions. Nicholas Thomas, however, notes that the emphasis on the reification of traditions often fails to integrate analytically the historical interactions between colonizers and colonized (1992: 213).
19. If they did reflect on gender issues, it was often a conscious effort to back up men's interests in matrilineal society, as was the case with a French administrator who in a report of 1937 promoted "male emancipation" in a Montagnard group. Missionaries to Uluguru promoted "female emancipation", but failed to see that their efforts achieved, if anything, precisely the opposite (Pels 1993a: 143 ff.)
20. It is important to note that in administrative discourse, what a Luguru chief represented was expressed in terms of territory (Chief *of* Uluguru, Subchief *of* Matombo, Headman *of* Konde, etc.).
21. Of course, this by no means denies that administrative or missionary ethnography was important for the colonial economy by, for instance, supporting the education of labour.
22. One is tempted to assume that settlers had a more limited access to publishing channels than missionaries and administrators, and would therefore be less inclined to record their practical knowledge of others. Settlers, however, did engage in other forms of writing, notably novels, short stories and autobiographies. These literary forms often did contain ethnographic statements, as is evident from Karen Blixen's *Out of Africa* (1937).
23. And not only in the "anthropological" form which Urry distinguishes from "romantic", "spectatorial" and "environmental" forms of the tourist gaze (1992: 22).
24. See, for example, Griffin (1989) and Boyarin and Boyarin (1989), and Said's response, (1989b); and the controversy between Shokeid (1992) and Swedenburg (1992).

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