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COLONIAL ETHNOGRAPHY: WEST NEW GUINEA (1950-1962)¹

During the 1960s and '70s the relation between colonialism and anthropology was, for a time, a much discussed topic. An early contribution was Macquet's (1964) article, *Objectivity in Anthropology*. Macquet claimed that there were two theoretical anthropological perspectives which had served colonialism in Africa. Evolutionism had justified colonial conquest and expansion, and functionalism had been useful during the next stage of colonial consolidation and administration. Macquet's analysis presupposes a direct link between the 'objective' socio-economic conditions of a group and its mental productions. However, the individuals concerned need not be aware of this connection. Nor does Macquet's analysis imply that anthropological writing did, in fact, help maintain the colonial system to a significant degree. 'What matters is that anthropology was oriented as though it wanted to preserve the existing situation' (1964:49). Macquet's argument is difficult to evaluate, because it is characterized by extremely broad generalizations at a very high level of abstraction. Still, the basic message which Macquet wants to convey is quite simple. Major theoretical perspectives in anthropology prevail because they serve colonial interests.

A more sophisticated perspective, leaving room for contradictions and ambiguities, was offered by Asad (1973). Nevertheless, it is also clear for

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him that British social anthropology before the Second World War did not constitute a 'basic challenge to the unequal world presented by the colonial system' (Asad 1973:17-18). Indeed, social anthropology avoided an analysis of this colonial system. Like Macquet, Asad is talking in terms of abstract entities and structures such as 'bourgeois consciousness' and 'colonial power structure'. Apparently one can only hope to escape the influence of such forces through acknowledging their existence. Only in this way is one able to transcend the contradictions of the colonial situation. Such an approach is of course highly attractive because the benefit of hindsight is turned into privileged insight.

However, anthropologists able to exercise such hindsight do not necessarily agree with Asad's analysis when turning to their own research experiences. A number of them were in fact asked to address the following questions: 'How far had they gone into the field critical of the dominant theories of their teachers? What constraints had they experienced as a result of the colonial situation whether in choice of topic, area, informants, the conduct of research, or the publication of results? What use if any had the colonial governments made of their work, and how had their work affected the people they studied?' (Loizos 1977:1). The answers were first given in the form of seminar talks and later published as essays (*Anthropological Forum* 1977). They make fascinating reading, but in the end one wonders whether the tremendous diversity of colonial settings and other parameters such as available time and funds make any generalizations about the relation between colonialism and anthropology plausible.

We seem to be caught between two extremes. On the one hand we have Macquet and Asad, who assume, at least implicitly, that anthropological practice in a whole continent or empire is sufficiently homogeneous for a considerable length of time to warrant the kind of generalizations which they proffer. Macquet deals with Africa in colonial times, Asad with the British Empire before the Second World War. On the other hand we have a number of practising anthropologists who deny that in their special case such generalizations can be upheld. It is our contention in this article that there is, in fact, a middle course. To be able to follow this, however, we shall have to shift our focus from theoretical perspectives to ethnographic practice.

Whatever else they may do, anthropologists are first and foremost ethnographers. They provide authoritative accounts of the way of life of other peoples. Recently there has been an upsurge of interest in the way ethnographic texts are constructed (e.g., Marcus and Cushman 1982; Clifford and Marcus 1986; Marcus and Fischer 1986). The idea that an ethnography can be a simple, detailed scientific description of events within controllable bounds has increasingly come under fire. The focus of evaluation of this 'new ethnographic criticism' (Fardon 1990:4) has shifted towards the person of the ethnographer. Yet, even while he (or she) is placed within the research situation seen as a social process, he becomes

detached, because he is the leading actor, the director, and the author of the monologue – the one whose personal ideology becomes imprinted on the resulting image. While not quarrelling with the resultant insights, we question the validity of this reduction to the individual researcher.

For one thing, ethnographic accounts must be relevant to the aims of those who are interested in commissioning the generation of such information. Such aims may be practical or academic; they may be directed towards improving or justifying specific ways of interaction with the subjects of research or to developing general insights applicable to some or all other societies. It is to be expected that differences in aims will delimit various knowledge-producing groups who may have little in common. More specifically, it could well be that a theoretically inclined academic anthropology project would be much less influenced by a colonial setting than a government research unit which had to provide useful knowledge for administrative purposes.

If we are interested in the institutional setting of ethnographic research, we should compare academic anthropology with applied anthropology and also include amateur ethnographers such as can be found among administrators and missionaries in our survey. A practical problem then is the question of when ethnography is no longer ethnography but becomes history, economics, agronomics, geography, linguistics, art history, theology, jurisprudence, and so on. Clearly no hard and fast rules can be given here, but it would seem that academic anthropology as practised by professionally organized anthropologists and documented in books, articles and films can constitute at least a prototypical core for deciding the question of what is acceptable as an anthropological problem, methods of investigation, terms for conceptualization, and format of reporting. Loosely, we can define as ethnographies those works or parts of works which are modelled on this prototypical core and are perceived as such. The gathering of information in preparation of such works can – in principle – be conceived as ethnographic field work and be studied as such.

In the next section we shall delineate a model of the ethnographic production process and of the setting, or the socio-cultural environment (including political framework and economic system), of this process. Clarification is sought through analytical differentiation, without claiming an ontological status for the categories employed. The usefulness of the analytical categories developed will be demonstrated with reference to a particular phase in the history of Dutch anthropology: the ethnographic work done in West New Guinea (Irian Jaya) between 1950 and 1962. In this way, this article will also form a contribution to the study of regionalism in anthropology. It will demonstrate the way in which

'[R]egional factors influence the entry (in the broadest sense) of the ethnographer to a field that is necessarily pre-imagined, the circumstances under which fieldwork will be carried out, the issues which

have been preconceived as appropriate and pressing, and, in writing up, the canons of adequate reporting and the audience to whom, in part at least, the work will be addressed and whose opinion will be the most telling. . . . To argue this is not to suggest that regionalization is not another strategy, with its own effects upon power and knowledge . . . Nor is it to suggest that regions are not often quite arbitrary, the historical creation of geographical master fictions. On the contrary, it is precisely because of these dimensions and because regionalism is so pronounced a feature of our professional practise that *to fail to address it risks falling into generalizations remote from our practise*' (Fardon 1990:24, our italics).

A model of ethnographic research

An ethnographic research project may be divided into three stages: preparation, field work and reporting. The researcher goes into the field with a *cognitive disposition* with which he or she confronts the local ethnographic reality. This disposition is determined by one's professional training and personal preferences for or assignment to a certain subject and area, but is also shaped by specific limitations, including existing knowledge. As Firth (1977:13) puts it: ' . . . I prefer to think less of constraints alone upon a field anthropologist, and more of a positive framework of ideas by which he may be stimulated as well as restrained, in his selection and formulation of problems and conclusions'. But this cognitive disposition not only consists of the perspective which the researcher brings to bear upon the object of research, such as particular ideas on the kind of data to be collected, categories into which the data can be sorted, and so on. It also involves the constraints upon the collection of data which stem from the conditions under which the research takes place: external demands, organization and working methods. For example, Lloyd (1977:82), who went into the field as a Marxist, could not do any survey research because of lack of funds and was obliged to operate in the traditional anthropological manner – acting as a participant observer and recording norms rather than statistical occurrences.

Field work itself is characterized by a dynamic balance between the cognitive disposition and the opportunities offered by the subjects of study. It should be emphasized that the cognitive disposition is by no means always dominant. Perceived characteristics of the ethnographic reality also shape any interests which the researcher might pursue during this stage. Firth mentions the importance of the ideas acquired from the people under study. 'If not conceded ultimate intellectual validity, at least these were used as operational guides, given positive values, and often defended in the face of colonial regulations and Western norms generally' (Firth 1977:30). Lloyd (1977:89) became encapsulated in an indigenous community and began to view the world through the eyes of his informants, precluding a more national view of the situation. Lewis's biggest problem

was revision of the model of Somali society which he had built up in imitation of the Nuer analysis by Evans-Pritchard before leaving for the field. 'The Somalis in many vital aspects were, alas, not quite what I had expected them to be' (Lewis 1977:101).

During the reporting stage the researcher distances himself from the field situation, mentally and very often physically (e.g., a return from the field). When he is writing up the collected material the initial cognitive disposition once again becomes the most important influence on the order which is now imposed. The resulting analysis again will in time provide feedback for changes in this cognitive disposition. This is well illustrated by Morris (1977:78), who wrote an account of the Melanau sago industry in Sarawak in the form of a functional synchronic account of an on-going system. But he also became aware that certain aspects of the situation, for example the ranking system which divided the population into slaves, commoners and aristocrats, could only be explained from a much wider frame of reference involving historical research. He himself, however, did not have the time to engage in such a task.

The information brought back from the field is not immediately generally available. Initially it becomes part of discourses which are aimed at producing knowledge. We propose to call this process *cognitive exchange*. First and foremost the field worker will participate in the cognitive exchange to which his patron belongs.² However, at a more informal level he will also be involved with interested and trusted colleagues. These various exchanges may or may not overlap and may differ significantly in cognitive disposition. A first product of this exchange are (drafts of) internal reports which may be confidential, or unpublished dissertations. Such material can form the basis for further cognitive exchanges before the information is published in books (ethnographic monographs) or journal articles and becomes generally accessible.

The result of the process of writing down one's information is an ethnography or ethnographic description. It involves the transformation of individually gathered (private) information into shared (public) knowledge. As a final stage in the production of knowledge such publications need to be *accepted* as reliable and truthful by significant other persons. For example, ethnographic fiction will not as a rule be accepted as knowledge with regard to the fictitious aspects of the work. Information is turned into knowledge through accepted forms of argumentation, through the establishing of credentials (academic training, description of research procedures), but this will also happen gradually when data remain unchallenged for a number of years. Such legitimation by default takes place especially with respect to archive material.

² We see the role of the patron, who commissions research and to whom the field worker is primarily responsible, as different from that of the sponsor, who provides the financial means. Both roles may of course coincide.

Acceptation is a social process but does not necessarily involve a well defined group. It may simply be a category of readers which the author has in mind when writing his report. Invariably, however, he will aim at one or more known cognitive exchanges. He will often follow successful models which have already been accepted by the reading public he has in mind. Perhaps they provide prototypical standards to which he conforms, including rhetorical means such as style and composition. The way in which acceptance by a group takes place depends very much on the research findings and also on the original assignment. It may vary between a simple re-use by members of the group in later publications and a formal (in the Netherlands ritually affirmed) examination in the course of which an academic degree is conferred on the basis of a thesis (cf. Jaarsma and De Wolf 1991).

The model of ethnographic research which we have presented here may suggest that the collection of information and the production of knowledge is a continuous and closed process. Yet this is far from being true. Ethnographic data are not exclusively gathered as part of a research project. There are many other activities which yield ethnographic information as a by-product. People acting as ethnographers in such a way may have (some) professional anthropological training, but usually they are (mere) amateurs, though there is nothing derogatory about this qualification. Nor is the anthropologist in the field completely self-reliant when collecting data. He makes use of knowledge resulting from previous anthropological research and of information from people whose expert knowledge of the research setting he believes to be reliable. Not infrequently, such advance knowledge will be indispensable during the preparatory stage when selection of the field-work location is considered.

Therefore any serious study of the relationship between ethnographic research in a certain area and its colonial setting should take into account all relevant and probable sources, and not concentrate exclusively on ethnographic production by academic anthropologists. This is necessary not only because anthropologists may rely on non-professional ethnographic accounts, but also because the various ways in which different interests influence ethnographic research and reporting provide a situation making for controlled comparison, by means of which we can test hypotheses about the supposed relationship between colonialism and anthropology. In any research situation there are many elements forming the external framework, and even in a colonial situation only some of these will be typically colonial. Their influence depends on the way various of these elements are constituted and on the way they are interrelated. It is our thesis that ethnographic knowledge production can indeed be analysed in terms of an underlying structure with systemic properties which can be visualized as a *research space*.

Elsewhere we have dealt extensively with the methodological aspects of constructing such a model (Jaarsma 1990:1-12; Jaarsma and De Wolf

1991). Suffice it to say that any research situation can be seen as a network in which (ethnographic) information is not only gathered, but also exchanged and (repeatedly) translated. Comparison of these networks or research spaces, especially for a limited time and/or region, should account for regularities. These regularities may be analysed in terms of structural limitations and stimuli. This structure of what we will call *limiting conditions* concerns on the one hand strictly material conditions, such as presence or absence of research time and means. On the other hand it involves a number of immaterial factors, such as the wishes and demands of patrons and sponsors. The specific constellations of the conjunctions of such limiting conditions allow us to gain an insight not only into the cognitive dispositions from which research is started, but also into the division into cognitive exchanges that is characteristic for both ethnographic products and 'producers'.

West New Guinea: A case

Before applying the theoretical notions which we have developed in this article to West New Guinea, we shall give some pertinent information on this area. Before the Pacific War, Dutch New Guinea was very marginal in the Dutch East Indies. Scattered along the coast were a number of government posts, but large parts of the island remained beyond regular control. The very sparse population contributed to this situation. Even in 1961, at the end of a decade of more intensive Dutch rule, the estimated number of inhabitants was 717,055, only 486,755 of whom lived in administered areas.

Protestant missionaries had been active in the north some 45 years before any kind of colonial administration was established. It was only after 1898, however, that Christianity began to take root among the Papuans. The reason for this was probably not a significantly greater Western physical presence, but a more visible one, which gave both the administration and Christianity greater interest for the population. A similar situation existed in the south, where Catholic missionaries started their work in 1905, but conversion on any scale did not take place until 1920, when large-scale health campaigns were launched. The administration, pressured by the Catholic mission, attempted to contain an epidemic of venereal disease and also actively encouraged formal education by subsidizing schools run by the churches. One should not overestimate the Western presence during the first half of this century, however. Catholics and Protestants, as well as the administration, relied heavily on personnel from Eastern Indonesia (the Moluccan Islands) for the actual work in the villages.

During the Pacific War the Japanese occupied large areas of Dutch New Guinea, inflicting extensive damage on both administrative and missionary interests. After the war the political future of the area remained uncertain for several years due to the Indonesian drive for independence.

Although in the end sovereignty was granted to the Dutch East Indies on terms dictated by the Indonesian nationalists rather than the Dutch, the latter wanted New Guinea to be treated separately from the rest of the colony. The status of the area remained undecided when Indonesia became an independent nation state in 1949. In the Netherlands there was at no time a consensus on the terms under which the Dutch would give up New Guinea. The political parties advocating a continuation of Dutch administration were powerful enough to block any attempts to reach a settlement acceptable to the Indonesians. Only in 1961, when the military threat of Indonesia became serious and international support dubious, this political resistance collapsed, and in 1962 Dutch rule was rather suddenly discontinued (cf. Van Esterik 1982; De Geus 1984; Lagerberg 1979).

In the period between 1950 and 1962 the Dutch had to turn a marginal district at the periphery of their pre-war colonial empire into a colony with an autonomous administrative infrastructure. They legitimated their presence with an appeal to article 73 of the United Nations Charter, obliging member-states to take action to achieve political self-determination for the inhabitants of non-self-governing areas as soon as possible. Economic development, communications, education and public health had to be taken seriously. Thus the need for all kinds of information, not only about the resources of the area, but also on the autochthonous populations, the Papuans, suddenly became apparent, and a special research office, the *Kantoor voor Bevolkingszaken*, was established.

On the basis of our analytic assumptions outlined above, one would expect that patronage would make a crucial difference to the disposition with which one engaged in ethnographic research. Not only would academic, administrative and missionary aims each require different kinds of information, but one would expect also that the results of field research would be mainly discussed within the setting of the organization responsible for the assignment and that the knowledge thus generated would influence the disposition bearing on further research. To a certain extent, this is what we do find in hindsight.

The main actors

The main apparent division in ethnographic research is institutional. Up until the early 1960s almost all research in West New Guinea was done from within administrative and missionary institutions. Occasionally academic researchers were contracted, but their contributions were to a large degree defined by patrons and sponsors. In the late fifties and early sixties academic research financed by academic institutions became prominent. These three institutional backgrounds – administrative, missionary and academic – are representative of the main differences in cognitive disposition. We will look at each in greater detail.

The administration itself commissioned a major portion of research. Administrators were asked in the course of their normal duties to answer

questionnaires on various aspects of the areas for which they were responsible. They also had to provide relevant ethnographic information in their periodical reports to their superiors. Occasionally publications were prepared on the basis of this material. For instance, much information for a publication on customary law according to the format of *adat* (native) law codification as practised in the colonial Netherlands Indies was culled from such reports (Adatrechtcommissie 1955). Central to much of this activity was the *Kantoor voor Bevolkingszaken* (Native Affairs Bureau). Part of the duties of its staff consisted in conducting, initiating and (more often) evaluating social and economic research. Most staff members were especially recruited for these purposes. By way of exception, regular members of the administration could be detached from their normal posts and given temporary research appointments. Initially the head of the *Kantoor voor Bevolkingszaken* was directly responsible to the governor for advisory purposes. In later years this partial autonomy was withdrawn and the tasks and responsibilities of the staff of the Bureau became more similar to those of other technical staff (health, agriculture, mining, etc.) within the administration.

Both administrative and missionary research share the same broad aims of pacification and acculturation. Yet, due to their respective preoccupations, this affected the cognitive disposition of the respective researchers differently. The administration used a simple set of general aims: the maintenance of order and prevention of social unrest on the one hand (pacification), and the efficient implementation of social, economic and – at least in part – political development (acculturation) on the other. Most research questions arose from problems and a need for information directly related to these aims. Pacification and acculturation acquired a different meaning for the missionary institutions. Their content was strongly influenced by the given tasks of Christianization and education. Pacification to them meant the removal of impediments to a truly Christian way of life, which differed in some, often crucial ways from what the administration saw as its duty. At first sight, missionary responsibility for education might seem to indicate more extensive involvement in acculturation than in the case of the administration. However, most schools were subsidized by the administration and run on conditions set by it as regards basic curriculum, teachers' qualifications, and so on. The ultimate responsibility for most initiatives aimed at social development was in effect a shared one.

Missionaries in their research met with specific typical problems in their role as missionaries. In the 1950s both Protestants and Catholics acknowledged that the introduction of a new religion – and with it a new way of life – presupposed at least some understanding of what one was condemning (Freitag 1954:77-78, 84-85; Van Randwijck 1981:414-415; Kamma 1950:273; Jaarsma 1990:75-76). This was not without certain problems. Collecting information about traditional religious practices was frequently

felt to be incompatible with the insistence on adopting a new religion, especially by the missionaries' informants, who were often new converts. The net result is a collection of ethnographic works which differs considerably from the administrative material (Jaarsma 1990:84-89, 96-97). Missionary publications are mostly aimed at a broad public of supporting fellow-believers at home and at other missionaries from the same congregation or missionary society. Occasionally, such reports found their way into administrative offices, but this was not common practice. The fifties in particular saw a growing awareness among missionary institutions of the importance of knowledge about the autochthonous population. This led to the independent recruitment of scientific researchers, both anthropologists and linguists, not only by the major denominations (the Roman Catholic and the Dutch Reformed Church) but also by the smaller American and Australian fundamentalist movements (the Christian and Missionary Alliance, Seventh Day Adventists, etc.). These initiatives resulted in a small, but very varied body of academic publications with a missionary background.

The academic researchers, and especially those anthropologists commissioned to do research for a doctoral dissertation, worked with an entirely different set of priorities. They had to keep in mind the requirements of their supervisors and examiners while out in the field and writing up their data. Research priorities were formulated in terms of ethnographic coverage (filling in blank spaces on the map) or opportunities for comparison. This might be on a regional scale, such as the comparison of highland tribes in East New Guinea with those in West New Guinea, or on the scale of comparison of isolated communities virtually living in pre-contact conditions with the situation prevailing elsewhere (Jaarsma 1990:123-127, 145-146). There were still areas which were not controlled by the Dutch administration, and these came to be very much in demand, especially among American anthropologists (Pospisil 1978:97; Jaarsma 1990:130, 137-138). West New Guinea in fact provided ideal 'laboratory' conditions (e.g., Heider 1979:23) for anthropologists interested in cultural evolution and a holistic methodology. On the whole this group of ethnographers was not concerned with the representativeness of their results for a wider area (e.g., Pospisil 1978:102-103). Even if they were concerned with the issue, there was often very little comparative material available (e.g., Heider 1975). They preferred to study a particular local community in depth. Their cognitive disposition can be described as being characterized by research questions inspired by theoretical interests and the provision of cultural inventories. As such, it contrasts sharply with the mostly practical and policy-aimed research conducted by administrative and missionary institutions.

Ethnographic research

Initially two extensive research projects were formulated by the *Kantoor voor Bevolkingszaken* with the aim of providing ethnographic information by trained anthropologists on Mimika and Sarimi respectively. Both studies – framed around fairly open instructions to provide a general description of the area – took two years to complete. Though in both cases short ethnographic reports were written on specific subjects, the final report was an academic dissertation which was viewed as a cause of additional delay by the administration. The arguments were eventually found to be too theoretical to be of much practical use. Drawing a lesson from this, the administration later commissioned research that seldom involved more than a few months and concentrated on providing background information for specific problems which had come to its notice. This research had to result in internal reports to be completed within a short space of time.

The restriction on research time meant more than just a possibly smaller scope of the research questions asked and lesser depth of the answers received. It put limitations on both the researcher, the subjects of study and the methods used. Schoorl (1967:176-178) formulates four prerequisites for the success of a short-term investigation, as follows: (1) personal experience of the researcher, either in this type of research or with the subjects of study; (2) access to records concerning the subjects; (3) availability of a contact language, spoken by both the researcher and the subjects; (4) a certain 'ripeness' for social science research among the subjects. It may be clear that one can never adequately cover new ground given these prerequisites. This way the phenomenon of acculturation inevitably became the focus of attention of administrative research.

The information gathered by the *Kantoor voor Bevolkingszaken* was rarely transmitted directly to the local level where it had initially been collected. Research gave rise to policy recommendations which might be taken up at a higher administrative level and used to formulate instructions to be carried out at the local level. Publications show the discussion (cognitive exchange) among staff members of the *Kantoor voor Bevolkingszaken* itself to have been of no direct practical relevance for the administration. They were preoccupied with theoretical notions, among which the usefulness of the concept of social structure and the applicability of general descriptive terms were prominent.

Turning to the missionaries, we see that there was almost no exchange of information between Protestants (the major churches as well as smaller fundamentalist groups) and Catholics. Working in the same region, trying to convert the same people, they viewed each other as competitors and as being unwilling to provide useful information. When this was not the case, there was of course little incentive to learn about different local situations. This was also true of the individual Catholic congregations and Protestant denominations, which not infrequently each had their own style of missionizing and were responsible for different areas. There was little need

for ethnographic information on places outside one's immediate area of responsibility or collected with different aims in mind. For example, missionaries working directly with the people they wanted to convert in their own vernacular had little in common with missionaries working within a more hierarchical organization through intermediaries, often minimally trained Indonesian or Papuan teachers, using the locally spoken variant of Indonesian. Paradoxically, the greater (social) distance between missionary and convert in the latter kind of situation made ethnographic information all the more important. Both the extent of the area covered and the methods used influenced the interest in ethnography, as well as the amount of data gathered and exchanged, thus determining the extent and openness of the resultant cognitive exchanges.

Academic research took place mainly in the late 1950s and early 1960s and remained by and large incidental. As Indonesia virtually closed the area to Dutch academic research for many years to come in 1963, and has generally been very restrictive in granting research permission since, the development of most cognitive dispositions was disrupted. As they could no longer function productively, they might even be said to have disappeared. Only in a few isolated cases a (semi-permanent) change of venue to Papua and New Guinea was made (Van Nieuwenhuijsen, Van Nieuwenhuijsen-Riedeman, Van de Kaa, Ploeg).

The resulting discussions remain highly fragmented to this day. Cognitive exchange with the aim of producing a body of knowledge sustained by a specific social group has been restricted to various national and local traditions. There has been a difference not only between, for instance, American and Dutch academic research which made the respective cognitive dispositions incommensurable in many respects, but also between Dutch universities, such as Leiden, Utrecht and Amsterdam, mutually. At Leiden there has been much more emphasis on the search for underlying structural principles at a fairly high level of abstraction than at Utrecht, while Amsterdam was heavily influenced by American developments with respect to comparative functionalism. Graduates from Leiden, and at a later stage Utrecht, dominated the discussion within the *Kantoor voor Bevolkingszaken*. Amsterdam became active only in the early sixties, with its graduates working mainly as academic researchers. After 1962 a number of former administrative researchers came to be concentrated in the Royal Tropical Institute in Amsterdam.

Although we can distinguish three broad dispositions on the basis of the patron institutions which initiated and/or sponsored research, this does not mean that communication was equally good within each circuit. The academic disposition simply had not the time to assume a definite, distinctive shape (Jaarsma 1990:144). The virtual closure of the area to Western researchers between 1962 and the late 1970s removed most of the incentives towards further integration. As was remarked above, the anthropologists of the *Kantoor voor Bevolkingszaken* were set apart from the rest of

the administration (Jaarsma 1990:104-108). This allowed, and in a way forced, them to cultivate their own theoretical interests (Kantoor voor Bevolkingszaken 1959; Pouwer 1961a, 1961b). Research on social change which inspired most of the requests for information from the administration did not lead to any consistent cognitive disposition. Each researcher elaborated his own views on local manifestations on an ad hoc basis (Pouwer 1961a:23-24; Jaarsma 1990:107-108, 121-122). Yet, when one reviews the literature, one cannot escape the impression that in most places traditional and new behaviour patterns co-existed and were selected by the autochthonous participants themselves according to situational criteria (e.g., Pouwer 1955:250-276; Schoorl 1957:230; Dubbeldam 1964:298-302; Galis 1955:224; Kouwenhoven 1956:152 ff.; Pospisil 1978:103-105). However, though researchers from all dispositions commented upon this phenomenon, this did not give rise to a consistent critique or even querying of the prevalent notion that acculturation had to do with a change in consciousness.

Most missionary and administrative ethnographic reports did not enter the academic cognitive exchange circuits, either because the relevant publications were aimed at a non-academic public or because the reports had only a limited circulation, if they were not actually declared confidential. Academic research, on the other hand, often came late, lacked systematic access to governmental reports, and was not exceptionally distrustful of missionary reporting. The religious bias of the latter was felt to be incompatible with scientific objectivity. Van Baal's comments on Verschuieren's analysis of gender relations among the Yéi-nan are very illustrative of the resultant ambiguity.³ Van Baal says:

'The point is that Verschuieren's data are thoroughly convincing. He did not realize their purport and tried to use them as proof of the moral superiority of the Yéi over the Marind – a vain attempt, but for this very reason an indubitable confirmation of both their worth and of the author's sincerity . . . Verschuieren was not an anthropologist, but his love of mankind and his interest in his fellow-men turned him into one of those wonderful ethnographers who are the real masters of our discipline in that they provide us with ever new models of human behaviour and expression' (Van Baal 1982:102).

Informal relations

In spite of the inherent differentiation, West New Guinea ethnography has much in common, in any case more than one would expect on the basis of the description of formal relations. How can this be explained? One important factor is that, if trained at all, the people participating in the

³ Similar comments are to be found in Van Baal's introduction to Hylkema's study of the Nalum (Hylkema 1974:v) and were registered during personal interviews with Father Boelaars M.S.C. and Father Peters O.F.M.

various cognitive exchange circuits had received similar anthropological training. From the 1920s onward Protestant missionaries were taught anthropological subjects by staff from Leiden University and the Ethnological Museum at Leiden. Many administrators and academic researchers received their training from the same teachers, although presumably at a different level of sophistication, as until after the Second World War missionary trainees did not need an academic degree. Kamma, who was mentioned earlier, completed an academic course in anthropology after a prolonged stay in the mission field. His studies culminated in a dissertation on the basis of material he had collected while in New Guinea, which was published in 1954 (English translation 1972), at about the same time that under the same supervisor (Professor J.P.B. de Josselin de Jong, Leiden University) the first dissertations written under the auspices of the *Kantoor voor Bevolkingszaken* were completed.

Among the Catholic missionaries additional academic training in anthropology and linguistics was provided from the 1940s onward, both as part of their preparatory training, and for some even at an academic level. This even led to the part-time appointment of Father J. Boelaars M.S.C. at the *Kantoor voor Bevolkingszaken* in the period 1951-1956, when trained anthropologists were difficult to recruit. Regular academic training in anthropology was also received by a Franciscan Catholic (H.L. Peters O.F.M.) and a Seventh Day Adventist missionary (G. Oosterwal). All were given leave or partial leave from their missionary duties in order to concentrate on their research, culminating in both academic dissertations and ethnographic monographs.

The two resultant theses were supervised by Professors Van Baal and Fischer respectively, who were both directly and indirectly much involved in research by both anthropologists employed by the administration and independent academic anthropologists. Both supervisors were key figures with respect to the evaluation of articles submitted to scholarly journals for publication, advising learned societies about the suitability of monographs for their publication series, and securing subsidies for books to be published commercially. Their editorial advice had to be taken seriously by amateur ethnographers and professional anthropologists alike.

The anthropological training received by administrative officials engaging in research and by Dutch anthropologists working as independent academic researchers of course varied somewhat, depending on the time at which they completed their studies and the university where they did their courses, but in most cases their anthropology courses were combined with other subjects, which received perhaps more attention than anthropology proper. Those starting their studies before 1951 (e.g., Pouwer, Van der Leeden, Schoorl) mostly had as background a course in what was called *Indologie* (Indonesian studies), which provided colonial civil servants with training in anthropology (which was not always obligatory), as well as, for instance, Indonesian languages, law and economics. In later

years, some (e.g., Broekhuijse, Dubbeldam) pursued a similar type of course, called *Niet-westerse sociologie* (Sociology of non-Western peoples), which aimed at providing training for persons who had opted for a practically oriented career as development experts. Others (e.g., Grootenhuis, Van Logchem) combined anthropology with a course in geography. This latter possibility existed at Utrecht and Amsterdam, but not at Leiden. In any case, even a degree course in cultural anthropology proper did not provide intensive field training, at least until the early sixties.

A secondary factor is that informal contacts in the field not infrequently transcended the formal institutional divisions between cognitive exchange circuits based on differences in sponsorship. Informal networks resulting from personal contacts during work in the field can still be traced today. Limited research time and funds obliged researchers to consult local personnel not only about the most favourable research location, but also for basic information. At the local level, the exchange of information could be quite intensive provided that academics, administrators and/or missionaries got along well on the personal level. If this was not the case, duplication in information gathering and knowledge production was unavoidable.

Distrust, however, was a very common reaction. There were definite stereotyped notions prevalent about the social situation in West New Guinea which restricted the interests of the various categories of researchers in significant ways. On the whole, the relation between the missions and the administration had a history of distrust dating back approximately to the previous century. Furthermore, at most levels the borderline between the responsibilities of the missions and the administration was often extremely vague, which could easily lead to conflicts of competence. The academic researcher, although he had but limited contact with the colonial society, posed a different problem for the administration and mission. The fact that academic researchers had no roots in colonial society, and that they sought relative isolation from the local administrative and missionary posts, made it difficult to control their activities (cf. Van Nieuwenhuijsen and Van Nieuwenhuijsen-Riedeman 1972:350). Their presence might even be perceived as a nuisance. Such problems of competence and control could only be resolved at the local level (Jaarsma 1990:131-132). The magnitude of the informal networks that can still be traced today is indicative of the extent to which researchers were dependent on one another (Jaarsma 1990:190-191, 200).

Guiding ideas

When the Dutch had decided to retain West New Guinea as a colonial dependency in 1950, little was known about the area and its inhabitants. This was, of course, one of the reasons for encouraging ethnographic research, especially in areas where previously there had been no administration or where it had been so minimal as to leave few useful admin-

istrative records. It meant, however that the cognitive dispositions of various categories of ethnographers were open to slightly stereotyped notions based on the scant available information (e.g., Interdepartementale Commissie 1953; cf. Jaarsma 1990:49-55). On anthropologists whose training had focused almost entirely on Indonesia, Papuan cultures created an impression of extreme variability and incompleteness (Van Baal 1953/1954; Held 1951). In most respects New Guinea was the opposite of Java, where the society was characterized by Geertz as being marked by 'cultural involution'.

The explanations of this variation differed in scope and intent, but initially they were formulated in terms of underlying cultural or psychological principles common to all Papuans (Van Baal 1953/1954:439-441; Held 1951:51-52). Later, emphasis shifted to the fragmentation of the population in isolated, small-scale, localized groups (Kantoor voor Bevolkingszaken 1959:137 ff.). Yet this explanation was as much an outcome of the research conditions as of inherent qualities of the object of research (Jaarsma 1990:117, 196-198; Pouwer 1968:14-20; Schoorl 1967:176-178). Pacification and resettlement of the population in villages for the purpose of improving access to education and medical facilities had restructured Papuan societies in such a way that the local group emerged as the most important unit of research (Van Logchem 1963:10-11, 50-53). This was especially the case where research had to be completed in a short span of time or where researchers preferred in-depth knowledge about a single community to data on regional networks. It is even doubtful whether such networks still existed and had not been destroyed as a result of the prohibition on warfare or supplanted by trade and other economic relations with Europeans (Van der Schoot 1969:113-114).

Another guiding idea which was apparent from much ethnographic research was the concept of acculturation. Changes in Papuan society were viewed against the background of essential aspects of Western culture, which formed the standard by which the success of missionaries and administrators could be assessed. Although the emphasis differed, little attention was given to changes among Papuans which were not seen as a direct response to the endeavours of missionaries and administrators. This was especially striking in urban contexts where Papuans organized themselves in various voluntary associations (Jaarsma 1990:167-170). The lack of interest among researchers was matched by administrative indecision with respect to the encouragement of (permanent) urban migration and the legal status of urban Papuans (Broekhuijse 1960:50). Other examples of such 'spontaneous' acculturation (Kamma 1961:5), such as cargo cults and particular religious movements, inspired such diverse explanations as 'traditional religious response to culture contact' and 'irrational reaction when traditional patterns of socio-economic

organization were felt to be no longer adequate'.⁴ The administration was not keen on research being done into these phenomena because the interest thus shown in them might easily give the Papuans the impression that such movements were taken seriously (Jaarsma 1990:120, 160 note 14). Though missionaries, too, gave attention to these phenomena (e.g., Kamma 1972 (1954), 1961; Kabel 1953), their principal attention was aimed at proselytization (Boelaars 1968; Kijne 1955; Kamma 1959).

Colonial interests

In West New Guinea, ethnographic research by missionaries and academic anthropologists remained by and large separate from direct administrative colonial interests. In fact, the Christian Churches – local sentiments notwithstanding – did not even unequivocally support the continuation of Dutch colonial rule when Indonesian pressure mounted (Jaarsma 1990:69-70). Protestant missionary interests in the Netherlands were highly critical of Dutch government policy, no doubt under the influence of a desire to avoid jeopardizing their Indonesian commitments (Jaarsma 1990:35). Similarly the Catholic Church was careful not to antagonize the Indonesians because it wanted to stay on even if the Dutch colonial administration would be forced to leave (Bank 1984:480).

Academic anthropologists were without exception committed to studying small groups in depth, and their interests were largely theoretical, in a way which could be only very indirectly, if at all, useful to the administration (Pospisil 1978:97, 102-103). Insofar as it can be viewed as salvage anthropology – the expression 'colouring in white spaces on the ethnographic map' was very popular in those days – it even made the point that pacification and Christianization were destroying the cultural diversity, which was deplorable from a scientific point of view (Jaarsma 1990:123-129). On the other hand, it would not do to be too critical of a colonial administration which went to great lengths to encourage and often sponsor anthropological research.

When we consider the research conducted under the direct responsibility of the administration, we even find that enough room was left for the development of cognitive exchange and that ethnographic knowledge production was geared more to academic than colonial administrative interests. On the one hand, much administrative research consisted in finding concrete answers to concrete questions. These activities were

⁴ Both comments apply to the Korero movement which was especially in evidence during the 1930s and 1940s. The authors worked in the area. The qualification 'traditional religious response' is taken from the anthropological dissertation by Kamma (1972, originally published in 1954), who worked as a missionary in the area. The other qualification is from De Bruijn (1949), an administrative officer who was to head the Kantoor voor Bevolkingszaken in the late fifties. The latter perspective, viewing these phenomena as an irrational response to social upheaval, was to be most eloquently expressed some years later by Van Baal through the concept of 'erring acculturation' (Van Baal 1960).

regarded as part of the normal administrative routine. This type of research resulted in a continuous stream of information in reports or in occasional assignments for more intensive inquiries into specific problems, but did not provide much opportunity for theoretical innovation. On the other hand, it left room for largely autonomous initiatives, especially where research as such was the *raison d'être* of an administrative institution, as in the case of the Kantoor voor Bevolkingszaken (Jaarsma 1990:122). Here it was easy for the academic interests of the researchers to become dominant in isolation from the actual practice of local administration. This isolation effectively existed because the tasks of the Kantoor voor Bevolkingszaken were restricted to the gathering of data and formulation of advice and because the majority of the personnel working there over the years had never actually served in an administrative capacity. Only its departmental head was invariably selected from the ranks of the administration (Jaarsma 1990:102-108).⁵

Conclusion

The overall picture of colonial ethnography in West New Guinea between 1950 and 1962 is at least as complex as any of the situations the anthropologist – whether the amateur or the professional – normally studies. Apart from studying the people who actually gathered the data, we also considered their respective backgrounds and mutual involvements, directly and indirectly, both on the institutional and the informal level. Ethnography, even when considered in a specific colonial setting over a limited period of time, turns out to be much less homogeneous than was assumed in the critical remarks of Macquet and Asad. On the other hand, we question the usefulness of focusing exclusively on individual cases which then appear to represent totally contingent situations. In fact, although each case is unique, the role of the ethnographer in the process of gathering information and getting it accepted as knowledge is, if anything, overdetermined.

It was not possible to find any single common denominator for the overall ethnographic discussion, but it was possible to distinguish several different cognitive exchange circuits, determined in part by the limiting conditions that apply to typical research situations. Although there is of course no such thing as 'typical' research situations, yet most if not all share a set of common limiting conditions. These are in part immaterial in nature (the training received, preparations for fieldwork, methodological preferences, but also the instructions from patrons and/or sponsors), and in part material (time and means allowed for research, access to certain areas, lack of knowledge of local languages, etc.). The overall result is a paradox.

⁵ Van Baal, Grader, De Bruijn and Galis all had extensive experience within the administration before taking up positions with the Kantoor voor Bevolkingszaken. The first three were the successive heads of the bureau, while Galis served as assistant head of the Kantoor voor Bevolkingszaken for most of its existence.

Looking at the cognitive exchanges on the institutional level, especially the immaterial limitations are seen to have tended to reinforce the available divisions into cognitive dispositions. This is particularly apparent from the 'localized' nature of missionary ethnographic interest. Yet, the difficulties met with in the field (the material limiting conditions) made researchers from whatever background in any one location dependent upon each other. The resultant informal networks of relations are an important factor to take into account when evaluating ethnographic work done in this period.

As was to be expected, a uniform reaction to the colonial circumstances which influenced ethnographic fieldwork was not discernible. Looking at the West New Guinea case, we see a variety of stances, ranging from the kind of active support that is presupposed by Macquet, through the passive condonation or toleration that is emphasized by Asad, to more or less open criticism. The option chosen depended heavily on the actual involvement of the ethnographer in colonial society itself. Administrative and missionary researchers could hardly avoid this, whereas academic researchers to a large extent could. But, perhaps even more than in other late colonial contexts, it was clear that Dutch colonialism in West New Guinea was no lasting option, and alternative scenarios anticipating a different political future were being evolved, for instance by the major Christian denominations.

When we consider the relationship between ethnography and colonialism from the perspective of the administration as the obvious representative of colonial interests, we find no evidence that it either used or was able to use ethnographic texts for positive confirmation of its position. In fact, there was a certain ambiguity in its attitude, if not specifically towards ethnographic knowledge, then at least towards the professional full-time anthropologists who produced this knowledge, including those of the *Kantoor voor Bevolkingszaken*. On the one hand, the administration's own involvement in the gathering of ethnographic data had few tangible results. There was an upward flow of locally collected information, but no corresponding downward flow. The links between research and policy formulation or implementation were coincidental and circuitous. On the other hand, the texts produced by professional anthropologists, whatever their backgrounds, did not on the whole fulfil any direct needs felt by the individual administrators and were apt to be considered superfluous. Internal (administrative) reports were not always available locally, and freely accessible publications were not always considered relevant. Both factors taken together created a feeling of distance between the administration proper and those engaged in gathering ethnographic data, giving rise to mixed feelings, not only at the personal but also at the institutional level.

The situation in West New Guinea was in many respects unique and should not be considered representative of late colonial societies in gen-

eral, let alone as a typical example of colonialism as such. Yet there is no reason to suppose that other colonial situations, or for that matter any other context in which ethnographic fieldwork takes place, are less multiplex in structure. The New Guinea case shows that ethnographic description, like any other scientific activity, is an attempt at adding to the existing knowledge in a controlled and controllable fashion. Knowledge, however, always remains, to use Fleck's words (1979:37-38), a 'harmony of illusions'; it does not exist in itself, but because it is shared, because it forms part of a social and historic process. Our concern with the relation between colonialism and ethnography allowed us to demonstrate that ethnographical research as a social process does not stop with the researcher and his subjects or object, but of necessity includes the setting of the research itself as well. It also illustrates that, '[t]rying to discuss ethnography without specific attention to place and time is indeed tantamount to cutting away the ground on which the fieldwork took place' (Fardon 1990:29).

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