A SHARED AND CONTESTED HERITAGE

THE DUTCH AND AMERICAN EXPEDITION

TO NEW GUINEA OF 1926

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“Les grand pères toujours ont tort, especially in politics and the arts.”

- Oscar Wilde *THE PICTURE OF DORIAN GRAY*

*Pictured right and frontispiece: sketches of ethnographic artifacts by Charles le Roux 1927*
Abstract

This paper aims to write the ‘cultural biography’ of the material heritage of the 1926 Dutch and American expedition to Dutch New Guinea. This heritage has been scattered across museums in Europe, North America, and Asia and is divided in various collections of ethnographic artifacts, photos, film, and one unique collection of wax recordings of Papuan music. In investigating the fate of the various collections, the paper touches on issues of colonial science and representation. In the final chapters, the paper narrows down to the collections at the Tropenmuseum in Amsterdam and analyzes the way this museum has dealt with its own colonial past. The paper ends with recommendations on how the Tropenmuseum may better achieve its goals and what further research on this material heritage may be considered.
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INTRODUCTION

Confronting Colonial Heritage

A common cliché says that the children will inherit the world that we create for them today. If we turn this saying around, it may be said that the world we live in today is the creation of our ancestors and in particular our parents and grandparents. Such statements approach platitudes but their inescapable truth is full of consequence. Among others, this means that if I strive to understand the world and its people in particular – the vain ‘will to meaning’ of the anthropologist, according to some (Argyrou 2002) – this cannot be done without knowing our direct ancestors, their actions and their dreams, because these dreams and actions bring about contemporary ones.

Here, I will be concerned with one aspect of this great inheritance, which is referred to as colonial heritage. It may well be argued that colonialism and its aftermath, the equally traumatic process of decolonization defined the generation of my grand parents and my parents (using the possessive metaphorically). In other words: to understand this world it is necessary to understand colonialism and its heritage. Moreover, this heritage is also a contested heritage – or in any case, it has been until recently.

Colonialism and its Heritage

The following paper is as much about the process of dealing with the heritage of colonialism as it is about colonialism itself, but first I need to define some terms: what is colonialism and what is its heritage? With this one, very reductive word – colonialism – I conveniently refer to a period in modern world-history, the ‘Age of Colonialism’. This can be taken to refer to the one and a half century or so between the 1820’s and 1970’s: a period that
saw the rise and wane of an aggressive expansion into – and domination of – the non-western world, particularly but not exclusively by (west-) European powers. It could be argued that in some cases colonial domination continues to this day, but on a rather minor scale and in a rather different context. My dates are rough and somewhat arbitrary, so they need some justification.

The beginnings of colonialism can be found much earlier than the 1820’s, in the 16th and 17th century exploration and subsequent domination by the Spanish, the Portuguese, the Dutch, the English and the French. However, after the catastrophic Napoleonic wars in Europe, these countries started to consolidate and expand control over their old trade positions, particularly in Africa and Asia. The British and to a degree the Dutch were particularly active in the early 19th century, whereas Spain was the notable exception because all its American colonies became independent in the 1820’s. In the mean time, British and Dutch colonial exploitation turned out to be increasingly profitable and other countries joined in: France, Belgium, the newly unified states of Germany and Italy, and the United States, which in the late 19th century declared its frontier to be closed. This competition caused an increasingly feverish quest for overseas possessions, a process that peaked in the 1870’s and 1880’s as the states mentioned above divided the world up among themselves. The year 1876 was particularly momentous. In this year the British formalized their domination of India by crowning their Queen Victoria Empress of India. In the same year King Leopold II of Belgium ‘acquired’ the Belgian Congo as a private citizen, one of the defining moments in the ‘Scramble for Africa’. This term is commonly used to describe the rivalry among Western European states for African possessions, a process that culminated in the 1884-85 Congress of Berlin, another landmark in Colonial history. After this period, the Colonial or Imperialist Age was at its heyday: only further consolidation of the possessions was possible, as there was scarcely any room for expansion.
The end of European colonialism came with the end of the Second World War: the period between 1945 and the 1970's was marked by decolonization, which at this time of great ideological conflict – the Cold War – was inevitably mixed up with much revolutionary fomentation both in the countries of the soon-to-be former colonizers and colonized. Over 30 years have now passed since Europe’s last major colonies became independent. This process of decolonization is incomplete; politically and economically there remain unresolved legacies. Examples abound, small or not so small island realms such as the Netherlands Antilles, Britain’s hotly contested Shetland Islands (or Malvinas), France’s Nouveau-Caledonie, the United States territory of Puerto Rico, and quite a number of other places remain problematically connected to their respective ‘mother countries’. However, the political and economic impact of these ‘overseas possessions’ – unlike their larger predecessors – is minimal.

Moreover, the intellectual climate has changed considerably. There are people who have argued that we are currently seeing a period of resurging colonialism, or ‘neo-colonialism’, especially manifested in the United States’ occupation of Iraq and Afghanistan. However, it may be too early to assess the political, economic and cultural impact of these invasions, and so I will ignore these issues, too. Moreover, the focus of this paper is on the heritage of ‘classic’, predominantly West European colonialism.

Others have called the current age ‘post-colonial’, a word that may be taken to mean ‘after colonialism’ – in the sense that the heyday of ‘classic’ colonial domination of the 19th and 20th centuries is beyond us. However, when I use the word post-colonial, I do not take it to mean that colonialism is something of the past entirely. Post-colonial(-ism) I take to mean an ongoing engagement with the heritage of colonialism (cf. Appiah 1991), not unlike post-modernism can be taken to be an ongoing engagement with ‘modernism’. The scope of this paper is entirely post-colonial in this sense. These terms –
post-colonial, neo-colonial, decolonization – also frame the discussions over the heritage of colonialism.

Case Study and Organization of the Chapters: the 1926 Dutch and American expedition to New Guinea

In this paper I want to investigate the history of colonialism and – perhaps more importantly – subsequent generations’ involvement with its heritage in times of de-colonization and post-colonialism. I will do so particularly, but not exclusively within the context of the museum and my method will be a contribution to the ‘cultural biography’ (Kopytoff 1986; cf. Appadurai 1986) of the material heritage of the 1926 Dutch and American expedition to Dutch New Guinea. This varied material heritage consists of collections of ethnographic artifacts, photos, film, and a unique set of wax cylinders with recordings of Papuan music. Immediately after the expedition, these material ‘remains’, this heritage have been divided up and they are now distributed over three continents. For reasons of space I have limited my discussion mostly to those collections that currently reside in Amsterdam,¹ the Netherlands. An important part of this material heritage has ended up in the collections of the former Colonial Institute and Museum in this city, which is the current Royal Tropical Institute (KIT) and Tropenmuseum (‘Museum of the Tropics’). Over time this institute has given parts of its collection away to the University of Amsterdam and the Nederlands Filmmuseum, also in Amsterdam, thus further scattering the collections.

This expedition is of particular interest with regard to the history of early Dutch colonialism in New Guinea, because it was the first expedition to

¹ Although the author conducted much of his research with Dr. Paul M. Taylor at the Asian Cultural History Program of the Smithsonian Institution at the National Museum of Natural History, his research was always centered on the Dutch aspects of this expedition. Whereas inevitably, he collected much material regarding the American collections, reasons of brevity joined with the considered Dutch audience of this thesis (at the University of Amsterdam and Tropenmuseum) made him decide to focus on the Amsterdam collection. Paul Michael Taylor wrote much of the history of the Americans in this expedition (Taylor 2006).
be organized primarily for anthropological and ethnographic research. The expedition was also a first, if controversial and ultimately unsuccessful attempt to give an airplane a central place in an expedition thus bypassing the tested, but cumbersome format of a military-led ground expedition. Furthermore, the *Tropenmuseum* uses this expedition as the ‘type’ of a colonial scientific expedition in its new semi-permanent exhibition about Dutch colonialism called ‘Eastward Bound!’ Yet very little has been published regarding this expedition – both at the time and afterward – and so the first chapter of this paper will first present a (preliminary) synthesis of the expedition’s history.

A diverse group of people was brought together for this expedition, some of who kept diaries during the expedition (Stirling 1926; Hedberg 1926; Docters van Leeuwen 1926b; Le Roux 1926c-d; Posthumus 1926b). The American anthropologist-ethnographer Matthew Williams Stirling was the initiator of the expedition and initially its over-all leader. He was accompanied by a film-operator, Richard K. Peck, a former journalist-*cum-*P.R. man, Stanley Hedberg, a Danish-born pilot Hans Hoyte and the airplane mechanic Albert E. Hamer. On the Dutch side there was the botanist, Dr. Willem M. Docters van Leeuwen who, halfway in the expedition, took over Stirling’s leadership, and the ethnographer-cartographer Charles C.F.M. le Roux. There was also the commander of the Dutch-Indies military contingent, Captain R. Posthumus, who was at the head of about 70 soldiers from all corners of the Dutch-Indies archipelago and the Netherlands. Moreover, the expedition relied on the rowing prowess of between 69 and 135 Dayaks from various villages\(^2\) along the upper-reaches of the Kayan-river in the interior of northwestern Borneo. The Dayaks worked on a contract and were paid for their services but the same could not be said of the 140 or so convict laborers who were sent on the expedition from various prisons in Java. The total

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\(^2\) These villages are Long Ledjoe, Long Pleban, Long Isau in the district Boeloengan (Posthumus 1930:483). Le Roux writes the name of one of these villages as Long Peleben (1926c:8, April 12)
number of people fluctuated as soldiers were replaced, sick were sent home, and as more or less forces were needed, but at any time between 295 and 365 people were in New Guinea for this expedition.³ For this reason, the expedition is of further importance for studying the formation of an Indonesian sense of nationhood: by bringing together all these people it raised awareness of their commonality (cf. Anderson 2006, esp. pp.163-185). However, this is another strand of research that I soon hope to pursue again with Dr. Paul Taylor, who brought me to his office at the Smithsonian Institution for exactly this purpose, that is to reconstruct the ‘village ecology’ of the expedition (Taylor 2006a:6)⁴

After the first chapter has prepared the ground by providing a rough history of the expedition, the second and third chapters focus on the ethnographic collection that form such an important part of the expedition’s material heritage. In these chapters, I will discuss two aspects of what may be called the ‘colonial’ chapter in the ‘cultural biography’ of this collection. Chapter two traces the collection’s journey from New Guinea’s jungle and highlands to Amsterdam and other locations, while trying to answer the question of the methodological rationale behind its formation. Among this ethnographic collection I do not just count the ‘classic’ ethnographic artifacts – net-bags, penis-gourds, arrows, fire-making equipment – but also the unique wax cylinder recordings of Papuan music. The following chapter three continues in this vein and provides further background to chapter two by discussing the scientific debates that the expedition and its collections were supposed to contribute to.

³These figures are based on lists in the diaries of Captain Posthumus (Posthumus 1926).
⁴My work on the Dutch expedition diaries with Dr. Paul M. Taylor was aimed at recreating ‘the village ecology’ of the expedition. Much of this research was at the basis of this thesis, but none of it has yet been reworked into the intended essays. Taylor wrote regarding this project (Taylor 2006a:6): “(...) entire major ethnographies have been written about villages smaller than the size of this expedition; and in fact the genesis and composition of the expedition as a village community did substantially affect the development of the scientific questions eventually addressed, and the investigative methods used, by the expedition. The 1926 expedition was a massive undertaking with many understated political ramifications. It is a supreme example of a transnational collaboration that (at least in principle) transcended mercantile, imperial, or colonial interests though it remained always suffused with them.”
The fourth chapter steps away from museum and science for a moment: it can be regarded as a short interlude discussing the problematic case of the film ‘By Aeroplane to Pygmyland’ that was shot during the expedition. This film is unique since it probably is the first moving picture shot in the interior of New Guinea. However, it is problematic for several reasons. First of all, the film cannot simply be seen in the light of the expedition’s scientific goals: thee Americans made this film primarily to help them pay their bills. Moreover, the film was the subject of a controversy between the Dutch and American partners and without further research it is impossible to tell whose version of the film survives. Finally, the chapter serves introduce some of the controversies surrounding colonial heritage that will be the subject of the final two chapters.

The ‘decolonization’ of the former Colonial Museum is at the center of chapter five. In fact, this chapter is about the period after 1950 in the biography of the ‘1926 New Guinea’ collection but by necessity it is more about the *Tropenmuseum* than the collection as the collection lay forgotten for much of this period: only recently did the author find out that it had survived in the attic of the museum. This is only the more extraordinary because the 1926 expedition recently was given a place in the new semi-permanent exhibition ‘Eastward Bound!’ of the museum. However, this representation is limited to fragments of the expedition film that are shown on the wall next to a diorama of Charles le Roux taking a picture of a group of Papuans along the Mamberamo-river: no background information on these images is given.

This exhibition fits within a new mission that former head-curator Susan Legêne has formulated for the museum, which entails a more historical and self-reflexive approach to the ethnographic museum. In my final chapter I will argue how I feel that the current exhibit ‘Eastward Bound!’ does not quite live up to Legêne’s ideas and after having surveyed the biography of the ‘1926 New Guinea’ collection at the *Tropenmuseum*, I will
argue that this collection has great potential for the museum to pursue these goals. For this reason, my conclusions take the form of a set of recommendations to the museum and a tentative plan for further research of the heritage of the 1926 Dutch and American expedition to New Guinea.
CHAPTER ONE

Introducing the 1926 Dutch and American Expedition to New Guinea

Early in January 1926, a group of Americans disembarked at Tandjong Priok, a port in Java near Batavia, capital of the Netherlands-Indies. Their arrival came as a surprise to the colonial government of the Indies: Matthew Williams Stirling, the 29-year old leader of the group, had sent a telegram informing the Dutch colonial government of his plans for an expedition to the interior of Dutch New Guinea but this telegram had lain forgotten ‘under some piece of lead’ in an office of the Government Secretariat at Buitenzorg (Indische Gids 1927b). Now that Stirling was here, though, the government decided it would help Stirling as best as it could.

Stirling’s credentials impressed the Dutch officials: after briefly serving in the US Navy during the First World War, Stirling obtained his BA in anthropology from the University of Berkeley in 1920 and for a year after he taught his alma mater, the academic home of the ‘deacon’ of American anthropology, Alfred Kroeber. After this year, in 1921, Stirling went on to Washington, DC, where he took an MA in the same subject at George Washington University, which he obtained in 1922. In the mean time, he worked as an assistant-curator in the Ethnology Division of the Smithsonian Institution’s US National Museum, a job he quit in 1923. Throughout this time and afterwards, Stirling was particularly interested in archaeology – he participated in several digs, notably in Florida and South Dakota – but he also participated as an ethnologist in the Hamilton-Rice expedition of 1924 to the Peruvian Amazon, where he first saw the use of an airplane for a scientific expedition (Richardson-Fleming 1992; Le Roux 1926a).

Stirling had taken inspiration for his expedition came from a book about the 1910 British Ornithological Union expedition, called ‘Papuans and
Pygmies: the Stone Age Today in Dutch New Guinea” (Wollaston 1912). This expedition had supposedly encountered a tribe of pygmies – called the Tapiro – who no one had visited since. Stirling later summarised his intentions thus: “Fifteen years ago an English anthropological expedition penetrated to the foot of the Nassau Mountains of Dutch New Guinea, and [...] discovered a small group of what were apparently true Negritos. [...] Because of this discovery it appeared likely that in the depths of the rugged Nassau Mountains would be found the home and territory of a Negrito race” (Stirling 1927 in Taylor 2006a:9) This article appeared in ‘California Monthly’, the alumni magazine of the University of California (of which Berkeley is part).

The Governor-General of the Netherlands-Indies, Dirk Fock, saw Stirling’s unexpected arrival primarily as an opportunity for Dutch colonial science to prove itself internationally. He ordered the ‘Indies Committee for Scientific Research’ (Indisch Comité voor Wetenschappelijke Onderzoekingen; henceforth: ‘ICWO’ or ‘Indies Committee’) to make sure that Dutch scientists would have a chance to participate in this expedition (minutes of ICWO-meeting no.64, Nationaal Archief, the Hague). But since Stirling’s telegrams had been disregarded, no preparations for an expedition to New Guinea had been taken. The last expedition to New Guinea, in 1920-21 had taken 5 months to prepare and now a new expedition had to be set up within a month or so (Posthumus 1926a: 275-6). This was an ominous beginning.

When Matthew Stirling arrived in Java his expedition plans were sketchy at best. It appears that he hoped to formulate his final plan based on the maps and local experience of New Guinea exploration that he expected to find in the Netherlands-Indies. Initially, his plan had been to use the small town Merauke as his base and to use his airplane – a hydroplane – to explore the largely unmapped central mountains of Dutch New Guinea from there. In

For some inexplicable reason many Dutch articles – contemporary newspaper articles, but also Imanse (n.d.) – refer to this book as ‘Pupuans and Pygmies’, not spotting the obvious spelling error.
particular, he wanted to explore the mountains that the Brits had been aiming for but failed to penetrate. Stirling then expected to find a mountain lake or river where his airplane could safely land and where his small band of collaborators could set up camp and conduct their research ‘in the field’.

The Dutch soon talked Stirling out of this plan: not only was Merauke some two thousand kilometers away from the intended research area, there was practically no European settlement within a radius of hundreds of kilometers. Moreover, Stirling had brought no replacements for some of the most essential parts of his airplane – notably no spare engine or replacement pontoons – upon whose continued functioning the entire expedition therefore depended. There was also skepticism over the airplane’s carrying capacity and over its ability to fly over the high mountains that were to be found in this area – with the Andes and the Kilimanjaro the only equatorial snowfields. Stirling listened to this advice, a fateful decision that made him entirely dependent on the Army. A good deal of this may be blamed on Stirling’s inexperience of what it takes to organize an expedition. According to the ICWO’s first draft expedition plan, Stirling effectively gave up responsibility for the logistical aspects of the expedition at the first major meetings:

He [Stirling, MM] asked the Indies Committee for Scientific Research [ICWO, MM] to prepare for him, in the spirit of cooperation, a plan that was practicable and to take upon itself the discussion and the preparation of this plan with the authorities and experts. In the meantime, Mr. Stirling would occupy himself with studying the literature that he had been provided with. (le Roux 1926a:7; my translation)

For a while Stirling retained nominal leadership over the expedition, but he would eventually be forced to abandon it.

Since Stirling walked away from the logistical aspects of organizing the expedition, this task was entrusted to the Charles le Roux, a former Army surveyor, 41 years of age, who had switched careers to become assistant-
curator at the museum of the Royal Batavia Society (Koninklijk Bataviaasch Genootschap) and secretary to the ICWO. Le Roux was a man with great scientific ambitions but with no university background (Imanse, n.d.) and this was the chance of his lifetime. Keeping in mind the Governor-General’s exhortations to ‘make sure Dutch science participate in this expedition’ and to ‘keep it small’, only two Dutch scientists participated in the expedition. One of them was the botanist Dr. Willem Marius Docters van Leeuwen, director of the Botanical Gardens at Buitenzorg (currently ‘Bogor’) and then acting-chair of the ICWO. Charles le Roux himself filled the second position: covering the two tasks of ethnographer and cartographer for the Dutch scientific contingent. Ethnography was his passion, but cartography was essential to pacify the Army’s demand that the expedition provide up-to-date maps of this huge, little-explored island. No outside scientists were invited to join the expedition.

Figure 1 Charles le Roux, Dr. Willem Docters van Leeuwen, and Matthew Stirling in front of the 'Ern', the expedition's airplane. Picture ID W086 (Taylor 2006)
Keeping the Army pacified was essential for the success of the expedition. In the period between 1903 and 1912 several privately financed expeditions had gone to Dutch New Guinea, but after 1907 the KNIL created a Military Exploration Detachment (*Militair Exploratie Detachement*; henceforth MED) that took upon itself to map and explore the entire island. The First World War forced the MED to prematurely abandon this immense task – prior to 1907 practically no maps of the interior existed – and by 1915 the last soldier-explorers left the island: the reports were published in 1920 (MED 1920).

Le Roux worked for the ICWO that had been created in 1888 to bring together government and private interests in science in the Dutch East Indies and this *Indies Committee* as it was commonly called had played an important role in all but the very first major New Guinea expeditions. The 1903 Wichmann or North New Guinea expedition and the 1904-05 Royal Dutch Geographical Society (*Koninklijk Nederlands Aardrijkskundige Genootschap*; KNAG) or South-West New Guinea expedition had not involved the ICWO. The failures through disastrous infighting of the latter (e.g. KNAG 1908; de Rochemont 1909) had resulted in the ICWO’s central role in further expeditions and after 1907 it received a regular government budget of £20,000 for research. Hendrik Lorentz’s 1907 and 1910 expeditions were the first to profit of these new arrangements. The first expeditions had been primarily coastal expeditions and did not rely on Army infrastructure as heavily: it been primarily the Royal Mailboat Company (*Koninklijke Pakketvaart Maatschappij*; KPM) that carried the supplies. But as expeditions came to penetrate the dense jungle and swamps of lowland New Guinea, an Army cover and logistical experience became invaluable. On his expeditions Lorentz had already run into some trouble with the Army: not trusting his cartographic observations, the MED redid Lorentz’s trajectory immediately after the latter’s successful but exhausted return from Mt. Wilhelmina’s snowfields (Lorentz 1913; Troelstra 2005).
Equatorial snowfields and cartography can be seen as the two primary obsessions that drove exploration in New Guinea at this time; indeed, it has been called the ‘race to the snow’ (Ballard, Vink and Ploeg 2001). Reaching the snow-mountains had driven all but the very first scientific New Guinea expedition. In 1903, the Utrecht University professor Wichmann was more interested in finding coal deposits, in 1904-05 the Royal Dutch Geographical Society aimed for the snow but ended up exploring the Etna Bay and the Digóel-river (KNAG 1908). In 1907 Hendrik Lorentz had the same goal but he, too, failed and when he tried again in 1910 he ran against a British expedition, the British Ornithological Union (BOU) expedition (Wollaston 1912; Rawling 1913). Lorentz won and the BOU expedition was ‘a spectacular failure’ (Ballard 2000:137) with an important unintended consequence. The discovery of the ‘Tapiro-pygmy’ brought the unexplored interior of Dutch New Guinea to the attention of the Cambridge professor of anthropology Alfred Cort Haddon (Haddon 1910, 1912) and through him to the attention of the wider scientific community.

Unfortunately for scientists interested in pursuing this new research agenda – more about scientific interest in pygmies below and in the next chapter – the KNIL remained more interested in snowfields and maps. Even Alexander Wollaston, who was the medical officer of the BOU expedition and who took the measurements that placed the Tapiro among the pygmies, considered himself primarily a mountaineer. He returned in 1913, still focused on reaching the snow. In that year he raced against the Dutch Army officer Franssen-Heidrich für for reaching the top of snow-mountain Mt. Wilhelmina – Lorentz had only reached the periphery of the snow-fields – and in a great British tradition, Wollaston failed again. After the First World War, the KNIL was intent on resuming the exploration of the New Guinea, in particular the central mountain ranges that had been neglected before the war. In 1920 and 1921, two consecutive expeditions were set up – the Central New Guinea Expedition – that aimed to be the first to reach Mt. Wilhelmina.
from the north; again the snowfields were the primary impetus. However, this expedition also brought along the medical officer Hendrik Bijlmer on the first leg of 1920 and the Swiss anthropologist Paul Wirz on the second leg of 1921. They were intent on researching these mountain Papuans and Bijlmer in particular expected them to be related to the Tapiro-pygmies. The next chapter will be discuss these issues further, but what is of present interest is that research of New Guinea’s supposed pygmy populations had so far been impossible without the support of the KNIL.

Matthew Stirling proposed to pursue this research agenda with an airplane – a plan first proposed in 1917 by the Swede Mjöberg (Mjöberg 1917), unbeknownst to Stirling – and thus dispensing of the Army’s long arm. But as Stirling’s aeronautic plan proved to be shaky at best, the expedition was driven back in the arms of the Army. The Army could now make demands, primary of which was pursuing the old program of mapping Dutch New Guinea. Le Roux had to concede to this and ‘sold’ his inclusion in the expedition on the ground of his surveying background, but as his real interest lay close to Stirling’s in ethnography this was a recipe for disaster. In a certain sense, this expedition was unable to escape the past of military-driven expeditions, although the airplane had promised a sense of freedom that le Roux and Stirling longed for.

Le Roux’s job was to coordinate planning, in the name of Stirling and the ICWO and according to ICWO tradition he had to involve the Dutch-Indies Army. As I have made clear, the KNIL had controlled most New Guinea expeditions, because it was the only institution able to provide the enormous logistical infrastructure needed to travel into New Guinea’s inaccessible interior. Le Roux was probably infected by Stirling’s enthusiasm over the use of the airplane in New Guinea’s exploration and he may have hoped that the American airplane would allow him to reduce the Army’s control over the scientific exploration of New Guinea. Instead of Stirling’s initial plan to use Merauke as his flying base and approach the BOU
expedition terrain from the South, the expedition would now use a northern approach of the central mountains via the Mamberamo and Rouffaer rivers. It was hoped that these rivers would provide excellent landing opportunities for the airplane. Moreover, this allowed the Army to map and explore a second point of entry into the highlands – the first point of entry had been explored in 1920-21.

The Army’s pessimistic assessment of the American airplane (Posthumus 1926a, 1930) turned out to be correct: the Americans had brought no replacements for their hydroplane’s motor and pontoons and both caused trouble in the jungle. The motor malfunctioned from the very beginning and in July 1926 one of the pontoons was irreparably damaged while landing in the Mamberamo-river. The airplane had to be abandoned for good and the KNIL was once again in full control of the expedition. What was initially known as the ‘Stirling Expedition’ and what had already turned into the ‘American and Dutch Expedition to New Guinea’, now became the ‘Dutch and American Expedition.’ At Stirling’s request, the ICWO replaced him as the nominal leader of the expedition and appointed Dr. Docters van Leeuwen, the most senior scientist. Although Stirling requested this himself he greatly regretted it, seeing it as the result of ‘blackmail’ on the part of the Dutch (Stirling in Taylor 2006a: footnote 1). Moreover, the Americans did not like Docters van Leeuwen’s character – he was known to be difficult (according to his grandson Buul Docters van Leeuwen, pers.com.). However, from Dr. Willem Docters van Leeuwen’s diary, the change of leadership merely seems a formalization of an already extant situation in which Stirling’s decision to entrust the logistics of the expedition to the ICWO and KNIL plus language barriers between Americans and Dutch Army personnel resulted in a situation in which Stirling had virtually no say in the day-to-day proceedings of the expedition.
The Army gained greater control over the expedition now the airplane had died. It turned out to be very fortunate that Posthumus had insisted that the airplane be accompanied by what Le Roux probably mainly saw as a ‘backup’. This backup was in fact what may be called an expedition of the ‘classic’ New Guinea type, which Lorentz had pioneered and that relied heavily on paid Dayak from Borneo to row transport canoes over the rapids in the New Guinea river and Indonesian convict laborers and cariers. The death of the American airplane meant that this ‘backup’ infrastructure had to be expanded to full strength – at great cost. This brought a simmering tension between ICWO-planner le Roux and his KNIL-liaison Captain R. Posthumus to the fore. In the expedition’s planning phase they had been under immense time-pressure – 4 weeks to organize this expedition vs. 5 months for the last – and this meant there was no time for experimentation on the old expedition-model. However, le Roux seems to have insisted on using the
American airplane to save on carrier costs, which in turn appears to have annoyed Posthumus immensely. As a result of this, when the airplane broke down, Captain Posthumus felt ‘betrayed’ by the scientists and le Roux in particular (e.g. Docters van Leeuwen, 1926b, June 10 and October 6).

The Americans were no longer on speaking terms with anyone (except le Roux, to a degree) and Posthumus was fuming. Docters van Leeuwen had an unenviable task. He had no way to sway the Army leader, who now seemed to do as he pleased within the scope of his orders, which was to bring Matthew Stirling safely to the central highlands. Posthumus’ annoyance was only exacerbated because Docters van Leeuwen appeared to have no idea of the vital importance of logistic decision in a jungle expedition. He in particular made irrational requests and interfered with Posthumus’ decisions. For example the botanist brought so much botanical equipment – to the point that some returned unused – and insisted that it be brought up, that some camps had had to ration their food-supplies (Posthumus 1926b, November 2). Apart from his sense that le Roux had betrayed the government by his rash insistence on giving the airplane a central position in the expedition, Posthumus was further annoyed that le Roux did not spend enough time on cartography. The latter had delegated this task to his assistant, a Minangkabau from Sumatra, Mohamad Saleh, and instead spent most of his time on ethnography (Posthumus 1926b, September 24 and October 16 and 22; Docters van Leeuwen, 1926b, October 26).

The 1926 Dutch and American Expedition to New Guinea was not to have a position of honor in the history of colonial expeditions. In accounts of New Guinea’s history it is usually only briefly mentioned, if at all (Souter 1962; Vlasblom 2004). First of all, little was published on this expedition – but this may well have been because its experience was unpleasant for all involved. The personalities clashed – Docters van Leeuwen had a difficult character and made strange requests, Posthumus does not seem to have been
very cooperative to the demands and wishes of the scientists. But perhaps an even greater part of this can be blamed on the inexperience of many involved – the organizers Charles le Roux and Matthew Stirling in particular – an issue that was only exacerbated by the time-pressure under which the expedition had to be organized. This in its turn was due to the Dutch-Indies government’s failure to act on Stirling’s telegrams that as we saw were reported to have lain ‘forgotten under some piece of lead’ in a Buitenzorg office (de Indische Gids 1927b). When the latter came out, the Governor-General quickly issued a statement denying it had happened so and it appears that the ICWO bore the brunt of the blame.

Already during the expedition there had been much bad press in the Netherlands-Indies about the expedition. There had been speculation about why the airplane was primarily used for transport rather than exploration – a field in which it would be much more promising (Javabode 1926, August 21 and October 12; cf. Mjöberg 1917). Moreover, in December reports appeared that the expedition had exceeded its budget by several magnitudes (Locomotief 1926, December 13; De Indische Gids 1927a-b), although an exact assessment cannot currently be had. Moreover, at the news of Matthew Stirling’s replacement as leader of the expedition the Netherlands-Indies press speculated over infighting and the expedition’s imminent failure (e.g. Javabode 1926, August 21 and October 12).

Still, this expedition represents a turning point in the history of New Guinea’s exploration. The role of the ICWO in the exploration of New Guinea ended in 1926. The memory of the perceived fiasco of 1926 haunted the Indies Committee and the exploration of New Guinea for years to come; although the financial crisis of 1929 presumably was of little help to the latter. Major expeditions to unexplored parts of New Guinea were only in the late 1930’s organized, although these made extensive use of (vastly improved) airplane technology. Assessing the impact of ‘1926’ on the role the Army played in these expeditions would need further research also, but it appears that their
role, too, was minimized. In the 1930’s followed a number of what may be called the final ‘great’ expeditions to New Guinea’s highland. It was only after these that the map of the Dutch half of the island was more or less complete. However, these expedition were not under the primary leadership of the Dutch-Indies Army. One of these expedition was again initiated by an American: in 1938 the millionaire Richard Archbold led an expedition to the great Baliem Valley, supported by the American Museum of Natural History in New York (Archbold, Rand and Brass 1942). And in 1939, Charles le Roux himself – by this time in his capacity of curator at the National Museum of Ethnology in Leiden – led the other expedition, exploring the valleys around the Wissel (Paniai) Lakes, under the aegis of the Royal Dutch Geographical Society (le Roux 1948-50).

There may be another reason why this 1926 expedition has been all but forgotten. Its way of ‘doing’ science, particularly that of Stirling and le Roux, seems utterly strange from a contemporary perspective. In a sense it is a discredited form of science (see also chapter 4). In fact, this process of discredit may already have been under way by 1926 and Matthew Stirling’s reluctance to publish his results may – or may not – be indicative of this. The scientific methods of the 1926 New Guinea expedition will be the subject of the next chapter.

Figure 3 Dead tree in the Mamberamo while a convoy of the expedition passes
Arb153 (Taylor 2006)
CHAPTER TWO

Formation of a Museum Collection

After introducing the expedition as a whole this chapter discusses the museum collection that is at the heart of this paper. Matthew Stirling and Charles le Roux had joined forces in collecting ethnographic artifacts. Throughout the expedition both men amicably divided among themselves whatever they had collected. Before setting off, Stirling had promised the first choice of his collection to the Smithsonian Institution’s US National Museum. Le Roux’s main collection went to the Museum of the Royal Batavia Society, currently the Museum Nasional in Jakarta. As was customary, Stirling and Le Roux also collected many duplicates for colleagues. It is unclear how many duplicate collections Stirling made and what he did with them. Only some objects associated with Stirling’s New Guinea expedition have been located at the Phoebe Hearst Museum of Anthropology of the University of Berkeley (Paul M. Taylor, pers.com). Le Roux’s duplicate collections are another story.

In November 2006, the International Institute for Asian Studies (IIAS) at the university of Leiden and the Asian Cultural History Program at the Smithsonian Institution organized a symposium at the Dutch National Museum of Ethnology under the name “By Aeroplane to Pygmyland: revisiting the 1926 Dutch and American expedition to New Guinea”. At this occasion, the director of said museum, Steven Engelsman, claimed that most of the Dutch collections, whose exact fate remained unknown, were kept at his museum. He based this assumption on the fact that le Roux had been curator and later director at his museum. Kees van den Meiracker – curator for Oceania at the Wereldmuseum in Rotterdam and one of the speakers at this symposium – had located collections from this expedition at his museum and museums in Leiden and Hamburg. People at the Tropenmuseum in Amsterdam had not been unable to locate any collections related to the
expedition. However, Engelsman and Van den Meiracker forgot that Le Roux after his return from the Dutch-Indies first worked at the Colonial Museum in the period 1934-35 and Le Roux had strong ties to that institute before his appointment in Leiden.

Figure 4 Objects from the Stirling New Guinea Collection in storage at the storage facility of the Smithsonian National Museum of Natural History in Suitland, MD. Photo by the author

Archival research has now shown that Le Roux first sent all his duplicates to the museum of the Colonial Institute in Amsterdam. The reason that initially this collection could not be found was that it had been accessioned – as collection 514 – with ‘Indisch Comité voor Wetenschappelijke Onderzoekingen’ (ICWO) listed as the donor, rather than C.C.F.M le Roux. Moreover, after receipt of the bulk of Le Roux artifacts, it appears that Amsterdam served as a transportation hub for sending further duplicates to other museums: Leyden (Dutch National Museum of
Ethnology), Rotterdam, Hamburg (Museum für Völkerkunde), Paris (Musée de l'Homme), London, and Rome (Museo Missionario-Etnologico) all received collections.\textsuperscript{6} It is safe to assume that the Colonial Institute had the second pick from the duplicates before any of the other museums.

The collection, moreover, played a role of some importance in the history of the Colonial Museum and its arrival was highly anticipated. After all, it was one of the first substantial ethnographic collections to come from the unknown New Guinea Highlands. In 1926 the museum had just moved into a new, purpose-built housing and it was in the process of (re-)organizing its collections and exhibitions. On December 2, 1926, an unknown curator at the museum wrote to le Roux – who was still on his way back from New Guinea – to say that they waited for the receipt of his collection before putting together the new exhibition on Dutch New Guinea.\textsuperscript{7} When it finally arrived in the spring/summer of 1927 it was first put on display as a special exhibition in the new museum’s magnificent central Lichthal (Hall of Light) (NRC 1927, June 16).

Both Stirling and le Roux received great praise for their collecting work: the yearly report of the Smithsonian Institution dedicated nearly two pages to Stirling’s expedition and writes: “thanks to the generosity of Mr. Stirling and his companions, the National Museum now possesses one of the finest collections of the kind from New Guinea in existence.” (Smithsonian 1927:10). Similarly, Charles le Roux received the silver medal from the Colonial Institute in Amsterdam for his efforts.\textsuperscript{8}

Having thus established the locations of the ethnographic collection, a closer inspection of its formation and constitution will be the subject of the

\textsuperscript{6} Letter by the general secretary of the Colonial Institute to the secretary of the ICWO (i.e. Charles le Roux), dated 3 March 1928: file 7494, archives of the KIT, Amsterdam. I assume that the museum in Rome is the Museo Missionario-Etnologico, as it opened in 1926 and because its first director, father Wilhelm Schmidt S.V.D., was a famous anthropologist who had written a major work on pygmies (Schmidt 1910)

\textsuperscript{7} Letter by anon. to Charles le Roux, dated 2 December, 1926: file 7494, archives of the KIT, Amsterdam

\textsuperscript{8} Letter by the Raad van Beheer to the Raad van Bestuur, dated 21 May 1927: file 7494, archives of the KIT, Amsterdam
rest of this chapter. For both Matthew Stirling and Charles le Roux, the natural sciences provided the primary model for their scientific quest. In the following section, I will therefore draw extensive comparisons between what the botanist Dr. Willem Docters van Leeuwen was doing and what occupied Stirling and Le Roux.

**The Natural Science Paradigm**

Collecting and observing were Docters van Leeuwen’s central scientific activities in New Guinea. Moreover, he knew that there was too much material for any one person to ever process in an unknown field like this and therefore he collected specimens for later study at home. With the aid of an Indonesian assistant – a so-called *mantri* – Docters van Leeuwen collected plants, samples of wood, flowers, leaves, also some insects, and whatever else of the natural surroundings that struck his fancy. While still in the field and in a lull of the expedition’s activities, he wrote and published an article about the natural environment of *Albatrosbivak* (1926a), but mostly he gathered collections for further research in the quiet space of the *Herbarium* that was part of his Botanical Gardens. He regularly complained in his diary about the fact that his leadership of the expedition kept him from doing his work (*e.g.* Docters van Leeuwen 1926b, May 16 and June 4), but he managed to gather an impressive amount of data and specimens (Ed de Vogel, *Rijksherbarium*, pers.com.).

Dr. Docters van Leeuwen did not just collect for himself. He tried to obtain plenty of duplicates of specimens, which upon return to Java were bundled in sub-collections and sent to other *herbaria* all over the world. On the one hand this was a kind of ritual exchange: by giving unique gifts, the giver’s status is enhanced (Mauss 1954). But there was also an expectation to receive duplicates from other collections in return, which served a practical purpose: the collector could never process all specimens himself. At these research institutes, the collected plants were inspected, described and
classified according to Carl Linnaeus’ system of plant taxonomy. Docters van Leeuwen never described any of his New Guinea collections himself; I know of only one who did and this was his colleague and predecessor as director of the Buitenzorg Botanical Gardens, Johannes Smith (Smith 1935). Smith, however, focused primarily on orchids and other parts of Docters van Leeuwen’s botanical collection remain un-described to this day.

This system of describing and classifying was primarily based on morphological differences among the plants: that is, observable differences in structure and development. As we shall see, the work of Matthew Stirling and Charles le Roux followed similar descriptive and classifying methods. But whereas nowadays – despite the revolution genetic analysis has unleashed in the system of plant taxonomy – Docters van Leeuwen’s botanical work is still recognizable and very valuable for botanists, the same cannot necessarily be said of Stirling and Le Roux.

Up until the 1920’s, but less and less afterward, anthropologists adhered strictly to the example of the natural sciences, such as botany: i.e. to collect in the field, and to classify and process data in the laboratory. Like Docters van Leeuwen, Stirling and Le Roux collected specimens and data for analysis elsewhere. Instead of a herbarium or a botanical garden this ‘elsewhere’ was usually an ethnographic museum or a university. Instead of plants and observations about their habitat, they collected anthropometric measurements, ethnographic artifacts, and human behavioral observations. The recording of Papuan music must also be seen in this light. The rationale behind all this collecting will be the subject of the next chapter; first, I will elaborate on the various collecting strategies.

**Anthropometry or physical anthropology**

Matthew Stirling was particularly interested in anthropometry or physical anthropology: the measuring of human bodies. This closely resembled the botanist’s morphological analysis, however, rather than
classifying plants species and genera, the goal here was determining the human various ‘races’, or the creation of a human racial taxonomy that would shed light on human evolution (see also chapter three). Stirling was primarily interested in finding a race of ‘pure negritos’ or ‘pygmies’ and physical length was supposed to be primary characteristic of this race. Anthropometry was Matthew Stirling’s exclusive terrain: there is no indication that Charles le Roux was also involved in it, although we will see his work was also related to this project. Wherever the expedition encountered natives, Stirling tried to take anthropometric measurements: length, size and shape of the skull, color and shape of the hair, the eyes, etc.

Figure 5 Matthew Stirling taking anthropometric measurements on a highland Papuan woman C022 (Taylor 2006)
Measuring was one thing; another was collecting specimens for further measurement and analysis in the laboratory. However, unlike plants it rather harder to bring human to a laboratory setting and so anthropologist usually skulls or skeletons were gathered. This also allowed for comparison of contemporary populations with fossilized remains of early humans (cf. Kleiweg de Zwaan 1923:23). It is unclear whether, or to what extent Matthew Stirling was involved in this. The collection of bones and skulls was certainly an established aspect of expeditions like this. This is well illustrated by a newspaper article, putatively summarizing Stirling’s intentions in New Guinea as ‘to find dwarves, measure them, film and photograph them, take some bones and skulls and whatever curious is found along the way, and then go home’ (Locomotief 1926, December 13, my translation, my emphasis). That Stirling at least considered collecting human remains is tantalizingly, but inconclusively sugested by a passage in Le Roux’s expedition diary. At this point in the expedition, there had been a violent encounter with a group of Papuans at a transition camp in the Meervlakte (Lake Plain) area. Some days before Stirling and le Roux arrive at this camp, soldiers guarding a transport of supplies shot a Papuan dead. Le Roux writes:

Outside the fence we were shown the grave of the killed Papuan, whose skull had been half blown away. Prof. Stirling will take care of the skeleton and in due time he will find a place for it in the National Museum in Washington. (Le Roux 1926c:30, my trans.)

However, this is the only mention in any of the expedition diaries about collecting human remains and Stirling himself never mentions taking a body to Washington. Given the sensitivity of these issues, I have not been able to prove nor to disprove that Stirling actually brought the body to the Smithsonian Institution. Indeed, several human mandibles are in the Washington collection, but they have been accessioned in the Stirling collection, but these are part of the ethnographic and not the anthropometric collections.
Ethnography or cultural anthropology

Whereas Stirling is usually called ‘anthropologist’ in contemporary sources, Charles le Roux is often called ‘ethnographer’ or ‘ethnologist’. This reflects the different task each had within the expedition: Stirling practiced physical anthropology and Le Roux was concerned with the cultural and social aspects. Le Roux himself preferred to be called ‘ethnographer’ (Fischer 1950) as this implied a descriptive rather than theoretical activity. This was a division of labor common in anthropology up until this time, but that unfortunately for Le Roux was quickly going out of fashion. Ethnologists – somewhat disparagingly known as ‘armchair anthropologists’ to later generations – commonly made use of a network of informants, the ‘ethnographers’, who often were colonial officers or missionaries active in the area that interested the ethnologist. The ethnographers supplied the raw ethnographic data that the ethnologists then processed into ethnological theories. This division of labor must have felt comfortable to the ‘amateur ethnologist’ Le Roux (Imanse, n.d.) who lacked a university education.

Although they had different tasks, Le Roux’s activities contributed to the same goal as Stirling’s. They believed that there was a direct link between race (Stirling) and culture or society (Le Roux). Because of his theoretical insecurity Le Roux accepted the value of anthropometric measurements, but discussions of race do not seem to have been his primary interest. He was primarily interested in all aspects of the lives of the people he encountered and whenever Posthumus allowed him to pursue his interests he could be found trying to talk with the Papuans: making word lists and taking notes of their behavior. His interest covered a wide range of subjects: marriage, religion, warfare, agriculture etc. His was an extremely challenging task, as the expedition encountered many Papuans in a ‘first contact’ situation: neither side knew the first thing about the other and communication was extremely difficult. Le Roux’s did the best he could, but
this meant that much of the observations he recorded are at least partially interpretations of what must have been utterly foreign and therefore there is an inherent unreliability in his work. Still, his subsequent accounts of life among the ‘Mountain Papuans’ as he came to call them, provides an important first glimpse of the life of these people: a life that probably about to undergo rapid change, although since the area was never brought under effective Dutch control it is extremely hard to tell what has happened to them since.

After his return to Java, le Roux worked hard to write up his findings but with his many other activities that took precedence, this took a long time. For years after 1926, there are references in the minutes of the meetings of the ICWO to the need of Le Roux completing his book – this book was also supposed to ‘counter’ the half-truths spread by the Americans about this expedition. Although Le Roux worked on and nearly completed a popular book about this expedition called ‘Mamberamo’, it was never published. Eventually, Le Roux combined his data from 1926 with newer, more extensive data from the 1939 KNAG expedition to the Wissel Lakes. Together, this was published in a large 3-volume book called ‘De Bergpapua’s van Nieuw Guinea en hun woongebied’ (‘The Mountain Papuans of New Guinea and their Territory’; le Roux 1948-50). Unfortunately, Charles le Roux died in 1947 just short of finishing this work, too. With the aid of his

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9 In January 1928, the ICWO wrote a letter to the Dutch Consul in San Fransisco to ‘sternly warn’ Mr. Stirling of ‘the dangers if he continues to advertise thus this expedition in America and elsewhere’. This ‘thus’ is not elaborated upon, but the misrepresentation hinted at may have to do with disparaging Dutch exploration efforts. The ICWO and Dutch government were interested in showing that the Dutch had explored quite a lot of their New Guinean possessions, Stirling on the other hand was interested in aggrandizing his own efforts by underlining unexplored New Guinea (see also chapter four); Minutes of meeting 71 of the ICWO (January 16, 1928), file 2, archives of the ICWO, Nationaal Archief, The Hague; my translation

10 Steven Vink senior research of the Photographic Collections at the Royal Tropical Institute found the manuscript of this work in the archives of the Royal Dutch Geographic Society (KNAG). The minutes of meeting 79 of the ICWO (November 30, 1930) mentions that Le Roux showed this manuscript – including pictures by the famous Indonesian illustrator Mas Pirngadi – but also states that the text needs a final revision. Presumably this never happened. Minutes of meeting 79 of the ICWO (November 30, 1930), file 3, archives of the ICWO, Nationaal Archief, The Hague
son-in-law, the text was given a final polish and the book appeared posthumously.

**Collecting Music: Ethnomusicology**

A special category of Le Roux’s ethnographic work was his collection of Papuan music, which he made for his friend, the pioneering ethnomusicologist Jaap Kunst. Basically, Kunst was interested in music as a particular way of approaching the issue of evolutionism that also preoccupied ethnology at this time. In the early days of the expedition, Le Roux made approximately eight recordings of the music of the lowland Papuans. However, since he was unable to haul his phonograph up into the mountains, he and his assistant Mohamad Saleh memorized simple melodies they heard in the highlands. Upon their return to Java, le Roux gave these recordings to Jaap Kunst and at a later date he hummed – accompanied by his assistant Mohamad Saleh on violin – several of the highland melodies (Kunst 1931). Kunst’s publication on this material is of particular interest as it is the only extensive contemporary publication relating to any of the expedition’s material: Stirling never published a thing and Le Roux only much later. For this reason, Kunst’s work provides a glimpse of the scientific questions that drove these gentlemen, but a closer discussion of this project will have to wait for the next chapter.

Kunst retired from Dutch-Indies government service in 1934 and returned to the Netherlands, taking Le Roux’s recordings with him. He soon found himself a position at the Colonial Institute and eventually he was also appointed to the University of Amsterdam, where he founded the ethnomusicology department. Jaap Kunst died in 1960; his ethnomusicological collection remained at the *Tropenmuseum* but in the

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11 See also: letter by Jaap Kunst to Charles le Roux, 29 February 1929, *Archives of mr. Jaap Kunst*, special collections, Library of the University of Amsterdam (UvA)
1970’s, they were donated to the University of Amsterdam (van Proosdij-ten Have and van Roon 1992).

_Collecting Material Culture: Ethnography Materialized_

Matthew Stirling and Charles le Roux imitated Dr. Docters van Leeuwen in every way. The latter made extensive notes on the local flora in his diary and articles (Docters van Leeuwen 1926a-b), and Stirling and le Roux collected similar observations. Like Docters van Leeuwen, they made extensive photographs of their research subjects, and, like Docters van Leeuwen, they tried to collect a complete overview of available specimens.

Before I discuss the rationale behind all this in the next chapter, I briefly want to discuss Stirling’s and Le Roux’s joint ethnographic collecting effort. Their objective was to collect the ‘material culture’ of the Papuans (le Roux also speaks of _cultuurbezit_ in Dutch): a collection that, in the absence of Papuans to question about his culture, could be mined for characteristics about this culture, particularly relations among different tribes, peoples, races that would come to light by comparing the objects they use daily.

The main objective of collecting was completeness: the expedition literally gathered whatever they could get from the Papuans. The catalogue for the Stirling collection at the Smithsonian National Museum of Natural History – the only non-duplicate collection I had access to – holds as many as 1305 entries, quite a number of which contain several objects: _e.g._ item E338252-0 ‘ca. 15 headdresses’; item E338693-0 ’18 woven bags’; item E338439 ’95 decorated arrow shafts’). It seems a fair estimate that the entire collection contains approximately 1500 items. On the other hand, it is fairly certain that the Colonial Institute sent 322 items to Hamburg: stereotypically the _Museum für Völkerkunde_ gave every one of the 134 arrows its own entry.\(^\text{12}\)

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\(^{12}\) Thanks to Ms. Jeanne Kokott of the _Museum für Völkerkunde_ in Hamburg for providing me with an inventory of their collection.
Figure 6 Matthew Stirling and Charles le Roux inspect ethnographic artifacts at Motor Camp, Rouffaer River. Y016 (Taylor 2006)
An interesting insight in these collections is further provided by annotated list (Le Roux 1927) of the duplicate collection that was exhibited at the Colonial Museum in 1927. Oddly enough, this list could not be found in Amsterdam but among the papers related to collection 2120 of the National Museum of Ethnology in Leiden, I located two copies: one fair with beautiful sketches of the objects in the margins, and another plain with a couple of handwritten notes. This list is not only valuable for the (sketchy) ethnographic background it provides to the objects but also in another way. The list may only have survived in Leiden, but from the accompanying letters it is clear that it was written with the Amsterdam collection in mind.

When one compares this list with the up-to-date collection inventory of collection 514 at the KIT – 316 entries – Le Roux’s list – 114 entries – proves also revealing of Amsterdam collection policy. It is not apparent from the discrepancy in the number of entries, but some of Le Roux’s items could not be located. When I asked curator David van Duuren about this, his first idea was that these items must have been deaccessioned because of damages, but apparently they were never accessioned at all. This was possible, because an entry in the yearbook for 1927 of the Colonial Institute makes clear that the collection was only accessioned after the Light Hall exhibition. It seems that the Colonial Institute did not accession some of these items because it did not consider these ‘true’ ethnographic specimens; although Le Roux, who collected them, obviously did. In other words, here’s an interesting conflict. The missing pieces are items number 1 to 4, and 73, different types of unadorned ‘magical stones’ – item 1 is an ammonite fossil – item 75, a human mandible, and item 99, an unspecified number of ‘pieces of metal and tin’ that Le Roux describes as ‘probably remnants of exchange goods of the 1914 [MED, ‘MM] expedition, and associated wooden handles’13 (Le Roux 1927).

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13 Le Roux originally wrote: ‘Stukjes ijzer en blik, waarschijnlijk overblijfselen van ruilartikelen der expeditie 1914, en daarbij behorende houten handvaten’ (Le Roux 1927, item 99)
The absence of the first items can probably be explained from the fact that the stones were unadorned and therefore they were nor pure artifacts – *lit.* ‘made by art.’ Hence, these objects were automatically classified as *naturalia* and of no interest to the ethnologist or the museum. The human mandible may have been moved from the Colonial Institute’s ethnology department to the physical anthropology department, although interestingly the Smithsonian collection did accession human mandibles into its ethnographic collection. However, from the perspective of contemporary ethnography and museology the loss of the last item – the pieces of metal and tin – is particularly to be regretted. Le Roux calls them ‘exchange goods’, but it may equally be possible that they were pieces of expedition garbage. Docters van Leeuwen describes how a tin that is thrown over board is quickly spotted and picked up by Papuans living along the Mamberamo (Docters van Leeuwen 1926b, April 29) and from other expeditions it is known that the Papuans – like natives unfamiliar with metal anywhere – were quick to appreciate the value of metal. The most interesting thing about these objects, however, is that they may well have been pieces of metal and tin that Papuans had shaped into tools themselves. Such items were not only harbingers of the changes that were to come to Papuan society but also testimony to the resourcefulness of these supposedly primitive people. In this regard, it is significant that Charles le Roux saw enough ethnographic value in these items to not only collect them but also display them in his exhibition. However, Le Roux was not the only gatekeeper to the Colonial Museum’s ethnographic collection. Someone along the line must have decided not to accession these objects and may have done so because they did not fit the kind of discourse that was expected. After all, the people this expedition had gone and found were *pure* Stone Age Papuans and Stone Age people do not use metals. From this, it appears that Papuans were being defined by what they were expected to be, and not by what they were found to be. Metal items
subverted their ‘Stone Age’ quality and for this reason the objects had to be discarded.

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**Figure 7** A page from Le Roux’s 1927 illustrated list of ethnographic objects in his collection. (Le Roux 1927, archives of the National Museum of Ethnology, Leiden, the Netherlands)
CHAPTER THREE

Evolutionist Anthropology and Racial Taxonomy:
the Problem of Alterity

We have seen that the goal of this expedition was to find the supposed homeland of a race of ‘pure negritos’ that was expected to have its ‘home land’ in the interior highlands of New Guinea. The last chapter has showed the link between Stirling’s anthropology, Le Roux’s ethnography, and the Natural Sciences-inspired project of racial taxonomy that was at the heart of anthropology/ethnology in this period. To achieve a more complete understanding of the 1926 expedition’s heritage, this chapter will explore the history and fate of the scientific project that it was a part of.

Matthew Stirling had speculated that ‘in the depths of the rugged Nassau Mountains would be found the home and territory of a Negrito race’ (Stirling 1927 in Taylor 2006a:9). This term ‘negrito’ was a technical term for a subgroup of the pygmy-race. Stirling wrote that ‘the distribution and relationship of Negrito tribes has, for a long time, been a problem of interest to anthropologists’ (again in Taylor 2006a:9), particularly, he hoped to help settle what the famous Cambridge anthropologist Alfred Cort Haddon had called ‘the Pygmy Question’ in an appendix to Wollaston’s book (Haddon 1912; cf. Haddon 1910). However, before we get to that it is necessary to step back and take a look at the broader project of racial taxonomy.

The Problem of Alterity

European scientists had been speculating about human races for centuries. Since the late 18th century, this debate focused on the question of (intellectual) equality of humans (Douglas 2008a), but in a modified form it goes back much further: since the 15th century Spaniards and Portuguese had argued over whether Africans and Indians had souls or not and therefore
whether they could be saved. In the 1730’s the Swede Carl Linnaeus published his *Systema Naturae* that streamlined an already extant system of classification of plants and animals, including humans. Linnaeus postulated a unified human species (*homo sapiens*) within the animal kingdom, but he also made it possible to speculate over innate differences among – different races of – humans. The issue of the unity of humankind and its inclusion in the animal kingdom was an issue of debate throughout the 18th (and 19th and 20th) century. Yet toward the end of that century the German Johann Blumenbach first proposed the five different ‘races’ that are still commonly in use today: he distinguished the so-called ‘Caucasian’, ‘Mongolian’, ‘Ethiopian’, ‘American’, and ‘Malay’ races. However, he also posited the unity of the human species. Blumenbach did not originally use the term race: he wrote in Latin and preferred terms such as ‘gens’ (people) or ‘varietas’ (variety), but in a 1798 German translation of the original Latin, these terms were for the first time rendered as *Rasse* (Douglas 2008a:37-41).

This tension between the essential unity of humankind and Enlightenment ideas about progress and social development was constantly looking for an uneasy balance and throughout the 19th century the issue was much debated. These issues cannot be seen independently from the political debates of this time: the French revolution of 1789, forcefully spread all over Europe, the birth of romanticism and nationalism, the liberal insurgencies of 1848: all contain elements of this debate.

Moreover, the publication in 1859 of Charles Darwin’s ‘On the Origin of Species’, followed in 1871 by ‘The Descent of Man’ made it possible to think in terms of ancient human ancestors: the first Neanderthal bones had been discovered as early as the 1820’s, Eugene Dubois discovered the pithecanthropus (*homo erectus* or ‘Java-man’) in a Javanese-Chinese apothecary in 1892. In this context the reasoning that if species evolve and can be diverse, it does not seem all that strange some more ‘primitive’ (i.e.}

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less ‘complete’, less ‘like us’) human ancestor may just remain in some unexplored corners of this world,

Some married this theory of evolution – in essence ‘change through natural selection’ – with the Enlightenment idea of ‘progress.’ Charles Darwin certainly strengthened this interpretation of his theory when – in later editions of his books – he endorsed Herbert Spencer’s term ‘survival of the fittest.’ This again strengthened the argument for classifying people as either ‘primitive’ or ‘advanced’. These ideas coincided with the heyday of European and North-American (colonial) world domination and so usefully justified white superiority. In other words, they came to justify paternalistic colonial domination (Douglas 2008a:73).

All these theories, all this speculation can be traced back to two of the most fundamental problems of being human: the problem of Alterity – difference – and of Self, which are related like two sides of a coin. These problems are still very much current today among other due to the increased mobility of the last centuries or so – maybe the history of colonialism could be rewritten as the history of mobility (that is travel, that is exploration!) In fact all societies, all cultures are primarily concerned with ‘incorporating’ its members – this has been called the ‘production of local subjects’ (Appadurai 1996:178-199) – and it may well be counted among the universalia of human culture that this is done by clarifying borders between ‘us’ and ‘them’. The problems of Self and Alterity will therefore not be resolved anytime soon, but because of their fundamental value to humanity it is interesting to look at how historically it has been regarded.

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Blumenbach had established his racial taxonomy through consulting travelogues like those of Joachim Forster, who had travelled to the Pacific with James Cook, and the study of skulls, that Forster and others had
brought back (Douglas 2008b:110). Although Blumenbach tended more toward researching the physical aspects of these ‘varieties’, these ‘races’, this interpretation of the physique was soon joined with that of the material and social culture of the ‘Others’. For this reason, the American anthropologist William Sturtevant placed the beginning of his history of anthropology and museums in the 1840’s (Sturtevant 1969). Until the 1890’s, he argued, most anthropological research was conducted in the context of the museum; after this time in the United States chairs of Anthropology were created at universities, something that happened somewhat earlier in Europe.

I have argued that these ethnographic museums were the ‘human’ equivalent of botany’s herbaria: the natural history museum, with its collections of stuffed and potted animals can be regarded as another equivalent and indeed on occasion these institutions can all be found under one roof, e.g. at Stirling’s US National Museum. Like at the Alexandrian library – the Mouseion or Temple of the Muses – where all the world’s knowledge was gathered, all scientific data converged in these encyclopaedic institutions. Rather than going to travelling all over the world – quite a task – gathering everything in one place was rather convenient and so skulls and skeletons, and complete overviews of any particular tribe’s material culture were gathered. Initially, these museums simply solicited travellers for their curios, later a network of colonial officers and travellers provided the material, whereas books full observations provided context for this material in the museum library.

These data contributed to two debates that happened simultaneously: on the one hand, the definition of human races – What constitutes a race and how many are there? – on the other hand, the evolution of humanity – Is it possible to reconstruct human evolution by mapping the human races? The museum, and later the university, provided the central vantage point from which to gaze at the world.
These collections provided an opportunity to spot connections between different locations. Some theorists focussed on the spread (diffusion) of certain physical traits, but mapping the diffusion of technologies, myths, was another important activity. In this respect, Jaap Kunst's ethnomusicology is illustrative: in his book ‘On Papuan Music’ (Kunst 1931), he maps the various musical instruments in use in different areas and similarly he analyses the melodies for comparisons with what could be heard elsewhere and for particular ‘primitive’ traits. In describing his analyses, he employed geographical or archaeological metaphors: he writes about an alluvium and a diluvium of Melanesian and Australian ‘deposits’ that represent certain ‘layers’ of culture (1931a: 39). It should be noted that le Roux used the mapping device in his book also: he provides a map of the various axe-shapes of New Guinea, giving an excellent illustration of the purpose of ethnographic collecting.

However, unfortunately for Stirling and Le Roux, Sturtevant also argued that as of the 1920’s anthropology gradually left the museum behind. The old antiquarian and natural-scientific issues of races and evolution were abandoned and in the footsteps of Bronislaw Malinowski, anthropologists increasingly focussed on more ‘immaterial’, sociological aspects of humanity: what was just a division of labor within a greater project for Stirling and Le Roux grew into two separate disciplines. In a sense, this is one of the tragedies of the 1926 expedition.

**Origins of the Pygmy Question**

Having discussed the great scientific debates at the root of this expedition, it remains to look at the specifics that led Matthew Stirling to go to New Guinea. Europeans have historically made a connection between New Guinea and Africa: the Spanish explorer Iñigo Ortiz de Retes had named the island *Nueva Guinea* in 1545 because the black-skinned inhabitants reminded him of the ones he had seen in Guinea, a Portuguese-controlled

Now it will be remembered that the 1870’s was a time when Europeans were busy exploring and subjugating Africa. Combining scientific interest with a good nose for sensation, many explorers published their adventures in books, newspaper articles and lectures. After all, Henry Morgan Stanley – who looked for Dr. David Livingston, presumed lost in Africa – was a journalist who wrote regular letters to newspapers like the New York Times. A contemporary to Livingston and Stanley was the German explorer Georges Schweinfurth. Now, Schweinfurth – like many of his colleagues – showed what seems a universally human tendency; that is to explain something entirely new and unfamiliar in old and familiar terms. In his book (1874), Schweinfurth describes how he encountered a small man at the ‘court’ of a native ‘king’ (Bahuchet 1993:163). These words are already steeped in European history, placing the events in a European context, rather giving a good understanding of the central-African context. The small man caught Schweinfurth’s eye and he claimed to have found the mythical ‘pygmies’, which was a reference to classical Greek and Roman learning. This made his discovery not just familiar to Schweinfurth’s intended audience, who received their classical education in Germany’s gymnasium, but also connected it to a kind of ‘higher wisdom’ represented by this knowledge.

Pygmies have a long history in what retrospectively has been called ‘western thought.’ The oldest references to pygmies can be found in the 3rd book (6th verse) of the Illiad, where ‘pygmaioisi’ appear in an elaborate metaphor for a Trojan attack on the Achaeans. Homer compares the Trojans’ noise as they descended on the Greeks to the noise of the cranes that make their trek across the Okeanos to bring ‘death and destruction’ to these pygmies. Homer does not elaborate any further on what these ‘pygmies’ may
be, but from the name it is evident that they are a small people: ‘pygmaios’ derives from ‘pygmē’, that is ‘fist.’ In other words, ‘pygmy’ means ‘fist-like’, or in this case rather: ‘fist-sized’. We will never know whether Homer referred to an already familiar trope or created it anew, but pygmies have remained a fixture of literature since: the philosopher Aristotle, the geographer Strabo, the ‘natural scientist’ Pliny the elder, the church father St Augustine, and the mediaeval encyclopedists all wrote about pygmies (Bahuchet 1993:153-158). In other words, when Schweinfurth used the word ‘pygmy’ there were a great many authorities to appeal to.

In the context of the raging debates on human evolution and interest in the diversity of human races, Schweinfurth’s book caused a great deal of interest and ‘the pygmy’ was soon canonised as one of the human races. The French anthropologist J.L.A. de Quatrefages de Bréan made a major contribution to this process by dedicating an entire book to pygmies, simply called ‘les Pygmées’ (1887). Customarily basing himself on travel reports such as Schweinfurth’s, De Quatrefages attempted to fit the ‘little negroes’ in the greater scheme of the human races. Among others he distinguished Schweinfurth’s African pygmies of the central Congo from those of Asia and Oceania. The first he called ‘negrilloes’ and the latter ‘negritos’; both terms mean ‘little negro’. The Central African pygmies are nowadays identified as several tribes with names as Baka, Efe, Asua, etc. (Bahuchet 1993:175). In his article on ‘the invention of the pygmy’ Bahuchet came to the conclusion that ‘the’ pygmy does not exist, however it appears that today these tribes have adopted this old colonial moniker as a collective name (Paul M. Taylor, pers.com). The Oceanic pygmies –or negritos – by contrast were supposed to be found in such places as the Philippines (Aeta), the Andaman Islands (historically also referred to as ‘Mincopies’), and the Malay Peninsula (Semang).

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After De Quatrefages had established the pygmy question, Oceania and New Guinea soon joined Africa as an important locus for research and speculation on the distribution of this supposed pygmy-race over the globe. The racial constitution of New Guinea came to be seen as a particularly interesting issue. De Quatrefages had posited two races in New Guinea: a Papuan and a Negritic (Negrito) race. However, in the 1890’s the German Adolf Meyer took issue with this distinction and claimed that there was only one, if rather varied race in New Guinea. This position, however, he retracted in 1908, saying that there might be, after all, pygmies living in the interior (Ballard 2000:134-5). However, neither of these gentlemen had actually been to New Guinea, in fact most of the island remained unexplored, coastal areas remained badly charted as late as 1900 and before 1884 there had a single European settlement that had lasted more than a decade. Only in 1880 the first reliable travel report appeared, written by the Italian ornithologist Luigi d’Albertis who had steamed up the Fly river in 1875. He had collected some skulls that had been examined by the Italian anthropologist Mantegazza (Ballard 2000:134). Regarding the interior of the island, d’Albertis’ observations were pretty much all the reliable evidence to be had for people like de Quatrefages and Meyer.

In British anthropology, the period 1890-1935 has been called the ‘Oceanic phase’ (Urry 1998). In 1898 the Cambridge Anthropological Expedition – a momentous first step away from traditional ‘arm-chair anthropology’ – had done extensive research in the Torres Strait Islands, between Australia and British New Guinea. This was one of the, if not the first time professional anthropologists, connected to a university and a museum, did extensive research ‘in the field’. Many of the expedition’s participants would become influential figures in British anthropology; among them Alfred Cort Haddon. In a sense, the British Ornithological Union expedition of 1910 can be seen as a continuation of this Cambridge expedition, albeit an unintended one (see chapter one). In any case, after
seeing Wollaston’s reports on the Tapiro, Alfred Haddon concluded that ‘racial history of New Guinea (...) proved to be unexpectedly complicated’ (Haddon 1912:309). In the Torres Strait, Haddon had already become aware that the physiques of Papuans were too diverse to easily classify and if the coastal people already were so ‘racially complex’: this left him hungry for more knowledge of the interior.

On the face of it, the BOU expedition’s discovery of the Tapiro-pygmies in central Dutch New Guinea ended decades of speculation: there really were pygmies in New Guinea’s interior. A discovery that the expedition members – plagued with adversity – were only too happy to proclaim to the world and Alfred Haddon aided them with genuine enthusiasm. The expedition brought scientific proof in the form of photos and a short list of measurements that Wollaston had been able to make during the very short meeting (a couple of hours) with these people. The Tapiro became a cause célèbre within anthropology and their pictures featured many textbooks as the ‘type’ of the New Guinea pygmy (Ballard, Ploeg, and Vink 2001) – the word ‘type’ is used within Linnaean taxonomy to refer to the first fully described instance of a new species. However, British interaction with the Tapiro had been very brief and so the appetite for more information on these people was strong. Several Dutch military patrols encountered ‘dwarfs’ or ‘pygmies’ in different areas of the New Guinea highlands – e.g. the ‘Goliath’-pygmies, named after nearby Mt. Goliath (Godschalk 1999) – whereas other people were retroactively classified as pygmies – e.g Lorentz’ Pesechem in Bijlmer 1922 – but no anthropologist returned to the Tapiro until 1936 (Bijlmer 1938, 1939). This was the gap Stirling intended to fill.
Figure 8 Unidentified Dutchman and 'pygmies' in a Forest near Damunaru, Nogolo Valley. Arb052 (Taylor 2006)
What is a Pygmy? – end of a paradigm

Following De Quatrefages first treated the pygmy as one of the human races there followed what maybe described as ‘Pygmy Fever’ (Vlasblom 2004:93), which in turn led to the question: what is a pygmy? They were a small people, but just how small? Each having their own reasons, theorists included or excluded yet other people in this list. For example, the Austrian anthropologist, father Wilhelm Schmidt S.V.D., argued that pygmies were surviving descendants of the most primitive humans – die Kindheitsvölker der Menschheit – which was characterised by their many cultural lacks and for this reason he included South-African San (Bushmen) and Hottentots among the pygmies (Schmidt 1910).

For years, the definition of ‘pygmy-dom’ remained unclear. Interestingly, my two key-sources regarding this issue – Bahuchet (1993) and Ballard (2000, 2006) – disagree on the way pygmy-status came to be codified, despite Ballard’s reliance on Bahuchet. Both Bahuchet and Ballard agree that the bar was eventually set at 150 centimeter, or 4 feet and 11 inches, but Ballard claims this limit was first proposed by a German called Emil Schmidt and that it came to be known as ‘Schmidt’s bar’ (Ballard 2006:138). Bahuchet says that the limit was initially proposed by the German Helmuth Panckow in 1892 and that this standard was popularized by the Austrian father Wilhelm Schmidt (Bahuchet 1993:165); hence Schmidt’s bar. However, both also agree that this bar of 1 meter 50 centimeter was the one and only determining factor for ‘pygmy’-status: if a given population averaged below that, they were pygmies, if not, not.

This definition seems rather arbitrary and oddly enough awareness of this arbitrariness sounds through in some description of the so-called New Guinea pygmies. For example, Wollaston wrote about the Tapiro:

By contrast with the Papuans they [the Tapiro, MM] looked extremely small and, what was rather a curious thing, though many of our Malay coolies were no taller than they, the coolies looked merely under-sized and somewhat stunted men, while
This passage has interesting echoes in the dissertation of Hendrik Bijlmer, written for the University of Amsterdam. Unbeknownst to Stirling, Hendrik Bijlmer had visited New Guinea’s highlands before him. Bijlmer had been a medical officer on the 1920 Central New Guinea expedition and had an strong interest in anthropometry. On this expedition he visited what the Dutch called the Swart valley (natives call it ‘Toli’: Ploeg 1997), regarding the inhabitants he wrote:

> It may come as some astonishment that the ‘pygmies’ of the Swart valley, who are doubtless of the same race as the Pesegem and even as the Tapiro and Goliath pygmies, were only some mm. below 155 cm., and the Dayak whom nobody ever called dwarfish, some mm. above that value! (Bijlmer 1922:7)

Bijlmer is interesting to us, because he did what Stirling never did: to analyse and publish his anthropometric data. Interestingly, Bijlmer – with so many words – came to reject the notion of ‘the pygmy’. He concludes that there is no separate ‘race’ of pygmies in central New Guinea: ‘it seems to me that the word ‘pygmy’ has lost most of its typifying character’ (Bijlmer 1922:80), there are only ‘little Papuans’ (Bijlmer 1922:79). However, this rejection is half-hearted and in trying to make sense of the physical diversity of the Papuans, he revives the older terms ‘Negrito’ and ‘negritic’, which de Quatrefages had introduced to distinguish Asian–Oceanic pygmies from African pygmies. In other words, Bijlmer rejects the validity of the parent category while affirming the validity of the sub-category. To justify this quandary, Bijlmer has to engage in some arcane racial theorizing: he proposes the term ‘Papuan’ for people of ‘pure race’ and the term ‘Melanesian’ for cross-breeds (Bijlmer 1922:79), but on the very same page he suggests that coastal Papuans are a mixture of ¾ of the Negritic race and ¼ is ‘Dravido-Australian’ whereas the New Guinea highlanders are ‘pygmoids’ and contain 4/5 negritic blood and 1/5 Dravido-Australian. And so the pygmy,
initially rejected, returns in two guises: as ‘negrito’ and as ‘pygmoid’. After all: ‘they looked emphatically little men’

The influence of Hendrik Bijlmer on Matthew Stirling is obvious in the latter’s use of the word ‘Negrito race’ when explaining his reasons for going to New Guinea (Stirling in Taylor 2006a:6). But Matthew Stirling published next to nothing regarding his studies in New Guinea and upon his return to the United States he soon returned to his old passion: archaeology of the Americas.

Colonialism’s Culture: Racial Science Discredited and the Mythology of the Pygmy

The inimitable and incomprehensible reasoning of Hendrik Bijlmer is an excellent illustration of the bankruptcy of ‘racial science’: the anthropometric project founded on observable – morphological – difference that attempted to tie human diversity to human evolutionary history. Only recently has this project been revived thanks to an increasing understanding of human genetics. This has led to a revolution in the understanding of human evolution and the relations among populations.

However, there is an aspect of Hendrik Bijlmer’s work and that of many of his contemporaries, which is much less marked in contemporary publications – if not entirely absent (cf. Ballard 2006:144-147) – that helped to discredit the entire project after European colonialism came to an end. This is the idea of the essential inferiority of the people under investigation. In a popular article on the 1920-21 Central New Guinea expedition, Bijlmer’s tone is generally condescending toward the people he variably calls ‘dwarfs’, ‘pygmies’, and ‘mountain papuans’ (Bijlmer 1923) and he thus reflects the confused categories of his doctoral thesis (Bijlmer 1922). Moreover, in his popular article – which appeared in the magazine De Aarde en haar Volken (the Earth and its peoples) – Bijlmer classifies his research subjects among the ‘lower races’ (Bijlmer 1923:42; my translation), repeatedly compares them
to children (Bijlmer 1923:41, 51) and comments on their half-bestial state (Bijlmer 1923:49). In contrast, however it should be noted that Le Roux and Stirling generally spoke friendly and respectful of the Papuans – no Malinowskian rants about ubiquitous ‘niggs’. At the same time, it should also be noted that there certainly was more than a hint of sensationalism in the Richard Peck’s film and Hedberg’s reporting (see also chapter four).

Colonialism has been called a ‘culture’ (Thomas 1994), meaning that the term implies much more than just a system of political domination and economic exploitation. This system literally permeated the mind of the colonizers. One of the aspects of this culture, according to Nicholas Thomas, is the tendency to describe colonial subjects as ‘primordial, infantile, and bestial’ (Thomas 1994:101). Bijlmer’s work certainly seems a good illustration of these. The trope of the infantile colonial subject is also well illustrated by Wilhelm Schmidt’s work on Pygmies as ‘die Kindheitsvölker der Menschheit’ and the trope of the primordial is evident in Wollaston’s book: ‘The Stone Age in New Guinea Today’ (1912). The point here is not that Papuans indeed used stone tools – they did – but what such statements implied: essential inferiority and thus: fit to be ‘elevated’, or rather: fit to be dominated.

Chris Ballard has argued that these tropes applied to pygmies in a special way: in particular, he claims that pygmies were often contrasted favorably to other colonized people (Ballard 2006:140-1, 144-147). This is an interesting notion to explore. It seems that the expedition members – despite public statements – did not consider the people they encountered in the highlands to be pygmies, yet they do on occasion compare the mountain people favorably with their Lake Plain cousins. However, this is usually based on objective criteria: Le Roux notes that generally they show a rather

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15 e.g. Le Roux 1926d:5 ‘de pygmaëen, beter gezegd pygmoïden en wellicht nog juister bergpapoea’s’; the pygmies, or rather pygmoids or maybe even better mountain papuans; my translation. Also: Posthumus 1926b: September 6, p.48: ‘Deze bergbewoners maken niet den indruk dwergen of pygmeën te zijn’ These mountain people do not give the impression to be dwarfs or pygmies; my translation.
jittery, nervous attitude (Le Roux 1926c:11), more attacks etc – and not, as in Ballard's examples, on any subjective criteria such as supposed thievishness. Further analysis of Dutch writings on New Guinea Papuans and pygmies would be necessary to draw any further conclusions. Still, Ballard proposed notion of a ‘pygmy mythology’ is interesting at the very least:

Throughout this history, the frame of reference for Pygmies has been mythological – earlier myths are instantiated through acts of encounter that are then, themselves, re-mythologized (Ballard 2006:135)

Earlier in this chapter, we have seen that the whole research of pygmies was in fact based on interpreting a real event in mythological terms. In the coming chapter we shall see how the 1926 expedition became itself ‘mythologized’ in similar terms.

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16 “De meervlakte Papoea’s zijn in hooge mate zenuwachtig en opgewonden. Ze gesticuleeren druk en schreeuwen of ze tegen dooven praten. Als mensen, die lata zijn, deden zij de meeste bewegingen, die wij deden na en trachten ons na te praten.” The Lake Plain Papuans are highly nervous and excited. They gesture wildly and yell as if talking to the deaf. Like people who are lata, they imitated most movements we made and they tried to imitate our speech; my translation.
CHAPTER FOUR

The Film “By Aeroplane to Pygmyland”: impact of the 1926 New Guinea expedition outside the Museum

Throughout the expedition, Matthew Stirling’s collaborator Richard Peck worked on a documentary film about the ‘pygmies’ of the unexplored highlands of Dutch New Guinea. This chapter will briefly step aside from the scientific and museological aspects of the 1926 expedition to consider the curious case of this film, whose (incomplete) history I will briefly sketch here. This chapter also serves to expand on Matthew Stirling’s motives to go to New Guinea and it serves to illustrate the effect this expedition had outside the more narrow fields of science and museums that have been discussed so far. In the final section of the chapter, I will take the expedition’s film as a representative of a genre of expedition films that shaped the dreams, the cultural production of generations to come. This last section serves as an introduction to the next chapter that deals with perceived problematic status of the heritage of colonialism and of this expedition at the Tropenmuseum.

Shooting a Film in New Guinea

It seems that Peck’s film was key to Matthew Stirling’s financial plan for this expedition. Unfortunately, the exact details are sketchy and the following is a reconstruction in need of further research. In the spring of 1924 Stirling left his job at the US National Museum and in July of that year he participated in the Hamilton-Rice expedition to the Peruvian Amazon (Richardson-Flemming 1992; le Roux 1926a). Upon his return, Stirling goes to Florida to participate in archaeological digs, earning money on the side by dealing in real estate. At this time in the 1920’s, Florida was experiencing a real-estate boom, if it was not a housing bubble that famously crashed in 1929. It was at such a dig in Florida that Stirling met Stanley Hedberg who
worked for a local real estate developer who may have sponsored the excavation in the hopes of raising the prestige – and price – of his property (Taylor pers.com.). Hedberg worked as a Public Relations man and soon became friends with the archaeologist. Together they decided to go to New Guinea in pursuit of Wollaston’s Tapiro pygmies.

Figure 9 Richard Peck filming Papuans in Agintawa, Delo Valley

Of Stirling it has been said that his undertakings were ‘often touched by romantic impulses’ (NAA, n.d.) and this was certainly the case here. The plan for the New Guinea expedition seems to have been a mix of adventure and investment. Stirling and Hedberg both men from relatively wealthy families\(^\text{17}\) and the money they had made in real estate was going to be their starting capital (Richardson-Flemming 1992). It is unknown whether Stirling, Hedberg or the others incurred much, if any debts before the

\(^{17}\) In a letter, dated March 23, 1926, Alexander Whetmore, assistant-secretary of the Smithsonian Institution, assures Mr. P.B. Gilbert of the State Department that he had ‘obtained definite assurance that the members [of the expedition, MM] were financially capable and able to support the heavy expenses incident to such work.’ He continues: “Both Mr. Stirling and Mr. Peck are from wealthy families so there was no question on this score” Accession Files to the Matthew Stirling Ethnological collection, at the National Museum of Natural History, Smithsonian Institution, Washington, DC.
expedition set off, although given the scale of their undertaking this seems likely. The returns of this investment may have been conceived to come from several sources. First of all, there would be syndicated press reports: Hedberg had a background as a journalist for Associated Press and throughout the expedition he wired his reports to the United States. At a time before television and with radio still in its infancy this was as close to ‘live from the jungle’ as one could get. These reports may or may not have brought in money – by selling rights to the newspaper syndicates like Hearst and Hedberg’s former employer Associated Press – but at least they hoped this would make people talk about the expedition.

The film, then, may well have been conceived as the second tier in the plan. The newspaper reports would create a ‘buzz’ and distributing and touring around the United States was a second way of making money. A film would certainly make Stirling’s planned lecture tour even more appealing and in Richard Peck, a friend of Hedberg’s, they had someone to operate the camera. Peck would shoot the very first film in the interior of New Guinea. These were ‘roaring twenties’ and the whole undertaking seems suffused with an optimistic trust in cutting-edge modern technology. Film itself was only one of these new phenomena that Stirling and Hedberg employed. People were flocking to the cinemas to see the latest ‘movies’, but something extra was needed to stand out. The airplane was one such point: flight held wide public appeal, as evidenced by the ‘around the world flights’ that Stirling referred to in selling his plan to the Dutch colonial government: Stirling claimed to have the exact same airplane model (le Roux 1926a). It comes as no surprise then that the men advertised their film and lecture tour as ‘By Aeroplane to Pygmyland’. The exotic location of New Guinea was another selling point: the dangers it posed, the jungle, the mountains and at the very heart, unknown, untouched and unexplored, the pygmies.

This film and lecture tour may well have served for two more ways in which the men could make some money – after all, Stirling and Hedberg
financed this expedition themselves! One plan was to publish Stirling's expedition diary (Taylor 2006b:1, 2006c:12-16), although for some reason this never materialized until Dr. Paul M. Taylor published it and Hedberg's, too (Taylor 2006). Stirling certainly intended his diary to be published, although it does not appear that the Hedberg's ever was, because its observations are more ‘frank’ – its discovery among the Stirling papers was therefore a happy coincidence.

The final plan was to attract sponsors. Hedberg was careful to mention these in his reports and they also feature in the film. Although the original film that was shown at Stirling's lectures did not survive in the United States, a copy survived in the Netherlands. There are some problems in using this copy for drawing conclusions about Stirling’s film – more about this below – but whatever it is that survived at the Nederlands Filmmuseum in Amsterdam has been restored and re-released (Filmmuseum 1995). One sponsored sequence stands out in particular. In this scene a group of Papuan men sit around a tin of motor oil, from which they are encouraged to eat, while the intertitle informs the viewers Pennzoil motor oil is a newfound Papuan delicacy.

**The Story of the Film: whose cut survived?**

The original version of the film that Matthew Stirling used during his lecture tours was lost to flooding at the Human Studies Film Archive, at the National Anthropological Archives in Washington, DC (Taylor 2006a:note 8). Nowadays the only version of Stirling’s film surviving in the United States is a rough cut of about two hours of footage complemented by an audio commentary that Stirling produced sometime in the 1960's. There may well have been renewed interest in Stirling’s material at this time because the National Museum of Natural History – heir to the Ethnology department of the former US National Museum – was constructing a diorama based on ‘his’ New Guinea highland pygmies (or Dèm). This diorama was to form part of
the new Pacific People Hall that opened in 1964, it was taken down only in 2004 to make place for the major new Sant Ocean Hall, which opened in September 2008. These film fragments – including Stirling’s commentary – are currently available online at the website of the Smithsonian Institution Libraries (Taylor 2006).

However, all is not lost. In 1995 the Nederlands Filmmuseum restored and re-released a mute film-documentary about the 1926 New Guinea expedition under the original American title (Filmmuseum 1995). Unfortunately, although the film certainly is about the 1926 New Guinea expedition, the provenance of this film remains unclear. The film that the Filmmuseum restored certainly is not what I will call the ‘American’ cut that Stirling used in his lectures (according to Stirling’s widow; pers.com. Paul M. Taylor). But then the question rises what it is if not that.

The Filmmuseum holds material from two sources: loose fragments stamped KIT from the Royal Tropical Institute (former Colonial Institute) in Amsterdam and a full film copy from a private donor, one Mr. Hodde. Curator Mark-Paul Meyer of the Filmmuseum informed me that the restored version was basically a complete cut that was among this material – presumably Hodde’s copy – and that re-editing was relatively minor: some of the intertitles were merged and a sound track was added. This sound track is a rather interesting for its incorporation of some of the original 1926 recordings, but is generally a rather clichéd affair with many bird noises and other supposed jungle sounds; quite different from the Chopin that accompanied the original showing of this (?) film in 1937 (Vaderland 1937, December 6). At this point, I should note that at some point in my research I decided against further investigating this film’s history as it was getting too complicated, but that I did this only reluctantly. Yet, along the way I picked up a thing or two about this fascinating history.

In the fall of 1928, one Lodewijk (Loet) Cohen Barnstijn, owner of the ‘Loet C. Barnstijn’s Standard Film, B.V.’ and of several cinemas in The
Hague, bought a copy of the Stirling’s film from an American company called ‘Producers International Corporation’ (PIC) and – as was obligatory as of 1928 – he had it certified by the newly established Dutch film censor. Presumably Barnstijn adapted this film, if only to translate the intertitles. The release of this film is an issue of concern to Le Roux and his colleagues at the ICWO, who read of it in the newspapers in the Dutch-Indies. They disapprove of the American view of the expedition, in particular they appear to object to what they see as exaggeration of the size of the unexplored area in New Guinea, thereby diminishing Dutch achievement and enhancing their own achievement. For this reason, the ICWO – by way of the Dutch Consulate in San Francisco – presses Stirling and Peck to send them a full copy of the raw footage. This copy arrives in Batavia in the spring of 1930, however at the next meeting – almost a full year later – Le Roux complains that what the Americans sent is all a mess: ‘one moment one sees before one the bay of Ambon, the next suddenly the highlands of New Guinea appear before the eye.’ Le Roux is still working on the film material when he leaves the Dutch-Indies in the winter of 1933-34 and he is still working on it when he takes a job at the Colonial Institute in 1934. In a report on his activities Le Roux anticipates to complete his cut in the summer 1935, but the first mention that his film is actual shows may be the final – and only – entry for 1939 in an official overview of ICWO history: ‘first showing [of] movie Dutch

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18 “By Aeroplane to Pygmyland” is film nr. 3934 in the archives Centrale Raad voor de Filmkeuring, Nationaal Archief, the Hague; cf. NRC 1928, December 25
19 Minutes of meetings 71 and 72, 74 and 75, file 2-3, archives of the Indisch Comité voor Wetenschappelijke Onderzoekingen, Nationaal Archief, The Hague; see also a letter by Dr. W.M. Docters van Leeuwen to Charles le Roux, dated January 22, 1929, in file 36 of the same archive.
20 Minutes of meeting 74 (July 19, 1930), file 3, archives of the ICWO, Nationaal Archief, The Hague
21 Minutes of meeting 75 (June 8, 1931), file 3, archives of the ICWO, Nationaal Archief, The Hague; my translation
22 ‘verslag van den concervator C.C.F.M. le Roux over zijne werkzaamheden gedurende het tweede kwartaal van 1935’; file 4415, archives of KIT, Amsterdam
and American Expedition to the Nassau Mountains in Central New Guinea.”

In the late 1930’s two more articles appeared in the newspaper ‘Het Vaderland’, published in The Hague, that discuss two further occasions at which a film about the 1926 New Guinea expedition was shown in that city (Vaderland, 1937, December 6, and 1938, February 5). Charles le Roux is mentioned in one of these articles, but he does not appear to have been involved in either of these events. It is unclear whether they were showing Le Roux’s cut or Barnstijn’s imported ‘American’ version: either case could be argued, particularly if le Roux indeed finished his cut in 1935.

Finally, as an aside to complicate the cinematographic aspect of the expedition even further: there is also inconclusive evidence that the Dutch colonial army may also have produced a film during this expedition. In the very beginning of his diary, Captain Posthumus writes that he bought a camera and 1 kilometer of film to shoot a movie during the expedition for internal or propaganda uses (Posthumus 1926, April 16, 17, 20; see also Posthumus 1930). Whether he actually used the camera, remains to be seen. In the rest of his diary, he only mentions his camera once and this is when he regrets that he was unable to film a particularly amusing encounter between two Papuan groups; he writes that ‘unfortunately’, ‘the Army’s film apparatus was not ready’ (Posthumus 1926, July 24; my trans.). Posthumus also briefly mentions ‘an apparatus for producing cinema films’ in an elaborate article reflecting on the expedition (Posthumus 1930:396), but this only confirms that the camera was brought and that ‘someone’ was found to operate it.24 Whether any film was actually produced by the Army and what its fate may have been remains entirely unclear.

23 Kort Overzicht van de Geschiedenis van het Indisch Comité voor Wetenschappelijke Onderzoeking, file 83, archives of the ICWO, Nationaal Archief, The Hague; my translation
24 Full text of what Posthumus wrote in this article for the Orgaan der Nederlandsch-Indische Officiersvereeniging, the Magazine of the Dutch-Indies Officer’s Society (Posthumus 1930:396):
‘Toestel voor het vervaardigen van bioscoopfilms"
**Pygmy Mythology – contested heritage?**

Matthew Stirling and Stanley Hedberg intended to ‘sell’ his expedition on the combined appeal of airplanes and pygmies – at least that is what the title of his film and lecture tour suggests. At the end of the chapter three, I quoted Chris Ballard on ‘pygmy mythology’:

Throughout this history, the frame of reference for Pygmies has been mythological – earlier myths are instantiated through acts of encounter that are then, themselves, re-mythologized (Ballard 2006:135)

Stirling’s expedition itself ‘instantiated a myth through acts of encounter’ and then re-mythologized itself through Peck’s film and arguably also Hedberg’s press reports. However, closer analysis of Hedberg’s articles in particular would be necessary.

In chapter three, we saw that the image of ‘the pygmy’ has long roots in Western thought. My key source for his chapter, Serge Bahuchet, wrote about ‘the invention of the pygmy’ (1993), but his article might rather be titled ‘the constant re-invention of the pygmy.’ By tracing the long history of ‘pygmies’, he himself implicitly acknowledges that ‘pygmies’ were not invented out of the blue. The point is that Georges Schweinfurth, and – arguably more importantly – his readers did not invent but re-interpreted the old and familiar image of ‘the pygmy’ for present purpose. Post-Schweinfurth, the pygmy came to be closely associated with black, small people living in the jungle of Africa – or its Oceanic counterpart: New Guinea – and this image appears to have played an important role in making sense of the colonial world in which Europeans dominated others (cf. Thomas 1994; Ballard 2006).

It may be problematic to attach conclusions to the Filmmuseum edition of the expedition film, but it certainly seems to fit a discourse on ‘pygmy mythology’ and ‘colonialism’s culture.’ The film is primarily a narrative of

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Voor het vervaardigen van bioscoopfilms van het militaire werk tijdens de expeditie, was uit het fonds der militaire sociëteiten een bedrag beschikbaar gesteld voor den aankoop van het toestel en de benoodigde films, chemicaliën enz. Voor de opname had zich één der officieren beschikbaar gesteld.'
adventure and discovery. Already we have seen how it essentially portrays its subjects as gullible – motor oil as delicacy – and the film continues in this vein when the narrative comes to the moment of first contact. In fact no camera was present at this historic moment – Posthumus had forbidden any of the scientists to be present in the vanguard of the expedition – but the narrative demanded that some visual material illustrate it. To circumvent this inconvenience, an alternative, fictional narrative is constructed, based on the 1914 MED expedition that explored this area. This expedition had spotted cultivated fields in the distance (Doorman and Langeler 1918), one of the reasons that this expedition came here. In the film the expedition also spots these fields from a distance, approached them and planted a ‘hidden camera’ to await the Papuans that would undoubtedly approach. The footage then shows a man jumping around excitedly, shooting arrows. For a moment, the viewers are led to perceive these Papuans as dangerous, but this quickly turns into a comic note as the intertitles tell us that the confused pygmy is shooting his arrows the wrong way, away from the Western visitors.

It may come as no surprise that pygmies became a problematic symbol at the time of decolonization. Colonialism as a culture was never monolithic, but decolonization was still a traumatic event. In my introduction I placed this event in the period 1950’s-1970’s, a time of great turmoil: old certainties of the pre-World War II were undermined on all sides: the German Nazis and their attempt at destroying the ‘Jewish Race’ and the fight for racial desegregation in the United States. The association with racial science certainly did the pygmy no good either in these days. However, fighting ideas is notoriously difficult. Symbols are easier to target and so pygmies became one of the objects, or maybe rather: the medium for resistance to the discredited culture of colonialism. An interesting example of this resistance against pygmies and colonialism is the case of the Oompa-Loompas in the classic, children’s book that appeared in 1964, Roald Dahl’s Charlie and the Chocolate Factory (Dahl 1964). Dahl was born in 1916; he was only ten at the
time of the Expedition and may very well never have heard of it, Stirling’s expedition was not unique and Dahl’s book shows that the young Roald certainly was familiar with imagery similar to that used by Stirling, Hedberg and Peck. In this book, the pygmies are called *Oompa·Loompas*, a tribe of small, industrious, chocolate-loving people that live deep in Africa’s jungle. At a certain point Willy Wonka – owner of the eponymous chocolate factory – tells the hero Charley that he saved the poor creatures from a terrible jungle habitat, where monsters like the ‘whangdoodle’ hunt them for food and where they subsist on a disgusting, distinctly unchocolate-y diet of leaves, caterpillars, and tree-bark. Fearing industrial espionage, the chocolate-industrialist fires all his ‘normal’ employees and henceforth only employs *Oompa·Loompas* in his soon very famous and now very secretive factory.

![Figure 10 Oompa Loompa in Tim Burton's 2005 Film Charlie and the Chocolate Factory (photo from typebrighter.com 2005)](image)

Initially, Dahl's work was much praised, but in the 1970's it suddenly generated a good deal of controversy. Notable is a debate between Dahl and the American author Eleanor Cameron in a scholarly magazine on children’s literature. In a larger article, the latter blasted Dahl’s book for its colonial
and exploitative depiction of the African *Oompa-Loompas* (Cameron 1972-3), Dahl responded and defended his work (Dahl 1973). However, he still rewrote the book, making the *Oompa-Loompas* into white pygmies (Howard 1996). Also illustrative of this sensitivity or resistance to certain colonial and racial imagery is the 1971 film version of Dahl’s book – renamed *Willy Wonka and the Chocolate Factory*, directed by Mel Stuart – portrayed the *oompa-loompas* as little orange-skinned, green-haired men. However, Dahl’s adaptations were reversed in the 2005 film version of the book, this time directed by Tim Burton. In this film the *Oompa-Loompas* are once again brown pygmies that came from the jungle. In one scene, Willy Wonka is explicitly depicted as a Victorian-type explorer who saves the poor pygmies from their miserable jungle existence. Sensibilities to a discredited colonialism and racism have gone to the background and what remained is the appeal and romanticism of exploring unknown areas and the wondrous things to be encountered there – indeed, like Stirling, to go somewhere where no-one has gone before. Moreover, the film was received well and surprisingly few critics were dismayed at the colonialist, exploitative, or racially stereotyping imagery that Cameron had denounced – although some scences could have been interpreted as such (cf. Typebrighter.com 2005).

![Figure 11 Oompa-Loompas in 1971 and 2005. Photos from imdb.com](image)

In making sense of these changes, interpreting ‘pygmy mythology’ may help. However, this requires instruments associated with the analysis of
mythology; for example Claude Lévi-Strauss’ thesis of mythology as the ‘science of the concrete.’ This thesis is elaborated in Levi-Strauss’ book ‘The Savage Mind’ (Lévi-Strauss 1966), where the venerable French anthropologist posits that mythology is basically ‘primitive science’ in which concrete objects or person come to represent abstract concepts and ideas. In other words, mythology according to Levi-Strauss is a way of making sense of the world with whatever means are readily available. To illustrate this point and true to his own thesis, Levi-Strauss used a ‘concrete’ metaphor: he compares the myth-maker with a tinkerer – bricoleur, in French – who makes and repairs objects with whatever is at hand. Similarly, the mythmaker uses stories involving concrete objects and persons to make sense of the world – after all, that is what science does too. The analogy of mythology and science in a way frees the ‘pygmy’: the association with colonialism will not disappear anytime soon, but the value attached to this association can change. After all, as any discussion between theologians can illustrate: the correct interpretation of mythology can be particularly difficult.

For a long time, Peck’s movie lay forgotten in vaults in Amsterdam and Washington, DC, and was not a matter of discussion in any debate like that of Cameron and Dahl. But we – (potential) viewers of the film – either were involved or else carry the outcomes in our heads: as inherited thought patterns, moral codes, imagery and associations. This chapter has been a brief excursion into the field of film and mythology, but it has also served to illustrate that the heritage of the 1926 New Guinea expedition is contested. In the next chapter, I will return to the museum collection and discuss how this issue of contested colonial heritage has affected the Tropenmuseum.
CHAPTER FIVE

Engaging Colonialism at the Amsterdam Tropenmuseum

Information on the catalogue cards associated with collection 514 at the Tropenmuseum – Le Roux’s 1926 New Guinea collection – allows for a tentative reconstruction of how the museum used this collection throughout its history. Although most objects now repose in storage in the museum’s attic, some objects have been on display in the period after the 1927 exhibition. An exact reconstruction I have not been able to make, but a handwritten note on one of the catalogue card says that at least one object – the tail of a cuscus or tree kangaroo – was used as an ornament for the mannequin of the ‘Mountain Papuan.’ This mannequin was taken down at the renovation in the early 1970’s (David van Duuren, pers.com). Van Duuren, curator Oceania and historical collections at museums, also assures that the mannequin was not set in a diorama in the same way that the Smithsonian collection was exhibited, but this to me seems a minor distinction: both are illustrative of the exhibiting techniques of roughly of the era of decolonization (1950’s - 1970’s; arguably of an earlier age, too, dioramas have a long history). These exhibitions were based on showing the particular ‘type’ of a people, race, culture, group, etc. in this case the type of the Mountain Papuan. Note that this terminology derives from the Natural Sciences where a type – or ‘holotype’ – is the first described specimen of a new species; such types – like Dubois’ ‘Java-man’ homo erectus skull at the National Museum of Natural History in Leiden - are among the best guarded treasures of a natural history museum.

Taking down the Mountain Papuan mannequin in the early 1970’s is illustrative of changing attitudes in ethnographic museums, and museums in general. By this time ethnographic collections – like number 514 – had come
to symbolize a doubly discredited past: discredited scientifically and culturally. This cultural rejection was discussed at the end of chapter four. Scientifically the collection was considered useless as very few anthropologists still did material culture research: William Sturtevant, who worked as an anthropologist at the Smithsonian National Museum of Natural History, estimated in a speech to the Washington Biological Society that of the 90% of any museum’s collections simply lay gathering dust in the attic (Sturtevant 1969) – and in the early 1970’s this was quite literally true in the case of collection 514.

From Colonial Museum to Tropenmuseum

The current Tropenmuseum and the Royal Tropical Institute (KIT) in Amsterdam are the successors of the old Colonial Institute and its museum. This Colonial Institute had been the private initiative of a group of citizens – merchants and politicians – that was closely involved in the business of the Dutch colonies of the East and West Indies. At its creation in 1910 the society was designated ‘Royal’, but that is where the involvement of the Dutch royal family stopped. This institute was itself the direct successor of an older institute that has been established in Haarlem in 1864 and the collection of this older Colonial Museum transferred to the new institute, where it was joined with the ethnographic collection of Amsterdam’s zoo Natura Artis Magistra. The construction of a building for this new institute took 16 years – delayed by financial woes and shortages during the First World War – and when it finally opened the doors in 1926 the institute was housed in the largest building in Amsterdam (Woudsma 2004). At the time of the opening, a brochure appeared that introduced the goals of the new institute as ‘making propaganda’ for the Dutch colonial project and serving as a central point for research and information regarding the Dutch colonies (Hasselman 1926).

As can be expected from a Colonial Institute, the museum was primarily occupied with ‘colonizing’ the subjects of the Dutch empire. Colonial
subjects were put literally on display – the mannequin of the ‘Mountain Papuan’ was no exception – and by doing so the museum ‘taught the Dutch colonialism’ (Legêne 1998). The Colonial Museum was both a trade museum as an ethnographic museum and as a result these people were displayed next to the commercial wares that also came from the colonies. The public was thus exhorted to be ‘good’ colonizers – perhaps in the vein of the ‘Ethical Policy’ that aimed at elevating the natives with the aid of science – but further analysis of the exhibition practice of the old colonial museum would be necessary before drawing conclusions. In any case display also meant domination, or in any case taught visitors of the museum domination. A good illustration of this can be found at the World Colonial Exhibition of 1883 in Amsterdam, where the colonial government of the Dutch-Indies started what would become a tradition of sending an entire Indonesian village, complete with inhabitants, to World Exhibitions (Bloembergen 2002).

Figure 12 Diorama of “Dem Negrito Pygmies” at the National Museum of Natural History, Washington, DC (1964-2004). Photo courtesy of Adrienne Kaepler
After the Second World War, Indonesian declares its independence from the Netherlands, an intense period of warfare followed but in 1949 the Netherlands finally concede and Indonesia become independent. This period is significant in terms of the museum’s collection history. In this period when the future of the Dutch colonial project was uncertain, the Colonial Institute became Indies Institute (*Indisch Instituut*). As if to underline this connection, in the short life span of this incarnation of the institute – in 1950, the name was changed again – significant parts of the collections that did not come from Dutch colonies were exchanged or sold off. Although collection 514 was obviously unaffected by this, it serves to illustrate that the Dutch in general and the Colonial Institute in particular had great difficulty to let go of the colonial past.

In 1950 Indonesia become definitely independent and the Indies Institute changed its name to Royal Tropical Institute (*Koninklijk Instituut voor de Tropen*; KIT) and its museum became known as the ‘Museum of the Tropics’ or *Tropenmuseum*. Since its inception, the institute had been an important center for the research of tropical agriculture and medicine, but after the independence of Indonesia by necessity the institute turned to the market of development work in ‘the tropics’; the Museum adapted accordingly. The Head Curator at this time was Dr. J.H. Jager Gerlings, who spoke about this transition in an interview:

One cannot imagine what it is to be stuck with a colonial museum, with mainly collections from Indonesia. First the collections on Surinam and the Antilles had to be improved and finished. Then, in a somewhat later stage, we had to build around this the entire world of the tropics. We had nothing, but the funny thing is that this makes one very resourceful. (Jager Gerlings in Van Duuren 1990:33; *my translation*)

This situation where the KIT increasingly became an actor in development work in what was called the ‘Third World’ was formalized in 1971 when the Ministry of Foreign Affairs and International Development proposed to
become the most important financier of the KIT (Legêne 1999a). The institute became an instrument in the Dutch government’s foreign aid policies, but Legêne also argues that the museum ‘was and perhaps still is an institution for the presentation of Dutch overseas policies’ (Legêne 1999a:267)

The removal in the early 1970’s of the Mountain Papuan mannequin must probably be seen in the light of this history. After the 1950’s, the museum was searching for a new mission to go with its rather undefined, new ‘Museum of the Tropics’ umbrella. Perhaps the Mountain Papuan had simply been an untouched relic from the old colonial museum days. In the 1970’s however, the reorientation process was completed and museum underwent a major renovation. It was making a virtue out of necessity and in the spirit of the times it started expunging its now unwanted colonial past. A good illustration of this can be found in changes to the museum’s architecture: the museum entrance, with its grand stairs that led one directly to the great Hall of Light was taken down and replaced by a rather cavernous affair that first leads one into the museum’s basement. An anecdote I heard at the museum speaks of the new floors that were installed in the exhibition areas at this time. Apparently, beautiful tropical hardwoods had been selected but for fear of being perceived as pretentious or opulent – in contrast with the museum’s new mission of raising awareness over the plight of people in the third world – the floor was installed with the ‘wrong’ side up and the beautifully polished side down. Whether this is true or not, I cannot tell; as corroboration I have shown areas where fragments of old batch markers were clearly visible. But in any case the anecdote is illustrative of the changing attitudes of this time.

One thing did not change and this was the museum’s preoccupation with ‘Alterity’ in relation to a self-improving – as opposed to self-analyzing – discourse. Instead of colonial subjects, the museum now displayed ‘Third World’ subjects. With the increasing focus on development aid in the 1970’s, the museum also increasingly focused on ‘decolonizing’ its public. Although it
no longer instructed its visitors in a form of paternalistic – *i.e.* ‘good’ colonial
domination, the museum did retain its paternalistic approach to the ‘other’
as one who was freed from colonial domination but still in need of saving.
Even though the museum was busy eliminating all manner of visible
reminders to its colonial past, this was one colonial heritage it did not
confront.

This process of ‘superficial’ decolonization of the museum may be
considered to have reached its peak in the 1970’s and 1980’s, but apparently
it lost some of its momentum in the 1990’s. Susan Legène wrote that she felt
the museum was lacking in vision at the time of her appointment as Head
Curator in 1998 (Legène 1999b:74). This is when the museum decided that its
semi-permanent exhibitions needed a makeover and a process of rethinking
the museum began. Legène was the driving force of this project that took well
over a decade. Looking back in 2008, she placed the beginnings of the
reorganization as early as 1993 when the old South-East Asia exhibit was
taken down (Legène 2008). In 2003 the replacement exhibition opened:
‘Eastward Bound! Art, Culture and Colonialism’, which may be regarded the
centerpiece of the museum’s new strategy. On the occasion, she formulated
the museum’s new strategy thus:

> The *Tropenmuseum* will no longer just present others – far
away, and anonymous; we also want to discuss ourselves a bit
more – close-by, and with names (Legène 2003, *my translation*)

In the making of this new exhibition, the 1926 expedition was
rediscovered and put on display in line with the new goal of representing ‘not
just others’, but also ‘ourselves.’ Charles le Roux is literally put on display,
representing ‘western science’ investigating ‘others.’ He is a mannequin set
up in a canoe, taking a picture of a group of Papuans along the Mamberamo-
river. The Papuans are not there as mannequins but as the picture that Le
Roux took. On the wall next to this picture and on the other side of this wall,
scenes from Peck’s expedition film “By Aeroplane to Pygmyland” are shown
in constant loop. In front of Le Roux’s mannequin, three museum cases show a rather chaotic collection of artifacts related to this kind colonial scientific expeditions: one with natural specimens (birds, plants, etc), one with instrument of physical anthropology, and one with photographic apparatuses. Among these is one picture, presumably taken by Dr. Docters van Leeuwen: a picture of Le Roux taking a picture of a group of Papuans. In other words, the picture is of the scene that the mannequin represents. Two things are particularly striking about this exhibition: 1) the 1926 expedition is now represented as a ‘type’ the way the Mountain Papuan mannequin was a type before and 2) in this display everyone goes unnamed.

Figure 13 Charles le Roux as the ‘type’ of the Dutch colonial explorer in the ‘Eastward Bound!’ exhibition at the Tropenmuseum (photo by the author)
CHAPTER SIX

Conclusion: the Postcolonial Museum

The changes at the *Tropenmuseum* over the last fifty years or so may be interpreted along these lines: until the 1950's the museum was primarily concerned with the colonial other, with the loss of the colonies a period of soul-searching began and in the 1970's the museum refocused on a narrative about the ‘post-colonial other’. This was an explicit rejection of the museum’s own colonial past – reflected in an uneasy relation with the building’s architecture. In the late 1990's and 2000’s the museum decided that it should not only focus on the post-colonial other, but also on the post-colonial self. These developments reflect a broader breakdown – coinciding with the end of European colonialism – in the ‘problem of Alterity’: what exactly do or can I know about the ‘other’ (*cf.* chapter three)? This problem came to be posed with great force among those scientists that were closest involved in these issues: anthropologists. In the 1980's and 1990's the colonial biases of their own work came to be under close scrutiny. The subtitle of one seminal work framed the discussion in terms of ‘the poetics and politics of ethnography’ (Clifford and Marcus 1986). Many anthropological authorities *seemed* to come toppling down and a great deal of introspection followed (*e.g.* Geertz 1988). Similar debates were raging all over the newly emergent ‘cultural studies’ field: for instance, the modern field of museum studies may be said to have been born out of these debates, as evidenced by this title – also a seminal work among museologists: ‘Exhibiting Cultures: the poetics and politics of museum display’ (Karp and Lavine 1991; see also: Karp and Kreamer 1992).

These are valid questions and some attention to the self is necessary, but too much focus on the self will only lead to narcissism. In chapter five, I already quoted Susan Legène’s statement at the opening of the exhibition ‘Eastward Bound!’ in 2003:
the Tropenmuseum will no longer just present others – far away, and anonymous: we also want to discuss ourselves a bit more – close-by, and with names (Legêne 2003, my translation)

The question is now whether the Tropenmuseum does what it preaches.

**The Ethnographic Museum Should Deal with Issues of Self and Other Simultaneously**

Let me begin by stating that I agree wholeheartedly with Legêne’s mission. Any effective museum needs to address the two issues self and alterity simultaneously. After all, this is what effective ethnography does, too: it is a process of cultural translation, which, like linguistic translation, involves an intimate knowledge of both ‘source’ language (the other) and ‘destination’ language (the self). This process of cultural translation, moreover, is an intense process of self-discovery: by being confronted with the strange (other), questions are raised about the familiar (self). The reason for this is that most people are unaware of the conventions that regulate the ‘self’: ethnographers have called this ‘habitus’. A good ethnographer is able to break down this habitus, although this can be a long and painful process, sometimes called ‘culture-shock’. As a result of this breakdown of the original habitus and then acquiring knowledge of the other’s habitus, the ethnographer becomes able to translate cultural meaning. In her work, then, the ethnographer should try to replicate this process in order to create understanding between the ‘self’ (the ethnographer) and the ‘other’; although it moot point whether this is truly ever achieved or even achievable (cf. Argyrou 2002).

**Auto-ethnography**

Many ethnographic museums fail to create this understanding. Arguably this is because their focus is too much on the ‘other’ and not enough on the ‘self’. We have seen that the Tropenmuseum used to fall in the same
trap itself. Legène’s way out of this conundrum was to ‘put the Dutch on display’, a method she has recently called ‘auto-ethnography’ (Legène 2008).

This term may indeed cover her intentions better, but I am not sure whether the exhibition is actually successful at it. The intention of the exhibit may have been to shift the focus from the ‘colonial other’ to the ‘colonial self’, but I would say that the ‘colonial self’ that is currently being represented is old, from such a long time ago that it has become almost unrecognizably ‘other’. Susan Legène once told a group of her students how another group of students had reacted with disgust at the new exhibition, or rather: the case with anthropometric instruments in front of the mannequin of Charles le Roux: for them these objects were too strongly associated with racism. It seems to me that the students who scolded this display could not recognize ‘themselves’ in it:

Arguably the museum has neglected to bring the idea of ‘auto-ethnography’ full circle and relate it on to the present. It has forgotten that the interaction between ‘self’ and ‘other’ also works temporally, or at least it does so in the modern West. David Lowenthal has argued this convincingly. The title of his book, he derives from a novel by L.P. Hartley: “The past is a foreign country, they do things differently there’, upon which Lowenthal comments

[This] is a perspective of recent vintage. During most of history men scarcely differentiated past from present, referring even to remote events, if at all, as though they were then occurring. Up to the nineteenth century those who gave any thought to the historical past supposed it much like the present.” (Lowenthal 1985:xvi)

As a result of this – despite the best intentions – the exhibition is still focused more on Alterity than it is on self – only the terms have changed.

My first suggestion to the Tropenmuseum is therefore to elaborate on the concept of auto-ethnography. This is a wonderful concept that has yet to be fully explored. Most of this thesis can in fact be considered an attempt at
‘auto-ethnography’ in relation to the 1926 expedition, but this research has only been preliminary and the concept can and should be carried further.

**Reconnecting with Source Communities**

In relation to the new direction that the *Tropenmuseum* is headed, there is a second argument that I would like to make. The current interest in the colonial ‘self’ carries a degree of danger in it. This is that the interest in the ‘self’ turns to narcissism and drive out any discussion of alterity. For this reason, I want to make a second recommendation to the *Tropenmuseum* to create a balanced approach to colonial heritage, which is to create space for the old ‘others’ who used to be the subject of the Colonial Museum. They may no longer be the same natives that they used to be, but their voices still deserve to be heard: they should be allowed to speak back to the museum and its visitors. After all, the museum collections represent a *shared* heritage.

Inspiration for such an approach may come from developments over the last two or three decades in ethnographic museums in countries like Canada, New Zealand, the United States, and Australia. In this period, many of these museums in particular have been forced to rethink their relations with, and attitudes toward the native people of their respective countries; whether they are Maori, Aborigine, native-American, or First Nation (*cf.* Peers and Brown 2003). The Netherlands, too, has seen some effect of the increasing political clout of these people. For example in 2005 the National Museum of Ethnology returned a so-called *Toi Moko* to New Zealand. This beautifully tattooed, mummified head of a Maori chieftain had had a long history in the Netherlands: it started out as a part of the cabinet of curiosities of Willem I, the first king of the Netherlands (1815-1840), a collection that formed the basis for the later National Museum of Ethnography in Leiden. Still, it was returned to the Maori – represented by the new national museum of New Zealand (that adopted ‘Aotearoa’ as an official name alongside New Zealand). The return of this skull to the *Te Papa Tongarewa* amounted to a
gesture of apology for the long suffering of New Zealand’s native population. The state of the Netherlands has returned or is planning to return other objects of value to people that were victims of colonialism. However, what has not yet happened is a process in which people with an extensive stake in Dutch museum collections get involved in the representation of their own heritage – or maybe I should say: a shared colonial heritage.

The buzzwords here are ‘source communities’ and ‘reconnecting’ and there are models to follow (e.g. Herle 2000, 2001, 2003) and even a reader recounting museums’ experiences (Peers and Brown 2003). Some developments in this direction have taken place. The Papuan Heritage Foundation PACE, based in Utrecht, collects and makes an inventory of Papuan material culture and heritage in the Netherlands with the explicit goal of returning this ‘as soon as the political and economic situation permits’ (website www.papuaerfgoed.nl, 23 Dec. 2008). Moreover, the Royal Tropical Institute KIT cooperates with cultural heritage institutions with former colonial ties to the Netherlands – particularly in Ghana and Indonesia.

The *Tropenmuseum* currently has not done a project along the specific lines I envision. In a recent speech, Susan Legêne (2008) did talk about ‘source communities’, but in different sense, as it did not involve a history of colonialism. In the Latin America exhibit is a case on an initiation ritual of the Amazonian *Ticuna*. This tribe had allowed an anthropologist to film this ritual and buy all objects associated with it, which forms the basis for the current exhibit. This is a great example of how modern museum acquisition policy, but still seems somewhat a one-sided in favor of the museum.

The point I would like to make about ‘reconnecting’ with ‘source communities’ is a two-way process. The way Peers and Brown (2003) describe it in their reader, it is a kind of ritual exorcism for driving out bad spirits associated with the colonial past of the museum. An excellent example of this process is the commemoration in Cambridge of the centenary of the seminal 1898 Torres Strait Expedition. In 1998, the anthropology department of the
University of Cambridge held a symposium on and published an edited volume reassessing and commemorating the expedition (Herle and Rouse 1998); which is all as may be expected on such an occasion. However, the Cambridge Museum of Archaeology and Anthropology also organized an exhibition called *Torres Strait Islanders*. For this exhibition it actively involved the ‘source community’ – the ‘source’ of the 1898 expedition’s collections – in the exhibition-making process (Herle 2000, 2001, 2003). Curators from Cambridge went back to the Torres Strait, thus making this truly a kind of ‘revisiting expedition’ of the 1898 expedition. Moreover, people from the Torres Strait had an opportunity to explore the Torres Strait collections, which is their heritage (too?), held by the Cambridge Museum. These collections did not just consist of ethnographic objects, but also photographs, sound recording, diaries full of observations of past daily life, *etc*., much like the collections of the 1926 expedition. An initiative along these lines is not just an opportunity for the museum to deepen and up-date its knowledge of the collections, it can also be very rewarding for the people who were once the museum’s subjects – who were measured, pictured, and collected and then mounted on display – to receive a ‘visit back’.

In the case of the Tropenmuseum, once again the 1926 expedition could provide an excellent opportunity for reconnecting with source community and thereby exploring Dutch colonial heritage through the eyes of the ‘other’. In the case of the 1926 expedition, two scenarios can be envisioned. The first scenario is the obvious one: to pick an area along the route of the 1926 expedition, for example one of the camp locations where much collecting was done: *Albatrosbivak* or *Motoreindbivak* or, naturally, the ‘pygmy’-village Tombè in the Nògòlò-valley. However, the process of reconnecting with these Papuans would be arduous and require intensive fieldwork, particularly since this area along the upper-Taritau (Rouffaer) river is one of least developed in an already underdeveloped province. Moreover, the area is still more or less ethnographic *terra incognita* (Brown
1979:97; van Baal et al. 1984:95), yet the attempt may ultimately be very rewarding.

An alternative scenario would involve reconnecting with some of the ‘other’ participants in the expedition. In particular, it would be very interesting – and feasible – to return to the villages of the Dayaks who were employed as rowers and carriers not just in the 1926 expedition, but in virtually every New Guinea expedition from 1907 onward. Anji Ipoei and Taman Lendah were two of the Dayak village chiefs who accompanied Stirling and his colleagues in 1926, but at this time they were already true veterans of the exploration of New Guinea. Three times Ipoei and Lendah had rowed up the Mamberamo and its tributaries: in 1914, 1920-21, and 1926. Undoubtedly this long engagement with New Guinea, by people from a relatively small group of villages along the upper Kayan river, has left its marks there – marks that may well still be visible. The Dayak surely made a great impression on the Dutch and American expedition members. They feature prominently in the photos, film and diaries of the 1926 expedition. Furthermore, the Matthew Stirling New Guinea ethnographic collection in Washington, DC, has a surprising number of Dayak artifacts, although the Dutch sub-collections related to the expedition have not at present yielded any. Ipoei and Lendah received decorations for their efforts, one explorer, Hendrik Lorentz, thanked and named each and every Dayak on his expedition in an appendix to his book Zwarte Mensen – Witte Bergen (Lorentz 1912 [2005]) Similarly, memories and mementoes of this time may well remain in Borneo, although after eighty years it is unlikely there many are surviving veterans. Chris Ballard’s experiences among the Amungme visited by Wollaston (Ballard, Ploeg and Vink 2001) or Joshua Bell’s experiences among the Iai (Bell 2003) may serve as examples here.
Figure 14 Dayak rower on the quai of Macassar before embarking for New Guinea
ER102n (Taylor 2006)
Currently, such a project is still hypothetical – although I am working at making it happen. However, it would fit very well in the current preoccupations of the *Tropenmuseum*, bridging as it does the issues of self and other, and the heritage of colonialism. Moreover, such a project needs to consider its medium: it could result in a book or a film, but the best medium would still be the museum, because this project is in fact an exploration of the museum’s self. An exhibition about reconnecting with source communities – if done well – could make the museum achieve self-reflexivity, or ‘auto-ethnography’ according to Legêne (2008). It allows the exploration of the ‘self’, by confronting questions like: why did ‘we’ – our (grand-)parents – go there? And how did that affect ‘us’ today? At the same time this allows exploring questions of Alterity: What did the ‘other’ mean, then and now? How did the ‘other’ see ‘us’ then and now? In particular answering this last question is essential to completing Susan Legêne’s project of ‘auto-ethnography’: indeed, what better exploration of the self than the mirror?
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Figure 15 Scene of a flooded Motor Camp Arb205n (Taylor 2006)