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David's Weapon of Mass Destruction: *The Reception of Thor Heyerdahl's 'Kon-Tiki Theory'*

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From the late 1930s to his death in 2002, Norwegian adventurer and amateur ethnologist Thor Heyerdahl struggled to find academic acceptance for his Pacific Islands settlement theory. He even went as far as using the biblical story of David and Goliath as a metaphor for his struggle against academia. However, there are numerous reasons to question the accuracy of Heyerdahl's description of his relationship to the scientific community. This paper discusses the reception of Heyerdahl's 'Kon-Tiki theory' among Pacific scholars in the late 1940s and early 1950s. By analysing contemporary reviews of Heyerdahl's 1952 book *American Indians in the Pacific* and comments on early drafts of the theory, this paper demonstrates that the material substantially differs from Heyerdahl's own claims. He was not excluded by the Pacific scientific community, but welcomed and encouraged. Above all, reviewers of Heyerdahl's theory praised the importance of the challenge he had posed to the established research narrative. However, Heyerdahl's academic amateurism failed to convince the scientific community of the accuracy of his theory.

Introduction

From the late 1930s to his death in 2002, Norwegian adventurer and amateur ethnologist Thor Heyerdahl (1914–2002) relentlessly struggled to find acceptance for his Pacific migration theory (henceforth the 'Kon-Tiki theory'). His quest for the theory's acceptance had brought him world fame in 1947 through the *Kon-Tiki* experimental raft expedition (Heyerdahl 1948). Heyerdahl also became a central figure in Pacific archaeology in the mid-1950s, when he arranged a large-scale archaeological project on Rapa Nui in search of supportive evidence for his theory (Heyerdahl & Ferdon (eds.) 1961, 1965).

The theory, published in full in Heyerdahl's 1952 book *American Indians in the Pacific: The theory behind the Kon-Tiki expedition* (henceforth *American Indians*), claimed that the first settlers of the Pacific island world, in stark contrast to established scientific tradition, had not been of Asiatic origin, but in fact white god-like men reaching Polynesia from South American shores.

Naturally, Heyerdahl's theory caused friction with the Pacific archaeology/anthropology community as it went in literally the opposite direction to the established research narrative. However, Heyerdahl was most definitely not a man to shy away from a fight and quite often turned his discussions and arguments with Pacific scholars into theatrical public performances. In his 1998 autobiography Heyerdahl used the well-known biblical story of David's struggle against Goliath as a metaphor for his relationship with the scientific community (Heyerdahl 1998:

170–214). The theme was hardly new and had already been adopted by Heyerdahl as a narrative driving force in the 1940s (e.g. Heyerdahl 1948: 7–17). This particular narrative of a lone crusader against a dogmatic establishment is a well-known cliché of political propaganda, and is commonly used, for instance, by conspiracy theorists to create mistrust of the adversary of the author/speaker (Castanho Silva et al. 2017; deHaven-Smit & Witt 2012; Imhoff et al. 2018; Sutton & Douglas 2014).

Heyerdahl's repeated use of this narrative unavoidably calls into question the reliability of his statements. There are also numerous other reasons to treat Heyerdahl's statements with careful consideration (e.g. Spriggs 2014). For instance, Heyerdahl's friendly and often life-long correspondence with Pacific archaeologists/anthropologists such as Kenneth Emory (1897–1992) and Henri Lavachery (1885–1972) suggests otherwise. In 1961, Heyerdahl was elected a member of the board for the *Polynesian Archaeology Program* which came to have a substantial impact on archaeological research in the Pacific region for much of the 1960s (Green 1961). He was also continually invited to present his thoughts at academic conferences and in scientific journals, even though he lacked an academic degree (Melander *in prep.*; see also Bakke 2017: 28–33, 134).

Aspects like these strongly question Heyerdahl's polarised description of his relationship with academia. It also poses questions on how his theory was received in its own time. The purpose of this paper is to examine the reception of Heyerdahl's theory, above all by looking at scientific reviews of *American Indians*, but also by looking at how earlier drafts of the theory were received in the 1940s. This makes it possible to view the criticism of Heyerdahl's Kon-Tiki theory beyond his own lone crusader narrative.

Due to Heyerdahl's international recognition as an adventurer, the Kon-Tiki theory, like Heyerdahl's work in general, was reviewed in numerous different types of journals, newspapers and popular magazines publications. Since the purpose of this paper is to discuss his interaction with the scientific community, only reviews and comments presented by professional scholars in related fields and reviews in established scientific journals have been considered.

The Kon-Tiki Theory

Even though Heyerdahl might well be the best-known 20th-century figure to present a theory on the settlement of Polynesia, it is surprisingly common to find confusion over the actual content of his theory. Misconceptions of the theory generally state that Heyerdahl argued that the Pacific island world had been settled by 'Amerindians', 'Peruvians', 'Incas' or 'South Americans' (e.g. Langdon 2001: 70; Skolmen 2000; Thorsby 2012). In fact, Heyerdahl did nothing of the sort, a problem that was pointed out already in the 1970s (Schuhmacher 1976: 807). It is thus necessary to commence with a short recapitulation of the Kon-Tiki theory's content.

Discussion on diffusionism and possible cultural connections between Polynesia and the Americas was not a novelty at the time Heyerdahl formulated his theory. It had been suggested already by James Cook, and is still discussed to this day (Dixon 1932; Ellis 1829a–b; Emory 1942; Kahn & Kirch 2014: 35; Nordenskiöld 1931; de Zuñiga 1814; see also Jones et al. (eds.) 2011). However, after the early 19th century it was extremely rare for such discussions to include suggestions of an American origin for Pacific peoples. It is in this regard that Heyerdahl's theory starts to deviate from the established research narrative.

The essence of the theory Heyerdahl presented in *American Indians* was the division of the settlement of Polynesia into two different migration waves. Heyerdahl argued that the Pacific island world had first been settled by a people referred to as the 'white bearded men': a highly civilised Caucasian race group defined by their long-headed crania, fair skin, blue eyes, fair or red hair, tall stature, and beards, as well as their navigation skills, stone carving abilities and sun-worshipping culture. Heyerdahl placed the white bearded men's origin across the Atlantic and claimed that they had created the high civilisations of Central and South America. A branch of this people had been forced out of their homeland around Tiahuanaco, in modern Bolivia, around the year AD 500, and taken refuge in the Pacific Ocean under the leadership of the culture-hero Con Ticci Viracocha (Heyerdahl 1952: 179–425, 621–764; see also Heyerdahl 1949a, 1950a–b, 1968, 1978).

The white bearded men settled and lived in Polynesia for some 500 years before their position was threatened by a new migration wave. This second wave of migrants, whom Heyerdahl called the 'Maori-Polynesians', were of Asian origin and spoke an Austronesian language. They had migrated out of their original homeland in Southeast Asia in a remote past and spread out over parts of East Asia, finally crossing a land-bridge, existing at the time in the Bering Strait area, into the American continent. According to Heyerdahl, the Maori-Polynesians were of the 'yellow-brown race', a racial

admixture between the original Asian 'Indo-American race' and the later 'Mongoloid race'. They had reached Polynesia at a late date around AD 1000/1100 from the American Northwest Coast (Kwakiutl area of British Columbia). The Maori-Polynesians were fishermen specialising in wood-carving and had a 'warrior spirit'. Heyerdahl argued that they were inferior to the white bearded men in all respects. Nonetheless, upon first contact between the groups, the Maori-Polynesians quickly either eliminated or assimilated the white bearded men (Heyerdahl 1952: 69–216, 709–764). The Kon-Tiki theory suggested that the Pacific island world was settled by two distinctly different migration waves, both coming via the Americas.

American Indians is most accurately described as an amateur ethnological study. Heyerdahl's aim was to use a cross-disciplinary approach to assemble all existing data from various fields in order to define and follow the movements of various ethnic groups (Heyerdahl 1952: 8–9). These ethnic groups were defined through a biological determinist type of reasoning where material culture, cultural practice, language, and intellectual capacity were connected to biology, forming various 'race nations' with associated monuments, behaviours and abilities (Melander *in prep.*).

Heyerdahl's valuing of these race nations established a hierarchical order. So-called aspects of high civilisation (written language, navigation, societal organisation, monument building, etc.) were associated with the white bearded men, while violent, superstitious and primitive behaviour was associated with the Maori-Polynesians and the Melanesians (e.g. Heyerdahl 1952: 187). This value hierarchy between superior (white) and inferior (brown and black) people has led recent studies to accurately emphasise racism as a key factor in Heyerdahl's work (Andersson 2010; Engevdold 2013; Holton 2004; Magelssen 2016; for further discussion on racism in colonial anthropology/archaeology frameworks see Ballantyne 2002; Ballard & Douglas (eds.) 2008; Ljungström 2004: 21–30; McNiven & Russell 2005).

The Theory's Origin and Development

Heyerdahl's tendency to modify events of his life into an almost mythological narrative made historian Axel Andersson label him as a 'mythographer' (Andersson 2007: 12–13, 23–44). The term is intriguing and accurately portrays Heyerdahl's autobiographical writing, especially after the mid-1960s when Heyerdahl and his friend and biographer Arnold Jacoby (1913–2002) reinvented Heyerdahl's life story. In this case, archival material presents a different image of what actually transpired than the statements made in post mid-1960s biographical and autobiographical writing. This is of relevance for this paper as it illustrates the literary liberties Heyerdahl took in his writing: changing chronologies, exaggerating and excluding contexts – mythologising. His earlier autobiographical writing (up to 1952) shows greater consistency with the archival material, even though the abovementioned tendencies are already present in Heyerdahl's first publications from the late 1930s.

In his early writings Heyerdahl continually returned to his 1937 expedition to the Marquesas Islands as the

decisive factor for his interest in Pacific migrations. He had first come up with his Kon-Tiki theory during this expedition (Heyerdahl 1948: 7–17, 1952: 9). In later writing, Heyerdahl instead claimed that he had travelled to Polynesia in 1937 to test his Kon-Tiki theory, which he had already developed in the mid-1930s (Heyerdahl 1974, 1991, 1992, 1998; Jacoby 1965). The early narrative is supported by the archival material, which illustrates that Heyerdahl had no knowledge of or interest in Pacific history or archaeology prior to his 1937 expedition to the Marquesas (Melander *in prep.*). The expedition, undertaken together with his then wife Liv Torp-Heyerdahl (1916–1969), changed this. The couple travelled to the Marquesas to collect zoological samples and to 'return to nature' (Anon I. 1936; Heyerdahl 1938, 1941a; Melander 2017: 78–79). However, in the Marquesas, they came face to face with the monumental archaeological remains of East Polynesian culture. Heyerdahl found it unthinkable that these spectacular monuments had been made by 'the docile brown people' of Polynesia (e.g. Heyerdahl 1974: 209). The monuments must have been made by someone else, he argued, someone more industrious, similar to the Norwegian copra farmer and Marquesas Islands settler Henry Lie (Heyerdahl 1974: 209; see also Heyerdahl letter to Bjarne Kroepelien, 20 October 1937, quoted in Skolmen 2010: 258–261). He subsequently separated the Polynesians from their own history and instead claimed that the monuments had been made by a pre-Polynesian people called the 'temple people' (Heyerdahl 1938: 69–70).

In the latter part of 1938, Heyerdahl was by coincidence introduced to the material culture of the Pacific Northwest Coast, and seemingly immediately connected it to the Polynesians.¹

His first presentations of what would become the Kon-Tiki theory were crude poster-style manuscripts, heavily influenced by adventure stories in *National Geographic Magazine* and by Scandinavian amateur researchers such as Halfdan Bryn and Waldemar Dreyer (Melander *in prep.*; compare Heyerdahl n. d. a., and n. d. b. to Bryn 1925; Dreyer 1898; McMillin 1927; Simpich 1929, 1930; Tschiffely 1929). None of these were ever published.

In late 1939 Heyerdahl travelled to North America to pursue his research aims and spent most of the early 1940s in libraries in the US and Canada, searching for supporting evidence for his theory.² In 1941, Heyerdahl published the theory for the first time in the inaugural number of the scientific journal *International Science* (Heyerdahl 1941b). The paper presented the theory in a premature state. It included the division of the settlement of Polynesia into two different migration waves, one from Tiahuanaco and one from the Pacific Northwest Coast, but several of Heyerdahl's later key elements were still missing from his argument (compare Heyerdahl 1941b to Heyerdahl 1952). Above all, the paper, presented without a bibliography, standard academic formalities and even headings, illustrates Heyerdahl's lack of formal training. The sources mentioned in the paper also suggest that Heyerdahl had very little knowledge at this stage about Pacific and South American archaeology. However, he had evidently started to familiarise himself with research literature on North American archaeology during his time in Canada.

As for so many others, the Second World War limited Heyerdahl's possibilities to pursue his research ambitions in the early 1940s. Nonetheless, he did not relinquish his studies; even when serving in the Norwegian army, he seized every opportunity he could to study physical anthropology and Polynesian and South American archaeology.³ It was during this period that he was introduced to the writings of novelists Lewis Spence (1874–1955) and Alpheus Hyatt Verrill (1871–1954) (Heyerdahl n. d. c.). Both Spence and Verrill enthusiastically argued that South and Central American mythology suggested that the civilisations of the Americas had been created by white god-like men of transatlantic origin (Spence 1907, 1913; Verrill 1929, 1953).

After the War, Heyerdahl returned to the US to pursue his studies.⁴ In 1946 he finalised a reworked draft of the theory that he had been working on since his *International Science* paper.⁵ The draft, entitled *Polynesia and America* (Heyerdahl n. d. c.), runs to around 600 handwritten pages and presents the Kon-Tiki theory in a similar fashion to its later appearance in *American Indians*. However, the amount of supporting arguments and material was still limited in relation to the later publication of the theory. The *Polynesia and America* manuscript was sent out to some of the leading scholars of archaeology and anthropology in the US and Canada (see further below). Heyerdahl also tried to have it published in *National Geographic Magazine* but was rejected.⁶

Correspondence material from the period suggests that Heyerdahl's failure to gain recognition for his theory made him frustrated and hostile towards the scientific community at large. Heyerdahl seems to have thought that he would only be able to get attention for his ideas if he made a name for himself through a major event.⁷ Subsequently he started drawing up the plans for the *Kon-Tiki Expedition*, which in 1947 would bring him worldwide fame. However, the success of the expedition forced him to postpone his attempts to have his theory published until 1952.

The Reception of the Theory Prior to the Publication of *American Indians*

Heyerdahl's aggressive response to the academic community's lack of interest in him and his theory might have been a reaction to the fact that he had already encountered criticism from the start. While doing research in Canada in 1940, the topic of his research slipped into the local newspapers and was commented upon, surprisingly enough, by none other than the famed anthropologist Margaret Mead (1901–1978). Mead's criticism of his conclusions proved positive for Heyerdahl, since it provided him with the opportunity to publish his preliminary results in *International Science* (Kvam 2005: 258). It is unlikely that this paper ever reached any wider audience, and there is no known criticism published on it. However, the paper did include an endnote in which the editorial board made sure to state that they did not share the ideas expressed by the author. They had published the paper to encourage debate on the subject (see Heyerdahl 1941b: endnote on p. 26).

According to Heyerdahl, his second attempt at getting the theory published in 1946 was snubbed (Heyerdahl

1948: 7–17, 1998: 170–214). This can be described as a truth with modification. It is true that Heyerdahl did not find any acceptance for his theory, but his expectations might have been too high. He was at the time an unknown former soldier with no academic degree. In that context, it is actually surprising that he was able to convince prominent scholars to read his manuscript. The *Polynesia and America* manuscript was sent in the Autumn of 1946 to Marius Barbeau (1883–1969) at the National Museum of Canada, Faye Cooper Cole (1881–1961) at the Field Museum in Chicago, and Ruth Benedict (1887–1948) at Columbia University in New York City.⁸

It is uncertain whether Benedict ever read the manuscript.⁹ Barbeau and Cole, on the other hand, did read the manuscript and sent Heyerdahl feedback on it. Cole's comments seem to have been mostly encouraging, praising Heyerdahl for the research he had done. But he also suggested some major revisions were needed before Heyerdahl sought to publish the manuscript. Above all, Cole objected to the selectiveness of Heyerdahl's argumentation. He wrote that Heyerdahl needed to pay more attention to material that did not support his theory, to avoid becoming a 'special pleader.'¹⁰

Barbeau's response was similar. He praised the research Heyerdahl had done, and seemingly did not object to Heyerdahl's white bearded men idea. On the other hand, when it came to Barbeau's own field of specialisation, North American archaeology, he was very critical of Heyerdahl's claims. Barbeau thought that Heyerdahl had misunderstood the material he quoted, jumped to conclusions and lacked knowledge on the current state of research on the topic. He wrote that the theory in its present state was unacceptable.¹¹ In the correspondence with Barbeau it is interesting to note that Heyerdahl appears out of character, with a more humble attitude than he otherwise expressed in the period. Heyerdahl was grateful for Barbeau's comments and assured him that he would expand on his bibliography and make adjustments to his theory.¹²

With the success of the *Kon-Tiki Expedition* in 1947, Heyerdahl's theory became known to the general public. It also started to receive criticism (as well as praise) from various directions. Perhaps the most noted figure to oppose the theory was leading Polynesian anthropologist Peter Buck/Te Rangi Hīroa (1877–1951), who briefly commented on its improbability in a local New Zealand newspaper (Anon. II. 1949). Heyerdahl was also criticised by American archaeologist Ralph Linton (1893–1953) in an interview with the Norwegian newspaper *Aftenposten*, which Heyerdahl felt obliged to respond to (Heyerdahl 1949a). However, the major debate about the *Kon-Tiki Expedition* and its theoretical background took place in various Scandinavian newspapers. Above all, Heyerdahl had to defend his theory against criticism from the botanist and Pacific expert Carl Skottsberg (1880–1963), the ethnologist and South America expert Rafael Karsten (1879–1956) and the archaeologist Stig Rydén (1908–1965), towards whom Heyerdahl would later develop a fierce personal antagonism (Evensberget 1994: 102–108; Heyerdahl 1949b–e, 1950c–d; Kock-Johansen 2003: 35–41; Karsten 1949a–d; Kvam 2008: 60–73; Rydén 1949, 1950; Salomaa

2002: 91; Skottsberg 1949a–b). Rydén's and Skottsberg's criticism of Heyerdahl's theory was in many ways similar. They pointed to compromising details which brought into question whether Heyerdahl was familiar with the research literature at all. They questioned Heyerdahl's selective and speculative reasoning, arguing that it led to erroneous conclusions and a theory that was, at least to Rydén's mind, absurd – Skottsberg seems to have been under the same impression but expressed himself more diplomatically (see Rydén 1949, 1950; Skottsberg 1949a–b). For Heyerdahl the criticism was devastating. Skottsberg had done fieldwork on Easter Island, where Heyerdahl had not yet been, and knew the Polynesian research literature; he was also a botanist with the Pacific as his field of expertise. Rydén was a noted South America expert, for instance, he and Wendell Bennett (1905–1953) were the only ones who in recent times had undertaken excavations in Tiahuanaco (Rydén 1949). Heyerdahl's theory had thus been dismissed by internationally recognised experts on both Pacific and South American material before he had a proper chance to publish it. In both cases, and even more so against Karsten, Heyerdahl defended himself by lengthy and repeated replies in various newspapers (Heyerdahl 1949b–e, 1950c–d). His replies emphasised that the criticism Rydén, Karsten and Skottsberg had targeted against the *Kon-Tiki Expedition* travelogue and his theory was unfair. Heyerdahl argued that the travelogue was a popular book and that he had not yet been given the opportunity to present his theory in a proper scientific publication. This argument was accepted by both Skottsberg (1949a–b) and Rydén (1949, 1950), who encouraged Heyerdahl to proceed with an academic publication of his theory.

Especially in the biographies and Heyerdahl's own writing the context for these debates has been omitted, making it seem as though leading scholars more or less jumped on Heyerdahl before he had a chance to fully present his theory (e.g. Evensberget 1994: 93–112, 164–170; Heyerdahl 1998: 170–214; Jacoby 1965: 231–253; Kock-Johansen 2003: 38–41). It is important to understand that these debates originated in the Swedish Geographical Society's decision to award Heyerdahl a medal for scientific merit. The decision was controversial as Heyerdahl lacked an academic degree and had no scientific publications to support his claims (Anon. III 1950; Anon. IV 1950; Bolinder 1950a–c; Selling 1950a–b). In consequence, the debates were strongly polarised over whether Heyerdahl's ideas could be considered as science or not. The criticisms presented by Skottsberg, Linton and Rydén, for instance, were therefore not directly targeted at the *Kon-Tiki Expedition* but at the Swedish Geographical Society's arguably poor judgement.

The Reception of American Indians

Since Heyerdahl's work transcended the borders of different scientific fields and was above all a part of popular culture, its reception has of course extended across various journals, books, and newspapers. In popular biographies of Heyerdahl, *American Indians* has generally been listed as a great achievement and important step in his career. The biographies either directly or indirectly referred to the

book as a 'weapon' (Kvam 2008: 80; Evensberget 1994: 106) and measured the book's magnitude by its volume, listing the number of pages and references used (see Jacoby 1965: 241–242; Kvam 2008: 98). Jacoby even wrote, 'The book was of such magnitude that it was a hard task even for professional researchers to read it' (1965: 242¹³). The biographers described Heyerdahl's achievement as resting in the book's physical size rather than its actual content; they highlighted quantity over quality. In this sense *American Indians* is of course an impressive book; the author's personal copy, for instance, weighs more than 2.5 kilos and measures 25 × 20 × 6 cm. However, the number of pages (821), weight, size, and number of references used actually says very little about the content of the book.

An overview of the reception of Heyerdahl's theory in the early 1950s can be obtained from contemporary reviews published either by scholars active in the field of Pacific archaeology or anthropology, or in established scientific journals. As noted above, Heyerdahl's books were reviewed in a variety of journals and contexts not commonly seen for scientific works. This analysis has focused on established scholars and academic journals to highlight how the theory was received in contemporary scientific discourse rather than among the general public. For such reviews it is important to keep in mind the scope of the genre. Scientific reviews are generally written in a polite tone, balancing praise and criticism, in a way that is not necessarily used for reviews of literary works. In Heyerdahl's case there are a few obvious exceptions to this principle. For instance, a review by Rydén on Heyerdahl's and Arne Skjølsvold's (1925–2007) archaeological work on the Galapagos Islands (Rydén 1958; see also Heyerdahl & Skjølsvold 1956) can be said to have stepped so far out of bounds that archaeologist Clifford Evans (1920–1981) felt forced to review Rydén's review (Evans 1958). However, this is a rare exception from a time after the publication of *American Indians*. Biased opinions were also expressed in the opposite direction. Heyerdahl's close friend botanist Olof Selling (1917–2012), for instance, declared in the Norwegian magazine *Nå* that:

Pacific research has never before seen a work [*American Indians*] with such a detailed and nuanced comparison. A massive volume, tight and clearly written for the scientific audience, but at the same time enjoyable for the general public. Filled to the limit with dramatic content, as exciting as a detective story (Selling – quoted in P. L. E. 1952).¹⁴

Details and Selective Argumentation

Even if Selling's praise was not repeated in other reviews, the overall reception of *American Indians* was much more friendly and positive than Heyerdahl made it seem in later life. However, reviewers did point out the impossibility of being an expert in all fields (e.g. Bennett 1953). For instance, a review by New Zealand archaeologist H. D. Skinner (1886–1978) ended by criticising a 'small detail', namely Heyerdahl's discussion of the distribution of *patu*. Skinner pointed out that Heyerdahl had completely misunderstood the distribution, was not at all familiar with the research literature, and that his illus-

trations of supposed *patu* also included several adzes, pounders, and other objects (1953). This very illustrative review of a 'small detail' reveals the weakness of the superficial cross-disciplinary approach to the material and science at large in Heyerdahl's book, and connects back to the criticism Heyerdahl had received for earlier drafts and publications.

Heyerdahl's limited knowledge of details, and above all the selective way in which he approached these details, was the main recurring theme in the reviews, generally comparing it to Heyerdahl's over-enthusiasm for his own conclusion. Archaeologist Gordon Ekholm (1909–1987) wrote:

An extraordinary amount and variety of anthropological, historical, and geographical evidence has been gathered together by Heyerdahl to validate this thesis of Polynesian origins. His attempt to bring the findings of diverse disciplines into focus on a major problem is a commendable procedure, but in many ways he has allowed his enthusiasm for his "theory" to cloud his judgment of conflicting evidence. Marshalling all possible support for his contentions, he minimizes or neglects evidence that should lead to an opposite view in a manner that leans toward the legalistic (1954: 308).

A similar type of statement was made by Linton, who wrote:

The author's unquenchable enthusiasm for his theories is evident on every page. Again and again the "possibility" cited in one paragraph becomes a "probability" in the next and an established fact half a page later. Another book half the size of this would be required to deal with his evidence adequately (Linton 1954: 123).

In his review, anthropologist Edward Nordbeck (1915–1991) raised exactly the same type of problem:

Numerous objections may be made to this work. The treatment throughout is opportunistic. Every straw is seized, bent and twisted to suit the author's purposes. Tenuous evidence is pushed beyond reasonable limits; conflicting data are given scant attention or omitted, and the manuscript abounds with incautious statements. The author is both ingenious and ingenuous, and verbal magic is a recurrently used tool. Even the reader who, like this reviewer, is only modestly informed on the areas concerned may find many hundreds of points which he will question or reject (1953: 93).

Two recurring subjects raised by reviewers to highlight Heyerdahl's selective approach were Peruvian pottery and linguistics. Several reviewers mentioned Heyerdahl's contradictory argument that it was impossible that Malay-Indonesian groups had lost the ability to make pots, but reasonable to assume that Peruvian groups could have lost their ability to make pots (Bennett 1953; Ekholm 1954; Firth 1953; Linton 1954; Skinner 1953).

Since linguistics was an essential part of the Pacific archaeological/anthropological discourse at the time (e.g. Buck 1945), it is not surprising that Heyerdahl's cavalier and uninformed approach to the discussion was seen as very compromising by reviewers (Bennett 1953; Ekholm 1954; Linton 1954; see also Schuhmacher 1976). Linguist A. S. C. Ross (1907–1980) even felt obliged to make a special remark in his review in *Nature* on Heyerdahl's 'omitting' of the Malayo-Polynesian language connection (1953).

The Reception of Heyerdahl's Race Views in the Early 1950s

As mentioned above, in recent years criticism has above all been directed towards the racial content of Heyerdahl's theory (e.g. Andersson 2007, 2010; Holton 2004). In this case it is important to note that hardly any of the reviewers in the 1950s were particularly concerned by or interested in this aspect. Apart from a few comments on incorrect use of craniological data, there were few objections towards the racial content in *American Indians*. The only exception to this pattern is found in Nordbeck's review. Somewhat selectively, Nordbeck's review is more or less the only one to be referenced and quoted in modern criticism (see Andersson 2007: 189; Holton 2004: 163). This selectiveness is problematic since the use of the exceptional as representative for the period creates an inaccurate image of views at that time. In the actual review, Nordbeck wrote: 'It will be difficult for many persons to avoid reading racism from this work' (1953: 93). He also mentioned Heyerdahl's use of mental traits as a dubious approach and questioned Heyerdahl's quick assumption that the Negroid element in the Polynesian cranial material was a result of Melanesian slaves being brought to Polynesia (Nordbeck 1953: 93).

As stated above, Nordbeck's review is an exception; the only other reviewer to briefly address possible race issues was Erik K. Reed (1914–1990). Reed praised Heyerdahl's discussion of a possible Pacific Northwest Coast origin for the Polynesians, but was more reluctant to accept the South American migration wave, questioning especially Heyerdahl's discussion on white sun-gods (Reed 1953: 309). In this case the Northwest Coast archaeologist M. W. Smith (1907–1961) actually argued in direct opposition, finding Heyerdahl's North American connection unlikely but the South American one believable (Smith 1953; see above for a similar view expressed by Smith's peer Barbeau). It should be pointed out here that Smith retracted this position only a few years later. In a review of Heyerdahl's popular book *Aku-Aku* (1957), she criticised not just the South American part of Heyerdahl's migration theory, but also its racist implications (Smith 1958: 386).

Nonetheless, summarising the reviews, it is hard not to point to a fairly general acceptance of the type of racism expressed in *American Indians*. The entirely justifiable debate on this issue from a present perspective cannot be said to have had the same relevance in the 1950s. The type of racism which characterises Heyerdahl's Kon-Tiki theory was seemingly a non-issue in the early 1950s.

The Challenge as a Consolatory Bone

Even though there was considerable opposition to Heyerdahl's uncontained enthusiasm and selective

approach, the same qualities were also praised, and all reviews can be said to end on a rather positive note. H. S. Harrison, for instance, wrote:

It will perhaps be agreed that the book represents a venturesome attempt at navigation against prevailing winds and currents of diffusional theory and speculation. It is natural that the theory should be accused of the defects inherent in novel propositions of such magnitude – namely, those of enthusiastic selective bias, coupled with an unconscious disregard of contrarities (1953: 47).

In his review Nordbeck made sure to state that his concluding praise should not be viewed as a standard of the genre, but as a true expression of appreciation:

It is a common practice in reviews of works which controvert prevailing theory to throw the author a consolatory bone by saying that his work stimulates reexamination of the problems and theories in question. I do not think the author's theories are so violently in opposition to general anthropological opinion as he appears to believe, and I wish to do more than throw him such a bone. Finally, I view this work as a contribution and wish its author good speed in his present venture in the Galapagos Islands (1953: 94).

Even though Nordbeck did not find Heyerdahl's theory to be 'violently in opposition' to the established narrative, Heyerdahl's determination to approach the question from a different perspective and his courage to stick with a different view were generally well received, appreciated and encouraged in almost all of the reviews. Smith, for instance, wrote:

At his best, Heyerdahl writes with well-considered caution. He shows himself capable of the best use of data and argument. At his worst, he uses doubtful sources excessively while omitting standard ones: thus error and weakness tend to pile upon one another. Yet always there is the broad vision. And to follow his exploits in expounding his theory is as exciting – and as nerve wracking – as if one had ventured with him on to the raft Kon-Tiki. His greatest contribution up to the present time, and it is not to be underrated, lies in the challenge to the experts which he so magnificently formulated on the basis of their own best methods (1953: 476).

Heyerdahl's major contribution to the discourse was not presenting a solution to the problem, but providing a challenge through his theory: a challenge not to take anything for granted, and to further develop and argue for the existing research narrative. The scientific reviews of *American Indians* echoed the comments the editorial board of *International Science* had expressed a decade earlier. Heyerdahl's theory was unreasonable but the challenge it provided was reasonable.

David or Goliath? Concluding Remarks

Heyerdahl's David and Goliath narrative can hardly be said to be verified by the reviews. His attempt at taking a reversed theoretical position was not scorned but celebrated by reviewers and critics. Reed and Ekholm praised Heyerdahl's chapters on Peruvian navigation techniques (Ekholm 1954; Reed 1953); Smith (1953) spoke admiringly about the importance the *Kon-Tiki Expedition* could have for anthropology (see also Evans 1958); Skinner (1954: 83) stated that the publication of *American Indians* was 'an important event in the study of Polynesian history'; and Bennett wrote: 'The quantity and quality of the materials which Mr. Heyerdahl has assembled are too great to be ignored. Henceforth, American contributions to the Polynesian cultures will have to be considered' (1953: BR1). Heyerdahl's theory was not greeted with intense hostility, but welcomed into the Pacific discussion with friendly encouragement. Putting the Americas back on the map as a possibility for Polynesian origins was received as an appropriate question. In this sense *American Indians* was most definitely a success. However, none of the reviewers had been convinced by Heyerdahl's theory; his methodological approach was questionable, his argumentation speculative and one-sided, and his lack of knowledge on important details was compromising. As Harrison (1953: 48) stated in his review, this was a new start for the discussion, not the final word.

With *American Indians*, Heyerdahl had evidently not been able to address concerns raised along the way during his roughly decade-long attempt to have the theory published. His lack of formal training was as evident in 1952 as it had been in 1941. This is perhaps most unambiguously expressed in the fact that Heyerdahl's more than 800-page argument for the theory lacked not just a summary, but also a concluding chapter – something reviewers did not fail to note (Nordbeck 1953; Reed 1953). Heyerdahl remained an analytical amateur. In the decade that had passed since 1941 he had greatly expanded on the material, but failed to develop his argument and his own skill set.

American Indians was definitely a step forward for Heyerdahl, but it had not become the weapon of mass destruction he had wanted it to be. He had received acceptance for the question's validity, the door had been opened, but his amateurism had made him unable to convince anyone to step inside. And as Pacific archaeology in the period moved into a phase more intensely focused on excavations (e.g. Kirch 2000: 12–41) – a development in which Heyerdahl himself played a major role – the likelihood of the theory became more and more distant each time a shovel broke new ground. The theory never came to have any impact on Pacific archaeology other than increasing the efforts towards finding new and more sustainable data to support the established out of Asia narrative. As most of the reviewers remarked, Heyerdahl's major accomplishment was posing the challenge. Even though the risk of exaggeration is evident, the influence this challenge had should not be underestimated either. Fuelled by Heyerdahl's fame, the question of Pacific migration patterns became known worldwide and

attracted scholars across the globe to 'prove Heyerdahl wrong', as historian Greg Denning expressed it (2004: 47). As a result, the question of Pacific migration routes moved from being a discussion about the existence of sunken fantasy continents to become, some 60 years later, one of the best-documented prehistoric migration processes in human history. Heyerdahl could be said to have mistaken his role in the narrative; he was not David but Goliath, the champion of the Philistines, the challenger, forcing the established research narrative to strengthen its argument.

Notes

- ¹ Bell, D. n. d. 'Untitled Interview with Thor Heyerdahl'. Kon-Tiki Museum Archive. Oslo. Reel 5/Side 1/p.3–4; see also Jacoby 1965a: 79–99; Kvam 2005: 227–247.
- ² Heyerdahl, Thor. Letter to Torp-Heyerdahl, Liv. 11 May 1942. Folder: Ragnar Kvam research material. Kon-Tiki Museum Archive. Oslo; see also Bell n.d. (note 1). Reel 5/Side 1.
- ³ Heyerdahl, Thor. Letter to Torp-Heyerdahl, Liv. 11 May 1942 (note 2); Heyerdahl, Thor. Letter to Torp-Heyerdahl, Liv. 7 January 1944 & 16 January 1944. Folder: Collection of World War II letters. Kon-Tiki Museum Archive. Oslo.
- ⁴ Heyerdahl, Thor. Letter to Spinden, Herbert. 16 December 1945 & 12 September 1946. Folder: General correspondence [01] – 1935–1947. Brooklyn Museum Archive. New York.
- ⁵ Heyerdahl, Thor. Letter to Torp-Heyerdahl, Liv. 14 June 1942. Ragnar Kvam research material. Kon-Tiki Museum Archive. Oslo.
- ⁶ Heyerdahl, Thor. Letter to Grosvenor, Gilbert. 18 November 1946. Axel Andersson Kon-Tiki Expedition correspondence digital database file HA(D)KM 6(1). Kon-Tiki Museum Archive. Oslo; Hildebrand, J. R. Letter to Heyerdahl, Thor. 21 November 1946. Axel Andersson Kon-Tiki Expedition correspondence digital database file HA(D)KM 6(2). Kon-Tiki Museum Archive. Oslo.
- ⁷ Heyerdahl, Thor. Letter to Olsen, Thomas. 27 November 1946. Axel Andersson Kon-Tiki Expedition correspondence digital database file HA(D)KM 4G(0.1). Kon-Tiki Museum Archive. Oslo.
- ⁸ Heyerdahl, Thor. Letter to Olsen, Thomas. 27 November 1946 (note 7).
- ⁹ Heyerdahl, Thor. Letter to Olsen, Thomas. 3 January 1947. Axel Andersson Kon-Tiki Expedition correspondence digital database file HA(D)KM 4G(1). Kon-Tiki Museum Archive. Oslo.
- ¹⁰ Heyerdahl, Thor. Letter to Olsen, Thomas. 27 November 1946 (note 7).
- ¹¹ Barbeau, Marius. Letter to Heyerdahl, Thor. 28 December 1946. Folder: Heyerdahl, Thor (1946–47, 1956–57, Box:B203 f. 40. Canadian Museum of Civilization, Library, Archives and Documentation. Ottawa.
- ¹² Heyerdahl, Thor. Letter to Olsen, Thomas. 3 January 1947. Axel Andersson Kon-Tiki Expedition correspondence digital database file HA(D)KM 4G(1). Kon-Tiki Museum Archive. Oslo.

¹³ Translation from Norwegian original by the author.

¹⁴ Translation from Norwegian/Swedish original by the author.

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Competing Interests

The author has no competing interests to declare.

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