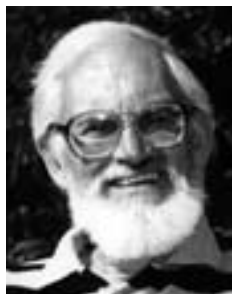


An Outsider Looking in: Jeremy Boissevain

Rob van Ginkel, University of Amsterdam

Irene Stengs, Meertens Institute, Amsterdam



Jeremy F. Boissevain (London, 1928) established his name in anthropology as one of the ‘Bs’ – Boissevain, Barth, Bailey, Barnes, Bott –, who in the 1960s and 1970s were important in superseding the structural-functionalist paradigm in British social anthropology with an actionist approach. Focusing on choice rather than constraint, on individual agency rather than structure, on manipulation and power-play rather than rules and tradition, and on dyadic relationships and ephemeral groupings rather than corporate groups, these transactionalists were instrumental in bringing the individual back into

the scope of social anthropology. Boissevain’s typology and analysis of quasi-groups (networks, coalitions, factions), ‘man the manipulator’, and phenomena like patronage, factionalism, and local-level politics provided new conceptual and analytic tools to anthropologists, especially those working in Europe. After he was appointed professor at the University of Amsterdam in 1966, it seemed as if he would move on after a couple of years, the reason being that he was unhappy with his outsider position and the local academic climate. But through a series of coincidences, he decided to stay on and to commit himself to developing new vistas for anthropology in Amsterdam. Working in the Netherlands during a period of expansion in the academia, he was able to gather a growing circle of anthropologists around him and to establish Anthropology of Europe as a legitimate subfield there. Over his long career in Amsterdam, he became a charismatic character whose adage was let a thousand flowers bloom. Although Boissevain conducted fieldwork in several societies, he is best known for his research in Malta. Doing research there for his PhD in the early 1960s, and, later, frequently returning, travelling back and forth between his Dutch residence and his Maltese house in St. Lucy Street, Naxxar. Casting his nets far and wide, in Malta, Sicily, Canada and Holland, he covered such topics as national and local-level politics, religious rituals and festivals, immigrant adjustment identity, the impact of tourism and, more recently, environmental issues. Peripatetic in childhood and for most of his early professional career, he was often puzzled by the modes of conduct he was confronted with. As is clear from a videotaped 1983 interview with Alan Macfarlane,¹ Boissevain has always

¹ For a short clip, see <http://www.alanmacfarlane.com/ancestors/Boissevain.html>. For more information on Boissevain and the department he chaired, see the introduction in Jojada Verrips (Ed.), *Transactions: Essays in Honor of Jeremy Boissevain*. Amsterdam: Het Spinhuis, 1994; Rob van Ginkel, Alex Strating and Jojada Verrips, ‘Trials and Tribulations of the Euromed Tribe’. In: Han Vermeulen and Jean Kommers (Eds.), *Tales from Academia. History of Anthropology in the Netherlands. Part I*. Saarbrücken: Verlag für Entwicklungspolitik, 2002. pp. 341-363.

been somewhat of an outsider looking in: a Dutchman in America and an American in Holland, on the road to new jobs in various corners of the world, developing his own Dutch-Maltese transhumance, always wondering why people behave differently in different settings.

Already as a boy, Boissevain was confronted with 'otherness'.

My mother was American of Swedish-Swiss extraction and very red, white and blue. My father was Dutch. He had no university education; he had been to the *Hogere Burgerschool* [Secondary Modern School]. He came from a business family. The Boissevains are all money people, shipping and so forth and his father was mixed up with that as well. My father was interested in classics, poetry and things that were absolutely ridiculous in the American business culture. People who were interested in that sort of things were wimps. So the first job he had as a young man in the U.S. in fact was working for an antiquarian, a high-quality second-hand book business, and that became one of the most famous book houses in the world, later on. He always was upset that he never continued with that. But his father, who was then in America, brought him into the shipping business. He was stationed in London, so that is why I was born there. Born in London, first four years in London, fifth birthday in Schouwen-Duivenland [the Netherlands], because I was staying with my aunt. It was the height of the Depression, I was five in 1933, and my father was off in America looking for a job. My mother was staying with my father's mother, in Hilversum with my two brothers. I was farmed out to an aunt. Then we went to America as a family, moved around to three or four different places – quite a few different places – and ended up in a funny little religious community of Swedenborgians in Pennsylvania, a very odd religion. That's another story, but probably this is why I later became interested in the machinations of priests and churches, and why I'm very sceptical of organised religion. It was a sect, yes, but a very powerful sect because the leaders were a very, very wealthy family. Anyway that is why we ended up for a time in America, and then we went back to Holland in 1938. My father was so interested in the sect that he was training to become a minister in that sect, in Holland. In The Hague they had a training centre, like a seminary. He was delighted and started to study Latin, Greek and Hebrew, which he enjoyed very much.

But then the war came and we left Holland in 1941, after the German regime was established. On the strength of my mother's American passport we all went to the United States. When we migrated from Holland to America we were looked down upon. Americans don't like foreigners coming in. And we were treated as dumb Dutchmen. I had my first fight with somebody who called me a dumb Dutchman, and he beat the stuffing out of me, he was twice as tall... My father always said: 'You boys are not hard enough to go into business, don't go into business. Get a profession, learn something, be an expert in something.' So eventually I followed this advice: I have never been in business. I did my high school in America, the army in America, I was sent to Trieste, and college in America - general liberal arts at Haverford College, Pennsylvania. First two years in this little college and then one year in Paris where I met Inga. I did not know what to do. I married Inga, my wife, in America two months

before I graduated, just before final examinations, and then spent the summer looking for a job. Through my father's contacts I was able to get a job with CARE [Cooperative for American Relief Everywhere]. He had done a lot of work for them, going to Europe and planning the distribution of the CARE's whole programme all over formerly occupied Europe. He recommended me and I was interviewed, got the job and went to the Philippines.

Boissevain began his professional career for the US relief organisation CARE. In the 1950s, he was stationed in the Philippines, Japan, India and Malta, successively. It was through his work for CARE that he first developed an interest in anthropology. Later, CARE was also pivotal in providing him with some funding to conduct research.

My first posting for CARE was in the Philippines. I came as a callow ex-student. But now I had an office to manage, I had a secretary to whom I was expected to dictate, I had a car and a driver: a totally unreal colonial world. I had that for five years until I went back to school to study anthropology. My interest in anthropology began in earnest in Japan, where I also went on working for CARE. The regional supervisor for CARE at the time, Paul Gordon, had studied anthropology under Franz Boas. I came in touch with some of the writings on understanding the Japanese mind and the Japanese culture, including Ruth Benedict's book on Japan.² That fascinated me. We were reading things in the book that we were seeing outside, that we were experiencing. That was my first contact with anthropology, but it really took a long time in the making. I also read Geoffrey Gorer – that book on the Americans.³ I was beginning to look at America from a different perspective already because of my wife, Inga, who was Swedish. She thoroughly disliked the United States. She showed me all kind of facets of America which I hadn't been conscious of, as I had been brainwashed or socialised into the American way of life. So this started things going.

Next, I had a job in India and we went often up through the Punjab. I spent a lot of time in the villages doing various development projects and I became more and more interested in that. I read some of the books on India, I visited the Wisers who had written a famous book called *Behind Mud Walls* about life in an Indian village.⁴ They were missionaries. When I was in India I began thinking of studying anthropology.

Then we went to Malta. At the time there was Governor's rule, a real colonial situation. There was a fight between the Maltese Labour Party and the church. So the leader of the Labour Party was at daggers drawn with the Governor and with the Archbishop. The Labour government was in power at the time. Dom Mintoff was Prime Minister and he had had a great clash with the British government. I had to go

² Ruth Benedict, *The Chrysanthemum and the Sword. Patterns of Japanese Culture*. Boston: Houghton Mifflin, 1946.

³ Geoffrey Gorer, *The Americans: A Study in National Character*. New York: W.W. Norton, 1948.

⁴ William Wisner and Charlotte Wisner, *Behind Mud Walls*. New York: Richard R. Smith, 1930.

there because my task for CARE was basically to take CARE away from the Labour Party. They had printed up posters saying: 'Vote for Labour and get CARE food.' But CARE was supposed to be a non-political organisation.

In Malta, Inga and I decided that we had moved around enough. In five years we had been in four different countries and that was by the time that we were starting to get a family. We decided that we should settle down and that I should try to become a teacher. Apart from that I wanted to gain insights in what was going on inside these communities in which I was working, but which we had only seen from the outside through interpreters, always far outside. I was increasingly dissatisfied with the work that I was doing and then wrote for advice to the man who had been my teacher in Haverford College – Lawrence Wylie – who had written the book *Village in the Vaucluse*.⁵ I had met Wylie in Paris; the year I was in Paris was the year he was doing his research, and my final year at this college was the year that he was lecturing to us on what he had found. He was lecturing on his field experiences. This seemed to me the sort of thing that I would like to do. Wylie was not trained as an anthropologist, but he wrote back and said: 'Well, if I had to do it all over again, I would study anthropology.'

I wrote around and then finally decided on a university in England, which had a quicker programme than in the United States. You had to do four, five, six or seven years of graduate work in America, since I had no background in anthropology. In England it was much swifter. You could get a degree in another subject and if you passed a qualifying examination after a year, you could be accepted for a graduate programme in anthropology. I decided to try for the London School of Economics, and with a very good recommendation from professor Wylie, I was accepted. There were Raymond Firth, Lucy Mair, Maurice Freedman, Paul Stirling, Burton Benedict. Firth had been a student of Malinowski and so had Mair. They were the ones who were very important, as was Maurice Freedman because he chaired the undergraduate seminar that I attended in the first year I was there. Another seminar, an evening class, was run by him and Paul Stirling. I saw them in tutorial classes, and the group of four or five evening students. They did not quite know what to do with the older students, because the undergraduates were eighteen, nineteen, twenty and in 1958 I was thirty. Going back to school was an adjustment: the literature to which I was exposed in that year of intense study, and all those scientific terms. I was thrown into that third year seminar. These twenty-year old bright young students were using technical language, discussing the structural implications of matrilineal cross-cousin marriage. I had no clue what that was all about. The literature at the time was certainly heavily African and heavily functionalist: Turner, Gluckman, Fortes, and Evans-Pritchard, these were the monographs you read.

The ones I was closest to were Raymond Firth, who led the graduate seminar, and Lucy Mair, who was my supervisor and helped me through my first year. Lucy supervised the preparation for my fieldwork, part of it. And much more important, she read my essays, and later supervised the writing of my thesis. You were left there

⁵ Lawrence Wylie, *Village in the Vaucluse*. Cambridge, MA: Harvard University Press, 1957.

to float more or less, thus soon after I arrived I said: 'Well, everybody is writing essays, should I write some essays?' She said: 'Yes', and she gave me some essays to write on Malinowski. I wrote something and then she said: 'You cannot say that about Malinowski, he wasn't like that.' She had a very, very strong feeling about Malinowski. Apparently she had – if not an affair – she had the desire to have an affair and didn't have. I do not know about that, but she felt very strongly about Malinowski.

So Lucy Mair taught me how to write. She was very, very strict. She refused to accept what she called 'the sloppy American terms and words'. She threw books at people, people said: 'Watch out, she is fierce.' I learned the hard way. She made me write short sentences, I developed a rule of thumb: a short sentence is thirty words. If it is longer you try to chop it down. It should not run for more than three typed lines. If it is more, then try to chop it up. It worked quite well. You chop sentences, you leave out words that you not really need. So these were things that I picked up from her. I am grateful to her. She became a good friend later. Rather hesitant, because she was so fierce and critical. Then when she got older she was more lenient to people.

Firth had a very gentle reputation. He was very kind and very conscientious. He would take extremely detailed notes of all the seminar papers that were presented. Then, when you would come back a couple of months later with another paper for his seminar on your topic he would go through the notes he made on the first one and would pick you up on it. Very important was his attitude to look at what people do. He taught us not to pay too much attention to what they say. All the graduate students in there were either writing about what they were going to research, or reporting on their research when they came back from the field. So they were all presenting papers, and Firth's questions were always: 'Why? Why do they do that? What are they getting out of it? What is the profit that they are getting? Why do they behave like that? And do they demonstrate what they say through their actual behaviour?' These questions were asked over and over again. He had a grip on people for two or three years. Running a graduate seminar and through the questions he asked, he had a tremendous influence.

Firth had us around for tea once a week. His secretary pointed out to us that he paid for the tea himself. So for the graduate students he had a formal seminar on Wednesday, and on Friday in the afternoon there was tea in another room where you were free to smoke and where you were free to talk about anything else. You just met and talked. And once a year he would invite the graduate students to his house for coffee and drinks. And cake. That is something that is the custom in England. So we met Firth informally, and he met us informally. This was very important because you swapped ideas all the time. As students, you knew each other, you met each other in the canteen and you had met each other socially at Firth's because the wives and girlfriends were invited to these social occasions too.

What I appreciated was that we had a good deal of freedom. That was different in the group that Gluckman organised. Freddy Bailey, who studied with him, told me it was terrible sometimes. You would even have to go to the toilet with him. He was standing there peeing, discussing your essay. Bailey said: 'You're all the time with him.' It was not easy to be so close to 'father' all the time. But our group was won-

derful, you were part of a family. It was all small-scale, twelve or thirteen graduate students, and most of them were foreign as well. I recall that most of these graduate students were Americans. They had fat grants and two or three years to work on their fieldwork. I did not have a scholarship. I was working on my savings. We saved while I was working for CARE. At the time we were earning well, and after five years we had saved enough to be able to pay for graduate school in England. Tuition was only forty guineas a year and after the fourth year it was free. It was amazingly cheap then. That lasted until I finished.

Firth was close with Malinowski, because he worked with him in the Department after his PhD. He did his PhD in economics in New Zealand. He wrote a book on the economics of the New Zealand Maori.⁶ Firth was much closer to Malinowski than Evans-Pritchard, who also had been his student, but he went his own way. That was the legacy that you had at the London School of Economics: 'Man as a political entrepreneur and the economics of social relations.' The link with Malinowski and with Firth introduced an entrepreneurial approach, in a way. Firth was very critical *avant la lettre* really of structural-functionalism because it did not provide a place for choice. He always wanted to know their alternatives, people have a choice. Why did they go this way and not that way? He pointed out to me the motives that people had. He would raise the links that people had with other people, and that would influence them to make certain choices. That was an important legacy I realised when I was criticising the structural-functional background in *Friends of Friends*.⁷ That was really the sort of thing that Firth was writing about. As you know, Lucy Mair wrote a book on primitive politics She was interested in politics and development activities.⁸ And those were my interests too, and that was why I was assigned to her by Firth, who was chairman of the Department. I did my examinations and passed them, and that meant that I was accepted as a graduate student working on a PhD.

After Boissevain was accepted as a graduate student he contacted the Lieutenant Governor of Malta to inquire as to the possibilities of doing a PhD project on Maltese local-level politics.

In British social anthropology you had this strong tradition of fieldwork. In the field, in the field, in the field: the idea of people doing a PhD based on literature was simply not on there. I went to Malta to see the Lieutenant Governor who was the administrative advisor and a sort of the head of Department for the Governor of Malta. I asked him whether he thought there would be any support for a study of local-level politics in Malta. And he said: 'Yes, certainly.' So I applied for a fellowship grant from the Colonial Social Science Research Council, and with Lucy Mair's support got it. I think it was one of the last colonial social science research grants.

⁶ Raymond Firth, *Primitive Economics of the New Zealand Maori*. London: Routledge and Sons, 1929.

⁷ Jeremy Boissevain, *Friends of Friends. Networks, Manipulators and Coalitions*. Oxford: Basil Blackwell, 1974.

⁸ Lucy Mair, *Primitive Government*. Harmondsworth: Penguin Books, 1962.

You came from a university in the UK, conducting fieldwork in Malta. Did that in any way influence your research? Did the Maltese see you as representing the British colonial government?

That was definitely something I was faced with. I designed my research without any influence of the government. I did not know what they wanted or what they did not want. Britain was having a terrible time with Malta; there was a big fight with the church and the Maltese government. ... When I arrived I called on the new Lieutenant Governor and he was sitting uneasily. He said: 'Look here, if I have any evidence of you contacting the unions or the Labour Party, you will be on the next boat out of here.' Like that, very uneasy, very bad tempered he was. I discovered that they had sent a whole copy of my research proposal to Malta, and I later learnt that it was also in the hands of a lot of the top civil servants. So I did not slide in there: they knew what I was doing. Now your question: did it influence what I was doing? In a way it did, in a way it did not. I did not really spend very much time investigating the unions and the national political things. It certainly put the wind up me, maybe, because I did not start thinking about going to see Mintoff until the very end. I had meetings with him a couple of times but I would say it didn't directly influence my research that I can think of, but I learned a lot of him. My schedule, my job that I had set myself and for which I had received the grant, was to examine what was going on at the grassroots level.

I had been there before for almost two years [for CARE], and I had very good contacts because I had been in a sort of semi-diplomatic status. I knew a lot of elite people, people in government. As an outsider for an outside agency there where very few peers at the time, so you had a high visibility. I still had these contacts when I went back [for research]. But I went back at a very different level. I was trying to understand what was going on, and I was fascinated by the divisions in the villages between rival band clubs and their infighting. I had read about it in the newspapers, and did not understand it... So, what was it that winds people up at the village level? And, what is the link between that and national politics?

Was there any resentment towards the UK, or UK representatives or UK researchers?

No, the people in the villages didn't know the exact nature of my grant. The people at the upper civil service level were aware of this because of the memos that had been sent out. They were all good solid pro-British civil servants, you see, so that was not a problem. What was a problem was that I was an American. I was accused of being a spy! Initially the villagers said: 'He must be a spy. He knows too much about us. What is it? How is it? He must be a United Nations spy because he has got a Swedish wife, he is American, and he comes from a British university. What is he?' I was able to neutralise that in a heavy drinking session with the person who had been saying I was a spy. I said: I am a student. So I did not have any further problem on that. Later I went around with the same Labour Party members. I felt personally linked, sympathetic to the Labour cause there. The basic fight was not with England, when I was there, but with the church. And my book [*Saints and Fireworks*] was better liked by the Labour supporters than by the other side. I was not sympathetic to the way in which the

Labour Party had been treated by the church. Because when I was there, it was bitter. People were not allowed to read the Labour paper. The staunch Labour supporters were interdicted, they were not allowed to get married, they were not allowed to attend church, to baptise their children. Well, some of the top leaders were not allowed to be buried in the cemetery. The church had these strong sanctions.

The choice for studying festivals and religious ritual as an angle for the study of local politics was not such a self-evident choice, at the time. Were you inspired to that at the London School of Economics, already? Moreover, this study formed the start of a long list of studies on feast and ritual. Can you tell us something on how this research influenced your thinking and future research?

With the exception of *rites de passage* and Maussian exchanges, rituals and festivals were not part of the package of anthropological puzzles we were exposed to at the LSE. I became interested in festivals because they formed the *raison d'être* of the village factions I was exploring. Much later, in 1976, I began puzzling out why, despite my predictions to the contrary, Maltese *festi* kept expanding. My first research opened my eyes to the tension between the ordered canonical aspects of the public rituals controlled by the clergy and village elite and the more ludic, popular aspects of the *fešta*, largely in the hands youths and others chafing at the control that the local establishment exercised over *their* celebrations. At first I thought the Maltese escalation of the ludic elements was unique, for colleagues elsewhere had also predicted such events would decline. An exploration demonstrated that the escalation in Malta was part of a wider pattern. Briefly, this increase is a comment on and a reaction to the growing isolation of people from each other due to the impact of the market economy and the commercialisation of daily life.⁹

Back in the UK in 1961-1962, Boissevain was under time pressure to finish his PhD dissertation.

I handed it in within a year because the wolf was knocking outside the door. I did not have a grant for the LSE. I had a research grant to sit in Malta, a very, very parsimonious grant, and another child was born in Malta. So my family responsibilities were growing and I just figured we had not enough to continue, so I had to get rid of it. 'Well', I said to myself, 'if Fredrik Barth can write a small book that is accepted as a PhD, so can I.' Paul Stirling was editor of the LSE [London School of Economics] monograph series. He said: 'If you write a book on this, if it is small, short, to the point, clearly written and with very few footnotes, it will fit nicely into our series.' So I aimed at that. I got through it with the help of Lucy Mair, because she was around to read the final chapters as they came in and criticised them all during the summer. Then I handed that in, and in September [1962] got a temporary job in Sicily for giving advice on a development project. In between I had to come back to London for my thesis examination in November. I also went to the American Anthropological

⁹ Jeremy Boissevain (Ed.) *Revitalizing European Rituals*. London: Routledge, 1992.

Association conference in Chicago where I presented my first paper, which was a seminar paper I had done for Firth, on factions.

This paper got me a job in Canada, in Montréal. At one of these little meetings [in Chicago], a kind of cocktail party, somebody appeared who said: 'Monsieur Boissevain, vous parlez Français?' and I said: 'Oui, oui.' And he said: 'We are looking for someone at our university.' Anyway, I went to Montréal for an interviews, and gave a French version of the paper I had presented at the AAA conference. My father was able to help me translate the paper into French. And they decided to hire me, which was very lucky for me. In Canada I prepared *Saints and Fireworks* for publication.¹⁰ Basically it was my thesis, I just had to change some names, eliminate some things in the conclusion. I also wrote up my thoughts on patronage, and a study on my work in Sicily. The latter was a very long report, published by Jan Pouwer in an obscure journal entitled *International Archives of Ethnography*.¹¹ I sent the whole thing to him, and he said: 'Fine. Perfect, just what I wanted. It fills up our magazine.' Anyway, I wanted it published, but I wanted the report published in a very obscure journal where people from Sicily would never find it. Because I wanted to go back, you see. It was a sort of preliminary analysis, because I had to leave Sicily before I had finished what I wanted to do. The man I was working for [Salvijn Duynstee] was accused of being a communist priest and was forced to leave. I was obliged to leave too, and it was a very difficult time. But I came back again. Professor Wylie recommended me for support to the American Philosophical Society, and I also got a little grant from CARE. In the meantime my family was in Malta with the children going to school there. They could not go to school in Sicily, because they had to learn Italian before going to a nuns' school, and that was all very difficult. A pressed year. That is how I got to Sicily. It was a contract, however, and they did not have enough money to keep me there and meanwhile I had gotten my job in Canada.

The first lecture I had to give in Montréal was on kinship, in French; that was very difficult. Fortunately, there was a book that had been translated on systems of kinship and marriage edited by Radcliffe-Brown.¹² And that saved my bacon. I had the words, anyway. That is how I ended up in academia. In Montréal I befriended Albert Trouwborst [a Dutch anthropologist who was working there] and Asen Balikci, who was a pleasantly mad Bulgarian. Balikci was very entrepreneurial. He advised me to apply for a grant. It was very important to get a grant as soon as I got there, because we were running out of money. This is the entrepreneurial thing. I had three children, life was expensive and then you start in academe at the lowest level. They do not take into account the fact that you are five years behind, and what it means to have a family. I wanted to make ends meet, and I wanted to continue my research on Sicily. Balikci

¹⁰ Jeremy Boissevain, *Saints and Fireworks. Religion and Politics in Rural Malta*. London: Athlone Press, 1965.

¹¹ Jeremy Boissevain, Politics and Development in a Sicilian Agro-Town: A Preliminary Report. *International Archives of Ethnography* 50: 198-236, 1966.

¹² A.R. Radcliffe-Brown and Daryll Forde (Eds.), *African Systems of Kinship and Marriage*. London: Oxford University Press, 1950. French translation: *Systèmes familiaux et matrimoniaux en Afrique*. Paris: PUF, 1953.

said: 'Why don't you do something on Italians in Montréal?' So I organised a research project on Italians in Montréal. I was able to hire a secretary and a research assistant and we did a study on the Italians there, and most of them were Sicilians.¹³

How was Saints and Fireworks received?

It was very well received and favourably reviewed in the professional journals and the local newspapers. It was the first anthropological study of Malta, the first examination of village life, and the first account from a grass roots perspective of the struggle between the Church and the Malta Labour Party. Over the years it has become something of a local classic, in Malta! My Maltese publisher is now pressing me for a new chapter to update a third edition. The term 'saints and fireworks' has even become a saying in Malta indicating turbulent *festa* happenings. I wish I could claim to have coined the book's catchy title. But that honour belongs to Julian Pitt Rivers, one of the external readers for the 1965 LSE edition.

Now you had three fieldwork experiences: Malta, Sicily and the Italians in Montréal. And you were publishing about all of these?

Yeah, the age thing. I was behind, I had to catch up. You know: publish or perish.

I had two years in Montréal. Canada is a lovely country, but still very North American. So North American that we felt more at home in Europe. We had lived in a sort of English colonial atmosphere in India and in Malta. In Japan and the Philippines it was basically expatriate, in the American sphere, but in India – ex-English – and Malta it was definitely English. My wife did not like America, and I was not very pro-America at the time anymore either. We both desperately wanted to go back to Europe. I applied for all kinds of jobs. The University of Sussex had a job going, and I flew out to the Mid West to be interviewed by a visiting member of the University of Sussex. I actually shaved off my beard, as my beard was beginning to become greyish, you see. I was extremely conscious of the fact that I was five years behind my age cohort: 'I need the money, they have to hire me and I'm already becoming grey!' So I shaved my beard. Then I was interviewed by a tatty looking chap with dirty feet, a beard, in sandals and a big long sweater, who was one of the professors from university. I was accepted, and grew my beard again.

Virtually as we were getting on the boat to go to England, I got this letter from Albert Trouwborst. He wrote that professor Jan Pouwer would be leaving and that there would be a job vacancy at the University of Amsterdam. I had some relatives in Holland, and I had very, very pleasant memories of Holland as a child. I was very interested in going to Holland. 'Christ', I thought, 'I am now going to England and there is possibly a job in Holland.' At one stage I flew over and was interviewed by André Köbben (see interview in this issue), Wertheim and Hofstra at Dikker and Thijs

¹³ Jeremy Boissevain, *The Italians of Montréal. Social Adjustment in a Plural Society*. Ottawa: Information Canada, 1970.

[a posh Amsterdam restaurant], with a sandwich, and they said: 'Well, we'll pay for lunch.' So I got my free sandwich there, and a cup of coffee. And that was the interview. I was never offered a refund for the trip. But anyway, they offered the job to me so it turned out to be a good investment. I could not complain. That was the summer of 1965. The three things I think that got me a job in Holland were *Saints and Firework* and two articles: a paper on factions that was published in *American Anthropologist*, and an article on patronage in *Man*.¹⁴

What kind of intellectual climate did you encounter in Amsterdam? How did you cope with that and how did you try to develop your own school or group of people there? Were the Dutch anthropologists interested in the kind of topics and research that you were interested in?

Well, the first impression was that it was a feudal setup. At the time, professors in Holland were sort of Olympian characters. I had never been part of the Dutch system of academic hierarchy, where the differences between professor and students and staff were enormous. This is the first thing I had to get used to. I remember the first meetings of the anthropological society here. I have a memory of a lot of men in dark suits smoking cigars, meeting in a black dark room at the University of Leiden, the ethnographic museum. That was a very depressing experience. These were the *crème de la crème* of anthropology. Everything was 'professors only' there. I did not understand that. I had great difficulty getting used to that. That was my first impression. Second impression: I came here as an independent person who had been doing his own teaching. I was absolutely aghast that here people who had done fieldwork and were PhDs were not allowed to supervise other PhDs. Everything was done by the professor. All the teaching was organised by the professor. He was responsible for it and the teaching was done by acolytes, people who were my age, you see. André Köbben had said it was not going to be easy: 'It will be very different.' So he said: 'You'll have a teaching assistant'. And I said: 'What do I need a teaching-assistant for? What is he going to do?' 'Well, he'll teach in the seminar'. And it was Jan van Nieuwenhuijsen, a man my age who was just back from fieldwork in New Guinea. 'So what is he going to do?' 'Well, he can take the attendance. If you're not there he can organise discussions'. He was the fifth leg on a sheep in a way. I didn't need a teaching-assistant. 'But he can look up things in the library for you and so forth. He's useful'. This is the way the junior staff was treated here. I thought that appalling. People in my position were supervising these people [the assistants]. They were running the courses, but the professors decided what they [the assistants] were going to teach, and so forth. And here almost all the lectures were done by the professors. Those were the impressions that upset me. That was difficult. I discovered later on that professors had a sort of God-like status here. I wasn't used to being a God, having to run a department. I had never had any teaching-assistants at all.

¹⁴ Jeremy Boissevain, Factions, Parties and Politics in a Maltese Village. *American Anthropologist* 1964, 66(6): 1275-1287. Jeremy Boissevain, Patronage in Sicily. *Man* N.S. 1966, 1(1): 18-33.

I was very unhappy here. I was very much an outsider the first number of years. I had negotiated for a secretary, which is something I am eternally grateful for. In Sussex I had met the philosopher Frits Staal, who was in Sussex on a sabbatical. And he said: 'It will be very difficult and very different for you in Holland, you will need a secretary. Get them to agree before you go.' So I said: 'Because my Dutch is insufficient, I will need a secretary.' And André Köbben organised that. My secretary was in a little room apart. She was not invited to go down and have tea with the ladies of the library and a little group of ex-colleagues and ex-students of Köbben. They were all a group who had been together for years and years. My secretary was kept out and I felt out of it too. I was an outsider coming in. I didn't know anything about the setup in Holland. I didn't know the etiquette and the rules or whatever. Köbben and his group had been working together for years and years. I was very depressed with the way things were going here.

At a certain stage there was a job available for the department. Anthropology was expanding and there was a post. Köbben said: 'I really want this post for myself, because I want someone to work with me.' There were a lot of people who worked *for* him but not *with* him. Wertheim said during one of the meetings we had: 'Well, colleague Boissevain doesn't have anyone yet. Perhaps it would be logical that he will get staff.' And I said: 'There is this young man, Anton Blok (see interview in this issue), who is just back from fieldwork in Sicily and who has been invited by Eric Wolf to go to Michigan. He wants to hire him there. If he's not given a job here, he is going to go to America.' Blok joined us and then we were two. I felt like an outsider, until Anton came. We developed Mediterranean studies together.

Still, I missed the kind of collegial contacts you had in Britain. Each unit here was isolated. Every department here had its own little institute with its little library. No central library, no linking of the various disciplines. In England there was the system of Friday seminars. You brought colleagues from other universities over to lecture. This tremendous crisscrossing of people, of exchanging ideas in seminar papers was totally foreign in Holland. There was no contact. So I wanted to go back to England. I accepted a job and resigned in Amsterdam. But then one of my children had some problems and we were told that these had to do with moving around too much. The worst thing we could do was to move again. Thus we decided to stay. But I had already resigned. Luckily, the University of Amsterdam was very sloppy. They had not processed the resignation papers. Wertheim came by one day, and he was fishing around, something about a successor, he said: 'It's a pity you're leaving here, we're having a terrible time trying to find somebody who can replace you. You wouldn't consider staying?' So we stayed! Having decided to stay, you know, changes your attitude. It was also a traumatic thing. But in a way it was good. I was totally committed to staying on here. I wasn't looking elsewhere anymore.

Anton Blok and I, and a secretary, with our own student assistant [Rudo Niemeijer], were a little faction here, with our own supporters in the Mediterranean study group. Our seminar was looked down upon by the other side. We started bickering for resources, the usual sort of thing in all universities. From a faction we became a corporate group. As a result of the democratisation, we became legitimate, and the others accepted that. The attitude of the others changed. We were part of the institute. We

were no longer the enemies. The working arrangements became much more pleasant than they had been. And it was a growing period. Rod Aya came, Nico Kielstra came, and I was able to hire Hans Vermeulen and Jojada Verrips. Jojada was already doing research in the Alblasserwaard [the Netherlands]. There were three [PhD students]: Carla Jonker, Lo Brunt and Jojada Verrips. They had all finished their research in the field by then. Before that Mart Bax had done his research [in Ireland] and finished it. It was pleasant. Of course there were theoretical differences between me and Köbben, but these were not fought out. I did not want to have the situation that you had in Holland before I came. Anton told me that the department was then divided. There was the faction of Jan Pouwer, structuralist in the Leiden school, and of Köbben who was a functionalist. They had a joint seminar for the doctoral students. They would catch flies score off each other. They would shoot down the papers of the students. They were fighting each other. Students were obliged to take sides. Anton said it was a very unpleasant atmosphere. We did not have that. We did not have these joint seminars.

So something new was cooking here. There was a faction that turned into a corporate group in terms of the theoretical approach. Politics, factionalism, networks, there was something new in terms of research locations. The Mediterranean had already been explored to some extent, but still was exceptional as a research location. Then came new locations in Europe and the Netherlands. So all the new things were turning into a new stew, with all these ingredients. That is probably a very interesting period to look back on?

Very exciting. I was able to do two things. I was on the verge of crystallizing some ideas on networks and factions which I then incorporated into my inaugural lecture.¹⁵ I had developed these ideas when I was teaching a course in rural sociology at the University of Sussex. I got hold of seminal articles on networks and quasi-groups. This was just the sort of thing that I wanted to get involved in. I worked this out, that was one thing I wanted to get off my chest. And that was *Friends of Friends*. That was born in a doctoral lecture group. I presented the chapters in lecture-form and then wrote them up.

The second thing I wanted to do was to establish the legitimacy of European and Mediterranean studies. I had discovered that the definition of anthropology here was basically the study of primitive non-Western man in group formation. My job description when I came here was Cultural Anthropology and Non-Western Sociology. I said: 'I am not working on groups in a non-Western area, I do not want to be limited to non-Western areas. Let's change it into social anthropology. It does not limit the study of anthropology to non-Western areas.' André Köbben was able to get the title of my chair changed to social anthropology. I did not want to be a cultural anthropologist, because Köbben's definition [of cultural anthropology] in *Van primitieven tot medeburgers* was 'the study of man in groups', while I was working on networks.¹⁶

¹⁵ Jeremy Boissevain, *Netwerken en quasi-groepen. Enkele beschouwingen over de plaats van de niet-groepen in de sociale wetenschappen*. Assen: Van Gorcum, 1968.

¹⁶ André Köbben, *Van primitieven tot medeburgers*. Assen Van Gorcum, 1964.

That was the background of that. So I accomplished a book and then was able to introduce the anthropology of Europe. We had a little group of people interested in that cultural area; that was satisfying. In my second year I gave a lecture course on Malta. I worked out some of the material that I hadn't developed in my thesis, on a small Maltese village. That's what the following year became *Hal-Farrug: A Village in Malta*.¹⁷ Much later, I organised a study on the problems of Surinamese entrepreneurs in Amsterdam.¹⁸

When did you first realise that your work was adding something to the academic debate? There was a transition period where structural functionalism was on its way out. There was a search for new paradigms, though not that consciously, but anthropologists were looking for alternatives. Firth was probably one of the first anthropologists who did work on individuals, choice, and agency, but he did not really turn that into his own paradigm.

I was annoyed about the way I had been brainwashed by the structural functionalists. I enjoyed writing that book, *Friends of Friends*. So that was when anthropologists were interested in what I had done on patronage. Then they began to quote from that, and then from my article on quasi-groups and networks which I published in *Man*,¹⁹ which was my inaugural lecture in Holland. It was also discussed. It was being referred to. *Friends of Friends* was cited quite a lot. It was not taken too kindly by some [of the structural functionalists]. Lucy Mair was quite annoyed by it. She huffed and puffed. I got a very snotty review from Anthony Cohen.²⁰ But that was to be expected. He was a favourite boy of Max Gluckman, and I was very hard on Max Gluckman at a point. Basically I had pointed out the paradox that though many people felt that the whole structural functionalist apparatus was questionable, they kept on following it. My question was: why do people continue to follow? To use the metaphor of a Maltese chap [Edward De Bono]: people keep digging a hole deeper and deeper; it's more comfortable to keep on digging rather than get up and dig a hole somewhere else.²¹ They follow the footsteps of the master. That's one of the ways of getting ahead. It is difficult if you leave, because then you antagonize the master. You have to kill father to be able to dig your own hole. It is very difficult if you are part of a tight school, to publish something that is contrary to the dominant paradigm. It was fun writing it [*Friends of Friends*]. It was the only thing I have written really that was a bit polemic. Gluckman was intent that everybody should be a Gluckmanian. On everybody who criticised him, he would lower the boom.

¹⁷ Jeremy Boissevain, *Hal-Farrug: A Village in Malta*. New York: Holt, Rinehart and Winston, 1969.

¹⁸ Jeremy Boissevain, August Choenni and Hanneke Grotenbreg. *Een kleine baas is altijd beter dan een grote knecht. Surinaamse kleine zelfstandige ondernemers in Amsterdam*. Den Haag: Ministerie van Economische Zaken, 1984.

¹⁹ Jeremy Boissevain, The Place of Non-Groups in the Social Sciences. *Man* N.S. 1968, 3(4): 542-556.

²⁰ Anthony P. Cohen, Review of 'Friends of Friends', *Man* N.S. 1977, 12(2): 347-348.

²¹ Edward De Bono, *The Use of Lateral Thinking*. London: Jonathan Cape, 1967.

Now transactionalism represents a transitional stage between the dominant structural functionalism in British social anthropology and a plethora of other paradigms that start to surface, like Cohen's more symbolic approach, Marxism, centre and periphery. Do you feel that there have been attacks on transactionalism? Some said that it is not a theory but rather a particular kind of approach, not necessarily a paradigm that can make theoretical statements or propositions. It is remarkable that after a ten-year period transactionalism seems to have disappeared from the scene.

In a way it was eclipsed by Marxism and other ideas. Basically, it is not a theory. It is a set of tools for looking and analysing things. I have always looked at it in that way. It is not a paradigm. People tried to make it into a paradigm, into a school, a transactionalist school, but I never thought of it like that. It was just an extra dimension alongside the other set of tools you had in anthropology for looking at things. You still have the framework of culture, rules and laws. But this is how you get an extra hold to understand how people manipulate and work with the cultural package they have inherited and the rules and regulations which frame their own lives, and behaviour, of course. That is the way I looked upon it. So it is not surprising it did not have the pretensions that the Marxist or Eliasian way of looking at things had. That was a more rounded way of looking at things, probably. So it was very much a transitional period. 'Transactionalism' was used really to hammer nails into the structural functional coffin, which was already being shaped, as it were, and you were putting a lid on it. And there were other things that were coming up.

So it was not all this new?

I realised later as an older, wiser man, that in fact Mair and Firth were not pure structural functionalists. Certain questions I was asking, the critique I had of functionalism, was evident in the work they had done, you see. Firth's book on social organisation was bringing in other domains, was trying to bring in agency.²² And much, much later I came across another lecture of his, in which he was also very critical of structural functionalism.²³ That was already in the 1950s, you see. That was not part of mainstream thinking, but he never pushed his ideas. Unlike Gluckman, Firth had a philosophy of 'let many flowers bloom.' He did not want to create a 'school of Malinowski.' And that is something that I have tried to continue in Amsterdam: the idea of many flowers blooming. Everybody should love to do their own thing, and not be forced into accepting something. Firth and Mair looked at how people behaved. Later I realised that this is also a legacy of Malinowski. And this is not the sort of legacy that you have with Radcliffe-Brown and Evans Pritchard. Evans Pritchard does not discuss how people recruit support, how new leaders operate. It was all kinship, you know. And that does not work for everything. But Mair wrote how patronage in Africa evolved, how leaders recruited support. This was something that was not

²² Raymond Firth, *Elements of Social Organization*. London: Watts, 1951.

²³ Raymond Firth, Social Organization and Social Change. Presidential Address. *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 1954, 84(1/2): 1-20.



Jeremy Boissevain and the Kirkop parish priest on the way to the farewell picnic, summer 1961

really discussed in *African Political Systems* at all, because that book appeared under the wing of Evans-Pritchard.²⁴ Firth and Mair were interested in that new modish word, agency. Lucy Mair would have had a fit if you had come with something like the discourse analysis that we have nowadays. Anyway, that is something else. But I am sorry, I always regret that I did not realise my debt to them while they were still alive, you see. Their criticism of structural functionalism was more implicit than open. Firth did not push that, but it was there. You had open criticism by Leach at a certain time. In his book, *Pul Eliya*,²⁵ on Sri Lanka, he really let fly at the structural functionalists. If you read the introduction to *Pul Eliya*, that is really flaming. He is very, very polemic there.

You kept returning to Malta. Why? Was it because it is an island society, with the idea that you can have an overview of a more or less bounded society, albeit with quite a large population, or was it the idea of continuity?

As most things in life, it is partly a coincidence. From 1964 to 1973 I did not do any research in Malta, but we returned virtually every summer with the children on holiday. So the link with Malta is not only scientific. I was going there, I kept talking to people, seeing what was going on there, but it wasn't in terms of a particular new scientific puzzle. I maintained contact for all these years, and I was able to combine a bit of research the first two summers I was there. Going back to Malta was a personal thing. It was a congenial place to go for holiday. It was combining work with pleasure. Because I had this continuity and a grasp of what was going on, I was able to work out a lot of existing theoretical puzzles. Two or three times I had a sabbatical on Malta for periods of two or three months. Once I had a period of teaching there.

To keep on going back was a unique opportunity, which had its advantages and disadvantages. The advantage of going back is the continuity and you realise the importance of a longitudinal contact with an area. But on the other hand, changes take place slowly and imperceptibly. So you are not struck with change like you would be when coming back after an absence of ten years. It was coming and going, but it did provide a number of insights and the importance of continuing contact with the place. When I published *Saints and Fireworks*, my mission was to make clear for the urban middle-class, upper-class in Malta, what was going on in villages. They had no clue of that, the people in town. But I wanted to explain what was happening there, to make it logical why people shoot off fireworks and make a lot of noise. That was an applied thing. I wanted to be useful, to make a contribution to Maltese understanding themselves. That is the way they have accepted that book. They are grateful for it. Helping to understand the world around you is part of anthropology. There is a sort of general thing that you can do. Now I have come back again to the applied thing, helping to make understandable what's happening in the world. It is a fairly arrogant

²⁴ Meyer Fortes and Edward E. Evans-Pritchard (Eds.), *African Political Systems*. London: Oxford University Press, 1940.

²⁵ Edmund Leach, *Pul Eliya: A Village in Ceylon. A Study of Land Tenure and Kinship*. Cambridge: Cambridge University Press, 1961.

way of looking at the world: 'explaining Malta to the Maltese.' But that's what I was doing. They were challenged by the way I looked at them. I have wished they would have examined the book more critically than they have, but they haven't. You're a big fish in a little pond. They say 'Oh, Jeremy Boissevain, the author of books'. I am known in Malta, but less and less, because there are more and more other foreigners and other business people. It is a changing society.

Did people in Malta ever strongly disagree with what you wrote about their society?

No, this is one of the things that has troubled me. There's only one person who disagreed in print. This was a rather stupid member of a band club in one of the villages, in Hal Kirkop. I had written in the book somewhere, quoting a person from the village, who said 'Before the war we were primitive, we were savages here. We had no running water, we had no sewage, we had no electricity. After the war, everything changed, we became civilized.' I quoted him. This chap had read it in the book *Hal-Farrug* and then he complained in a letter to the newspaper that I called them primitive. He hadn't seen the quotation marks: he didn't know how to read a book like that. It was discussed in the press. People came to my rescue in the press. That was the only time it was really discussed publicly. It was new. It was the first book that had been written from a sociological point of view about people in the rural areas. It created a lot of interest and people quoted from it, but never in relation to any serious discussion, to my knowledge. I would have liked to have had discussions, I would have liked to know: was I right about this? Recently, in 2003, I was given an award by The Malta Council for Culture and the Arts for my work on the culture and history of Malta. The chap [Ranier Fsadni, an anthropologist] who introduced it, discussed some of my work, which was the first time. He pointed out that the things I had written on Malta had a much wider significance than just Malta. That was last year, and I was quite pleased, but this recognition took a while.

You retired more than a decade ago, but you continue to do research in Malta?

I lived in Malta pretty systematically most of the 1990s for six months a year. In 2001 we sold our house in Malta. I have a huge clipping file on Malta, and I am still going back. I am basically still interested in what is going on, but I am interested in different problems now. These I can pursue partly through that, but partly not. So it is a different kind of research because I am not in Malta talking to people so much. I am doing research via the Internet now. I read the comments, the continuation of things I was observing when I was in Malta.

When I retired, I wanted to do something for Malta. I lived *off* Malta as an anthropologist for all these years, and when I retired, I wanted to do something useful *for* Malta. I wanted to do something about tourism. What offended me – still offends me – is the way in which Malta almost prostitutes itself for tourists. They were willing to put up with anything as long as tourists came. Things that were insulting to them, they accepted. The way that tourists went around and came into their churches, half dressed, upset people. They walk into a bank with a bikini on practically. That upsets

people, but they accepted it as part of tourism, on which they depended and did not complain. With tourism I initially challenged the centre-periphery argument; that tourism is creaming off, exploiting. A lot of people said it is the perpetuation of dependency. But for a lot of people it is the only development they have. It is just nonsense to say that the poor grow poorer, and that the gap between rich and poor has increased. The people of Malta have a much better standard of living now. But it is a new form of development. I brought forward a lot of material on Malta in which I was challenging the notion that the periphery of Europe was becoming a tourist colony being exploited by the centre.²⁶ A lot of the things I have written were to challenge accepted notions. I also challenged Greenwood's 'Culture by the Pound' thesis,²⁷ the idea that pageantry is mainly done for the tourists in order to earn money, that feasts grow bigger because of the tourists. But that's a simplistic thing. So I challenged that.

I wanted to do something to make a contribution to help Maltese gain voice, to challenge what was happening to their own society. That they did not need to prostitute themselves to get tourists, that they didn't need to go on accepting things. I started talking about the dark side of tourism, which was contrary to the initial way in which I defended tourism as developing the economy. This was partly the result of the fact that I went back and lived in Malta [after retirement] and saw what tourism was doing. So I became quite critical. The Maltese did not disagree [with me] but they did not sound off, they didn't write Letters to the Editor that were very critical. They didn't mind. The only people who did mind what I wrote were the people in the tourist industry. They responded to some of the things I wrote, some of the talks I gave. The Minister did not like the research that showed how dissatisfied people in one walled city, Mdina, were about the way that tourists were affecting them.²⁸ But in general, the Maltese didn't complain.

The only thing some complained about was what was happening to their country and the way in which the environment was being destroyed by the building of hotels, golf courses, swimming pools and, later on, huge tuna-pens – fish farming. The environment. That is what I am writing on now. The Maltese who were protesting about that, doing something, were the NGOs. These groups are now criticising, and

²⁶ Jeremy Boissevain, *Tourism and the European Periphery. The Mediterranean Case*. In: Dudley Seers et al. (Eds.), *Under Developed Europe. Studies in Core-Periphery Relations*. Hassocks: The Harvester Press. pp. 125-135, 1996.

²⁷ Davydd J. Greenwood, *Culture by the Pound*. In: Valene L. Smith (Ed.), *Hosts and Guests. The Anthropology of Tourism*. Philadelphia, PA: University of Pennsylvania Press, 1977. Jeremy Boissevain, *Ritual, Tourism and Cultural Commoditization. Culture by the Pound?* In: Tom Selwyn (Ed.), *The Tourist Image. Myths and Myth Making in Tourism*. London: John Wiley & Sons, pp. 105-120, 1996.

²⁸ Jeremy Boissevain and Nadia Sammut, *Mdina: Its Residents and Cultural Tourism. Findings and Recommendations*. Malta: Med-Campus Euromed Sustainable Tourism Programme, 1995.

Jeremy Boissevain, 'But We Live There!' Perspectives on Cultural Tourism in Malta. In: Lino Briguglio et al. (Eds.), *Sustainable Tourism in Island and Small States: Case Studies*. London: Pinter. pp. 220-239, 1996.

organising protests.²⁹ I thought that was interesting, it was a new sort of development. I helped introduce tourism studies. Tourism studies was a new field that I suddenly stumbled on. Malta had been living off tourists for thirty years, but there were no studies of tourism. Nobody was studying the impact. There were only studies on how many tourists were coming, what can we do to get more tourists to come. They were selling their own country, but not consulting the people about it, you see. But now it is a subject at the university [University of Malta].

So that was how I also got involved in forms of applied anthropology. I have always swung between theoretical engagement and trying to get my feet back on the ground and look at some concrete problems that people had. Usually, as anthropologists get older, they stop doing fieldwork and start analysing books again. Leach and Pitt-Rivers both turned to the Bible, analysing the Bible. It is a phase of not being so involved in fieldwork, so you start analysing what people write. It is another way of doing it – not my way of doing it. I like getting my feet wet with people. Anthropology for me is being in touch with people and what is happening to them. I think that is the important thing.

²⁹ Jeremy Boissevain, Hotels, Tuna Pens, and Civil Society. Contesting the Foreshore in Malta. In: Jeremy Boissevain and Tom Selwyn (Eds.), *Contesting the Foreshore. Tourism, Society and Politics on the Coast*. Amsterdam: Amsterdam University Press. pp. 233-260, 2004.