Autobiography, Anthropology
A Personal Historical Recollection

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Abstract

In the 1980s, the theme for a future ASA conference had to be personally proposed by a potential organiser at the conference two years earlier. The proposer had to personally convince attending participants, who decided by a visible vote of hands. This recollection on the theme "Anthropology and Autobiography" traces the successful 1987 vote for the 1989 conference proposed by myself with Helen Callaway. Before the vote, there were many negative comments claiming our proposal was mere 'navel -gazing' and a 'feminist plot'. Inspired by the problematisation of the use of 'I' in Clifford and Marcus' '*Writing Culture*', we wanted further confrontation of the gender, age and personality of the participant observer. This article includes references to Malinowski's controversial *Diary* and the proposers' struggles with earlier publishers. Comments are made about the photographs in the ensuing volume. Bizarrely, it is now taken for granted that the specificity of the fieldworker is crucial when it comes to the choice of subject and rapport with key individuals in the field.

Keywords

anthropological debates, ASA, autobiography, feminism, history of anthropology, reflexivity

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The book *Anthropology and Autobiography,* which I co-edited with the now late Helen Callaway, has an extended early history before its completion, drawing on what was once declared highly controversial, indeed unpublishable. Decades later, many aspects of the volume are taken for granted and younger generations, if not my own, are bewildered by and incredulous at the 1980s opposition to confronting the specificity of the fieldworker, the effects of her/his interaction and the varying accessibility of indigenous allies in the field.

Recalling my initial proposal for the biannual conference of the Association of Social Anthropologists (ASA), founded in 1946, it is strange how things and procedures have now changed. It is bemusing as to how the theme was then seen by some 'fellow' anthropologists as near crazy, indeed a threat to the very discipline of social anthropology. In those days of the late 1980s, where the Internet and e-mails

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were non-existent, I, like others before and after, had personally to make the proposal theme at the preceding 1987 ASA conference. The proposer had to arrive with sufficient sheets of paper outlining the theme, which was to be circulated to each attendee. Not being adept at typing, I was able to ask one of the departmental secretaries at my then university (Essex) to complete all the paperwork. This was the taken-for-granted practice in those days. Few academic staff had computers. Secretaries typed all our reading lists, reference letters and research proposals. At the last minute, I recognised the advantages of having a co-convenor and telephoned several friends.

The key person available was the brilliant Helen Callaway, who proved to be diplomatically adept relating to potential panel participants and the final contributors to the ensuing volume. In those days, an ASA conference had only single panels attended by everyone through the entire event. Attendees filled just the one conference room. Helen was also superbly skilled in the subsequent detailed editing.

The preceding 1987 conference was at the newly established University of East Anglia in Norwich. I took the train from Colchester and later met up with Helen. We distributed the pile of papers just before the deadline on the first day. There was no way that, in the pre-computer days, the single overall theme proposal could be circulated by e-mail for approval, as in current times. Instead, each proposer had to make his/her speech in person. Then, after varied comments from the floor, the audience of attendees present were expected to vote by a public show of hands. Our other 'rival' proposer, now a celebrated professor, having weeks, if not months, earlier heard about another proposer (seemingly the only competitor), had successfully encouraged this 'rival' to merge with his own theme. So confident was this proposer in succeeding, that he did not bother to travel to Norwich, but instead delegated the presentation of his proposal to his Head of Department, Professor Marilyn Strathern.

Both before and after our speeches, the range of negative comments towards our proposal from the floor was very disturbing. Attendees were informed by specific individuals that my proposal was 'narcissistic', 'navel-gazing', a 'feminist plot' and 'California speak'. One professor insisted that bookshops in California were already groaning with such self-obsessed books. There was no need for any more in the United Kingdom. My only knowledge of that California locality was of an inspiring radical named Bob Scheer, whom I met when he visited Oxford. We were on a CND march from Oxford where I was studying. Bob, an American citizen, had illegally visited Cuba,

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protested against the Vietnam War and worked in the famous City Lights Bookshop in San Francisco, which was founded by Lawrence Ferlinghetti. I still have a postcard from Ferlinghetti sending best wishes from Bob. I never saw self-obsessive narcissism in the latter's impressive political biography and publications. He took risks as a pioneer just like Helen and I did.

Having rehearsed my proposal well in advance, I still trembled and sweated before my speech, outlining the arguments all carefully written in advance. Eventually, after the range of responses, it was time for the vote by the open show of hands. One vivid image remains with me. To my joy and astonishment, Edmund Leach and Raymond Firth, seated a few rows in front of me, both raised their hands in approval of our theme, 'Anthropology and Autobiography'. Indeed, Helen and I won by a respectable majority. Thus, the future fate of the edited book to emerge in 1992 was sealed. Sometime after the voting decision, I encountered Marilyn Strathern in the ladies toilet. She spontaneously declared: 'All is forgiven in love and war'.

By the ensuing deadline for the 1989 conference, we had a surplus of participants: so much for the negative comments in 1987. One applicant, later appearing in the book, had once been our theme's most vocal critic. Where possible, Helen and I chose to have at least one woman in each panel, thus subverting the long tradition of mainly all-male panels. We played about with possible rivalries, setting up individuals who might stimulate significant disagreements. I also recall an angry letter from a Dutch anthropologist when we did not include her doctoral student as a presenter. Later, at the conference, she apologised when realising there were *no* simultaneous panels, as would be customary years later.

Despite the small number of presenters, there were still awkward decisions to make as to who should be selected for the ensuing volume. Helen and I decided that a key factor was if we *both* agreed about a contributor. If one conference applicant did not fit with the criteria of either of us, the individual was usually excluded. But years later, one such reject, by then a professor who I encountered at an American conference, seemed never to have forgiven me.

It is revealing to explore the context and history of our original choice of theme. It was not a sudden, seemingly trendy decision. Retrospectively, I realise it was subconsciously driven by years of personal experience of sexism, making it impossible to ignore my gender in early and later academia. Thus sexism was institutionalised both before and after fieldwork. Some of those 1987 male objectors

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had indeed dismissed our proposal as a 'feminist plot'. But the male anthropologist may arrogantly presume his gender to be the taken-for-granted norm. Already I had been inspired by the controversial debates over the use of the 'I' in contributors to Clifford and Marcus' *Writing Culture* (1986). But many of us wanted to argue that the introductory use of 'I' in texts was much more than a sign of authority of having been there, as highlighted in one article.

The anthropologist's gender specificity is never invisible in the field. In academia, I was ever so branded, never gender-neutral, and of course later made extremely sensitive to the implications of doing fieldwork with 'Gypsies', not only as a Gorgio outsider but especially as a female in her twenties. Here, social anthropology proves to be the crucial discipline where face-to-face fieldwork by the researcher is central. It cannot be delegated by the now labelled Principal Investigator (PI) who, as a distant, allegedly detached true scientist, writes up the anonymised othered employees' empirical research data.

Social anthropology, thanks to Bronisław Malinowski and other pioneers, depended, from the early twentieth century, on a *direct* rapport with the research subjects. Information was not dependent mainly on research 'slaves' gathering the data through set questionnaires whose content was then passed back to the 'Leader' researcher. How ironic that the latter was the least likely to have encountered the research subjects. A modern PI writes up the accumulated material often without ever having encountered the subjects, let alone having visited the locations. There was in some disciplines even a belief that a researcher who had visited the locality and spent time with the 'informants' was dangerously 'biased' by any personal encounters. Spatial, social distance and ideally total detachment were and still are perceived as comprising the true scientific method. Today, there are still disciplines which embrace such positivism, which has long been rejected in social anthropology.

How ironic that it was the School of Sociology (not anthropology), based in Chicago, that first invented the term 'participant observation'. Yet this has been side-lined, or forgotten in UK sociology and elsewhere. Before the Second World War, the mainly white male sociologists based in Chicago had the admirable curiosity to study hitherto unknown districts which were actually part of their *own* Western city. They moved beyond their social class and ethnicity to make the acquaintance of fellow citizens, mainly of Italian origin, who, despite living in nearby localities, the academics had rarely encountered.

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One such sociologist, William Foote Whyte, in the now celebrated 1955 appendix to his original monograph *Street Corner Society* (1944), recorded his rapport with a local informant who delicately suggested to Whyte that he stop asking questions and just ‘hang around’. It is therefore shocking that, just a decade ago, I discovered that two professors of sociology at a UK university where I had joined a doctoral research committee had to be informed by me of the academic value of participant observation. They repeatedly critiqued the candidate, trained in social anthropology, for doing qualitative ethnographic fieldwork. She had moved beyond the set questionnaire the professors had prioritised. It seems that many UK sociologists have forgotten their academic history and prioritised quantitative data collection. Such detachment automatically excludes any recognition of the specificity, let alone personal experience, of the researcher. Ironically, the *Anthropology and Autobiography* (1992) volume would, still in the twenty-first century, have to face from those professors the very critiques that I first encountered in the late 1980s when I proposed the ASA theme.

I had already faced such controversies before embarking on my 1970 research amongst ‘Gypsies’. Fortunately, when studying for the postgraduate anthropology conversion diploma at Cambridge in 1969/1970, we five postgrads had a special course devoted to the complete works of Malinowski that was taught by his former student Edmund Leach. It was Malinowski who first argued that the anthropologist should ‘pitch his tent in the village’ (1922). This argument gradually developed into full participant observation, a practice that would be practised by the Polish-born researcher, who would become a professor at the London School of Economics.

Nonetheless, Malinowski’s own self-analysis as part of his Trobriander research project was side-lined in our Cambridge course. The very privatised recognition of Malinowski’s specificity, indeed autobiography, later emerged with the then-posthumous publication of his diary in 1967. In his private daily narrative, we learnt of his controversial racial stereotypes and his regular contacts, indeed close friendships, with local white officials and traders, all contradicting the image of the Trobrianders as cultural and economic isolates. Malinowski even reprimands himself for devoting too much time with the Western outsiders, seeing them as his preferred friends. His monographs barely mention the political and economic interconnections between incomer outsiders and the indigenous islanders. We learnt also about Malinowski’s feelings of loneliness and sexual longings, as well as

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sexism. His dependence, as a man, on mainly male informants has the potential to explain his limited accounts of Trobriand sexual beliefs.

The controversial publication of the diary was deliberately delayed. In fact, Edmund Leach insisted to us postgraduates that it should never have been published. He argued that Malinowski's widow had been shockingly pressured to release it. Another of Malinowski's protégés, Raymond Firth, eventually wrote a tactful introduction to the first published edition of the diary. A special neutralising footnote was devoted to Malinowski's use of the 'N word', which if so allegedly bland never appeared in Malinowski's official texts.

I am proud that in 1975, after my main fieldwork with Roma 'Gypsies', and then registered to write up my doctorate at the ISCA Anthropology Department in Oxford, I published an article – 'The Self and Scientism' – defending the insights of Malinowski's diary. This was in *JASO* (the *Journal of the Anthropology Society of Oxford*). Again, I could not anticipate that such a focus on the value of confronting the fieldworker's specificity was later to be dismissed as mere 'California speak'. But a feminist standpoint probably subconsciously drove me.

The struggle just to be an intellectual had a very early history. In my late teens, after A levels, I escaped my dreaded boarding school, where the headmistress had told me that I would be 'selfish' to go university. It did not matter that I had fourteen O levels and studied for four A levels. Apparently, I would be 'depriving a more worthy person of a place'. Few non-Brits understand the imagined culture for the upper- or middle-class women in that era. The royal family reflects this. Neither Princess Anne nor Princess Diana even had the minimum qualifications, let alone the desire, to attend university. Branded a rebel, I enrolled at the Sorbonne in Paris to study a course prepared for foreigners. I stayed in a cheap hostel.

Independently, I read the works of Simone de Beauvoir, especially *Le deuxième sexe* (1949). She converted me to feminism. Years later, in 1986, I published a book on her in the Virago Pantheon Pioneers series. Already this contained a relevant theme presaging the ASA. I had argued that de Beauvoir's autobiographical *Mémoires d'une jeune fille rangée* (1958) had demonstrated that some of her generalisations in the earlier seemingly impersonal *Le deuxième sexe* unconsciously reflected some of her own autobiography. She was happy to make generalisations about peasants and gender, but there were few if any insights into working-class female Parisians. Yet Paris was her main living locality, where she had years of personal experience.

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Approaching the Virago Pantheon Pioneers series, as a non-historian, I felt unqualified to write an introductory historical chapter on the context of de Beauvoir's celebrated 1949 volume. Instead, I decided to cite some of my innocent 1960 'under-linings' of my original French volume. This was to show the changing context for my lived experience and the contrast with perhaps other generations' readings of de Beauvoir decades later. My own reading in earlier times, before the emergence of the Women's Liberation Movement, was very different. In 1960, as a recent school-leaver, I had scribbled celebratory comments about de Beauvoir's assertion that women were of *equal* intelligence to men, something which came as a revelation to me. There were further revealing 'under-linings' and comments which I would never have made later in the mid 1980s. Gender inequality issues had become more apparent by then. I also included autobiographical testimony from my female students, of varied cultures, on this pioneering feminist. So it was not all about the one self.

The Virago editor approved each chapter draft as I posted it. But suddenly she summoned me for a formal meeting. I was warned that the American publisher had insisted that I delete *all* such personal asides from my drafts. Apparently, this was because I was *not* a celebrity. When I refused, I was warned I would miss out on the massive advance. I was also asked to accept a new chapter written by the publisher's staff. Again, I did not accept this without corrections. There were multiple errors. The non-French-speaking American 'expert' had depended on the highly controversial translation made by a male biologist.

I won the major struggle. Even in recent years, my insistence on retaining the personal as intellectual record continues to be valued. Recently I was asked to give a lecture on autobiography to a multicultural group of students at UCL. Many loved seeing the historically dated scribbles on my copy of *Le deuxième sexe*. Just this past September in 2021, I received an e-mail from a Sikh-born professor in the United States recalling with pleasure my personalised analysis of de Beauvoir. The US publisher did, however, succeed in deleting from their edition the photo of de Beauvoir buying a daily newspaper from a female working-class Parisienne. Beneath, I had explained that this was probably the only regular contact the author had with such underprivileged women. The US publisher defended this exclusion because 'in America we don't do class'. Thus I had learnt from personal experience how, even after the 1970s Women's Liberation Movement, some publishers rejected the celebrated argument that

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'the personal is political'. In fact, the publisher's actions actually confirmed the feminist stance. But because I had rejected their major critique that the personal should be excluded, they ensured that the publicity and sales of the US version would be minimal. Their multiple editions were 'lost' or pulped.

Today, ironically, the autobiographical text, now 'populistically' labelled the 'memoir', has become a mass product, especially for media celebrities. But additionally in academia, biographies are now more fully respected. At Oxford's postgraduate Wolfson College, the Oxford Centre for Life-Writing hails autobiography or biography as a crucial intellectual contribution to knowledge. Key incidents of an individual's life history may help to explain subsequent research foci, theories and interpretation. It is therefore no longer perceived irrelevant to scrutinise individual biography, let alone condemn it as mere narcissism as did some academics in 1987.

How much more relevant is it now accepted to confront both the anthropologist's pre-fieldwork biography and his/her in-depth experience in the field? Sigmund Freud is taken seriously within different disciplines. Some pioneering anthropologists have also expanded hitherto unexplored consequences of the researcher's ethnicity, class, age, gender and individual personality for fieldwork practice. There are the immediate and long-term consequences as to who welcomes and is ready to confide in the outsider researcher. Roger Sanjek, in a pioneering 1993 article, elaborated the crucial role of assistants in the field. He suggested that their co-operation could be seen as a form of 'hidden colonialism'. Again, I suggest that the specific assistants depend crucially on the individual outsider anthropologist who is assisted and often befriended in key aspects of the ensuing field research, analysis and final publication. Thus the biography of the fieldworker has vital consequences in ever new ways. There will also be prior clues as to a convenor's choice of theme in making a conference proposal.

Just as the specificity of race is now more readily acknowledged in wider politics, for instance after Black Lives Matter, so gender, and especially the feminine gender, is taken more seriously in assessments and judgements. However, despite the early 1970s Women's Liberation Movement, there was little impact in academic institutions in that decade. Here, the autobiographical experience of this anthropologist had its painful impact. At my 1976 interview at Durham University, the all-male committee had noted my CV naïvely mentioning that I had been the first woman member of the Oxford Union and that I had published an article on 'Gypsy' women in Shirley Ardener's *Perceiving*

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Women (1975). I was asked by a senior member of the interview committee: 'We see you are a feminist. Would you still be interested in the job if you could not teach anything on women?' I had to promise I would *only* teach about men when discussing cultures around the globe.

Thus gender specificity had its impact in academia long before my proposal for the 1989 ASA conference. In fact, sexism was built into the culture for undergraduates at Oxford for some centuries, thus encouraging that it would be taken for granted at the staff level in other universities in the 1970s. The swinging sixties had a minimal effect in traditional university establishments, although students made attempts at subversion. Fortunately, I had arrived as an undergraduate at Oxford in the early 1960s as a feminist, having immersed myself in the works of Simone de Beauvoir. But again, there was a need for feminist struggles. Women were forbidden membership in the famous Oxford Union Debating Society. Friends and acquaintances of different political parties joined together for a campaign. Eventually we won the necessary majority vote, and I was labelled as the first woman member.

To conclude, this celebratory article devotes little space to the final content of the co-edited book, which is available for all to read and celebrate. Instead, it has been important to explore some of the context of its emergence. But I should mention something about the photographs. They indeed reveal the personal identity of the specific researcher, including the revealing two photos through time of Margaret Kenna. One is of her with a small Greek boy on her lap, but the other is a picture of that same boy, now a man, helping her take off in her boat. Here is visual evidence of the changing specificity of the anthropologist and her different status according to age and acquired seniority.

Before the conference, I had asked each contributor to offer a photo of him or herself in the field. The open display in the conference room caused a stir, especially that of Roland Littlewood, who sensationally presented a photo of himself full frontal naked. He lived with a naked community and eventually went 'native'. I jokingly asked if this could be the front cover of the book to attract sales. But this was unsurprisingly refused. Instead, I chose a photo of Paul Spencer. This shows the tall indigenous Samburu staring in disbelief as a small white man struggles to move his Land Rover from a dip. It reveals cross-cultural contrast in behaviour when help in the white man's culture might be taken for granted. My own photo demonstrates how, even though I

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knew the Gorgio photographer well, I had unconsciously imitated the 'Gypsy' woman's consenting but defensive stance towards a stranger. This proved my bodily integration.

Finally, the insights offered by what is now called 'auto-ethnography' have been explored in my recent book, *Anthropological Practice* (2012). I tape recorded dialogues with over twenty anthropologists of multiple nationalities who had done fieldwork around the world. Their personal answers to my questions revealed major insights into how fieldwork is conducted and research completed. Thus decades later, the original objections to our ASA proposal are, ideally, exposed as fully dated and irrelevant.

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