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Panel A3: "Researching Tourism: Reflexive Practice and Gender".
Filareti Kotsi: "**Mirroring the Anthropologist: Reflex-ions of the Self'.**

This article is part of an overall research that explores the enchanting experiences of the pilgrims and tourists visiting the pilgrimage site of Mount Athos (situated in the north part of Greece)[1]. It concerns a monastic peninsula where only men are allowed to enter at the total exclusion of women. The bibliography that concerns Mount Athos is written mostly by men, being the only ones that have access to it. All existing works are conducted by byzantinologists, archeologists, and geologists who study the monastic peninsula as a whole. The scope of the anthropologist is not common, especially that of a woman anthropologist studying women pilgrims. My research thus more specifically explores the pilgrimage that takes place in the sea where women have the opportunity to see the monasteries of Mount Athos from a 500 metre distance and meet the monks who come on a little boat from their monasteries with the relics of various saints[2].

I conducted my research by living in Ouranoupolis (the last village situated at the border of Mount Athos) for two to eight months per visit during the years 1997-2000. Present from the beginning of May to the end of December, I lived the rhythms of the village during the cold and deserted winter months as well as the hot and packed summer months, at a time when the village is full of pilgrims and tourists. During these years of research I myself became a pilgrim, a tourist, sometimes a guide and even at times I practiced being a saleswoman. I was taken for a journalist when jotting notes or a photographer being inseparable of my Nikon F70 or even a spy when I hung about at the same places at different hours, asking questions or simply remaining silent.

The aim of this paper is the evaluation of the positive and negative results that derived from my indigenous identity and my gender during the anthropological practice. Throughout my research I found myself working in my own country after some years of absence and I rediscovered the meaning of being a Greek, an orthodox and a woman. As a result, my research has developed into an auto-ethnography. The term auto-ethnography traditionally referred to the cultural study of one's own people, but according to Norman Denzin, more recently it is "a turning of the ethnographic gaze inward on the self (auto), while maintaining the outward gaze of ethnography, looking at the larger context wherein self experiences occur" (Denzin, 1997: 227). Having found ways to distance myself from what I thought as familiar, I felt enchanted while rediscovering the Greek culture and the orthodox religion as well as gender issues, looking at them through the anthropological lens. In particular, in this paper, I explore the outcome of knowing the tacit codes of the culture as well as the language, in other words I examine what it means to be a native anthropologist. Given the fact that my research concerns pilgrims, I also look at another aspect of my identity, my belonging to the orthodox religion and the impact that this had on my research. I also examine the consequences of being a woman and how this influenced

[1] *La communication enchantée. Une anthropologie réflexive du tourisme religieux autour du Mont Athos, Grèce*, Lyon, Ecole Normale Supérieure, 2003.
[2] "The Enchantment of a Floating Pilgrimage. The Case of Mount Athos, Greece" in *Vrijetijdstudies*, Wageningen, 17, 2, 5-20.

the representation of reality and whether this fact facilitated, impeded or modulated the conduct of my research. I discuss the inconveniences and the advantages of doing fieldwork as a single woman and working as an anthropologist at home. I explore the ways the identity of the researcher influences the gathering of data and its interpretation during the ethnographic research and during the writing of the ethnographic text.

I focus on these implications of my personal identity in a reflexive manner making this paper auto-ethnographic in the double sense of the term as referred to by Deborah Reed-Danahay. The term refers "either to the ethnography of one's own group or to autobiographical writing that has ethnographic interest" (Reed-Danahay, 1997: 2). I unify these two aspects, using the term auto-ethnography as a study of my proper culture as well as an auto-biography limited however to the total and exclusive duration of the research[3].

According to Myerhoff and Ruby (1982) **reflexivity** is the process by which an anthropologist understands how her social background influences and shapes her beliefs and how this self-awareness pertains to what and how she observes, attributes meanings, interprets action and dialogues with her informants (Hertz, 1997: 152). Numerous are the writers nowadays that are in favor of reflexive ethnographies. The literature that dates the two last decades shows that anthropologists are preoccupied with the reflexive way of making their research and of writing the ethnography. They do not want to move away the writer/ethnographer from his own narrative. "The reflexive I of the ethnographer subverts the idea of the observer as impersonal machine" (Okely & Callaway, 1992/1995: 24).
"Reflexivity implies a shift in our understanding of data and its collection – something that is accomplished through detachment, internal dialogue, and constant (and intensive) scrutiny of "what I know" and "how I know it" (Hertz, 1997: viii). Several of the authors in Hertz's collection *Reflexivity and Voice* admit that revealing oneself is not easy. The crucial question they pose is how we set the boundary between providing the audience with sufficient information about the self without being accused of self-indulgence and how much of ourselves we want to commit to print (Hertz, 1997: xvi).
Pierre Bourdieu suggests that the genuine reflexivity does not consist in devoting *post-festum* to "reflections on fieldwork" as with the case of Rabinow, but claims that the position of the observer should be subjected to the same critical analysis in which the constructed object is subjected (Bourdieu, 1992: 36).

Being of greek origin, I lived in Belgium for seven years during the time of my studies. My anguish for choosing the place of doing fieldwork was evident. My interest in tourism was incontestable since I was attracted to traveling and the themes that are inseparable to tourism such as cultural, linguistic or culinary diversity. Nevertheless I could not decide on the actual place of fieldwork. My idea was to go as far as possible, to choose a distant and if possible remote place, in order to resemble classical anthropologists like Margaret Mead or Claude Lévi-Strauss whose experiences at the Samoa and Brazil made me wonder. At first I thought of going to Bali, reputed as a tourist paradise, because I believed it required all the necessary elements for an anthropologist working on tourism. I now consider that I was thinking like a tourist, as if searching for the best place for my vacation. It had to be, not less than an exotic place where I would be proud of being a "real" anthropologist, starting at least from learning the foreign language. Financial constraints made me acquire a more conformist spirit and I thus started considering Belgium as a possible place of fieldwork. Although Belgium is not considered a famous tourist destination, possibilities for such a research were surely possible. But this idea did not fascinate

[3] My ethnography (doctoral thesis) is also written in a reflexive way.

me at all. Having lived by that time for four years in Belgium, I naturally wanted to change destination. My behaviour reminds me yet again of the tourist who desperately needs a change after having stayed at the same place for a long time.

As time was approaching for my departure, I had the timid idea of going back to Greece. In the beginning I had excluded my homeland regarding it as an easy destination, having the impression that since I knew the language, the culture, the religion, there would be no originality and particularity in the outcome of the research. Undertaking a research in Greece would mean for me going home after a four year absence and it felt like going back without accomplishing the goal I had set. I did not even dare think to go to my natal town for research; it felt as if not doing fieldwork. I decided to do the research in the south of Crete and only when I wrote about it retrospectively in my doctoral thesis, I realised that the choice of the small village of Sfakia situated at the extreme opposite end of my natal town was chosen in order to be and feel far away. I was haunted yet again by the classic image I had of anthropology. This decision was materialized by a banal doctoral proposal which suggested the study of the identity of the Cretans and the effects that tourism would have on the inhabitants. I myself rejected the project and proposed a new one. Once again, it was as if I acted like the tourist who was not happy with the destinations proposed to him by the travel agent.

At that point I had decided to work on the notion of enchantment and the ways it can be produced in tourist sites even though I had not yet found my specific site of fieldwork. Before giving it a second thought I actually went to Santorin, one of the most enchanting Greek islands and fell into the trap of wanting to study the notion of enchantment in an enchanting place. It was not until the writing of the ethnography and the relevant detachment I had acquired, that I realised enchantment is produced by a multitude of different factors and directed by the professionals of tourism. While being in Santorin, I comprehended that the notion of enchantment should not be studied in an enchanting place. Parallel to finding the best place of fieldwork and while deepening the subject of my thesis, I felt a "paradoxical contempt" for the tourist. This expression, used by Jean-Didier Urbain in *L'idiot du voyage. Histoires de touristes*, signifies the contempt that a tourist feels for himself (Urbain, 1991/1993: 90). Once again it was as if I acquired the tourist's mentality who paradoxically denies his identity as a tourist (by at least regarding himself as a voyager or a traveller and calling the others –but not himself– pejoratively as tourists).

A different sort of traveller, compared to the tourist, but with many similarities to him, is the pilgrim. Both of them travel away from home, they seek to be in a liminal situation during their journey, "in a state of existence which is out of space, out of normal, everyday social-structural and cultural environment and beyond its social and moral constraints…" (Nash, 1981) and can be found at the same place and at the same time for different reasons and purposes. Pilgrimage sites can be simultaneously tourist attractions. The fact that pilgrimage is less studied by anthropologists and finding the interrelation of tourism and pilgrimage epistemologically more interesting, I eventually decided to study this amalgam. The work of the American anthropologist Jill Dubisch (1995), who did her research at the Greek shrine in the island of Tinos, provided my initial inspiration for working on the notion of pilgrimage and I realised that studies regarding Greek pilgrimage shrines undertaken by Greek anthropologists are almost non-existent. Approaching pilgrims during fieldwork and not only tourists intrigued me. My anguish disappeared.

Suddenly I knew the exact place of my research, as if by divine inspiration: the particular sacred site of Mount Athos.

All these thoughts took form when I started writing the ethnography, after having come back from the field. The choice of the field had troubled me so, that I had the need to write about it. It reflected the standpoint I had for anthropology. The need to be in far away exotic places reflected the influence that classic anthropology had on me in the beginning of my post-graduate studies. Unconsciously I had chosen three islands (Bali, Crete and Santorin) possibly believing that insularity would offer me the symbolic space I needed for searching the exotic. I believed that being at a far way island, even if that was in Greece would justify my anthropological status. I thought that if I had cultural and geographical distance it would guaranty me certain objectivity. When I abandoned my fascination with the exotic and turned my attention on my own society, I stopped sharing the idea of the first anthropologists that "the proper object for anthropological study was determined by its cultural distance from the West" (Davies, 1999: 33).

What finally led me to modern Greece was the enchantment I felt for reflexive and indigenous anthropology, as opposed to the English anthropologist Michael Herzfeld, who was led to anthropology by the enchantment he felt for modern Greece (Herzfeld, 1998: 3).

Judith Okely in her book *Own or Other Culture* proposes to see the West as exotic and rediscover it (Okely, 1996: 5). In classic anthropology the important was to familiarize the exotic while the anthropologist was in search of the "other". In **native anthropology**, the anthropologist must see the "self" as the "other" and redefine these two notions. The Greek anthropologist Dimitra Gefou-Madianou, in an article that concerns reflexivity and anthropology at home, suggests that the lack of geographical, cultural and sentimental distance gives another dimension to the notions of the "I", the "other", the "indigenous" because the "I" being at the same time the "indigenous" penetrates and becomes part of the "other" and vice-versa (Gefou-Madianou, 1998: 368). Kirsten Hastrup, in yet another article about indigenous anthropology, argues that the shifting of the anthropological interest is not the result of the disappearance of the primitive civilizations but the result of new epistemological researches that consider that all societies can become object of analysis (Hastrup, 1998: 339).

Indigenous anthropology and reflexivity are interdependent. As Dimitra Gefou-Madianou highlights, reflexivity introduces the redefinition and objectification of the "self" through the research of the "other". Studying the others, anthropologists become reflexive for their own "collective self", their own culture. In order to be in a position to see their "collective self", become in other words reflexive, they have to distance themselves from their "self" (Gefou-Madianou, 1998: 398). Indigenous anthropologists should have the ability to separate the personal "self" of the "collective self"; separate and keep the distance between themselves and their culture.

The repatriation of anthropology demands the defamiliarization of the familiar (Marcus & Fischer, 1986: 137-164). Hale Bolak points out that "while a foreign researcher runs the risk of being culture blind, an indigenous researcher runs the risk of being blinded by the familiar" (Bolak, 1997: 97). Nevertheless, I came across exoticism and difference in doing indigenous anthropology and I worked out procedures in order to defamiliarise the familiar.

> Diary, 7 august 1998 - A dinner party was arranged by the Papadopoulos family because they were making an extension to their house. A cock was killed and the constructor and his family were invited to the party as well as friends. I gave a ride to Katina and the hostess insisted that I stayed for dinner as well even though they met me for the first time. There were two enormous wooden tables and the garden was candle-lit. The cock was prepared in a wine sauce and placed on the tables together with ouzo, retsina and other mezedes…

I sometimes felt like a stranger towards my own culture. Going back to Greece after having lived in Belgium, I shared the feelings of strangeness like those described by Alfred Sch tz's for the *homecomer* (Sch tz, 1944a: 369-376). "To the homecomer home shows –at least in the beginning– an unaccustomed face. He believes himself to be in a strange country, a stranger between strangers, until the goddess dissipates the veiling mist" (Sch tz, 1944a: 369). The homecomer suffers the typical shock felt by Ulysses, the most famous homecoming in literature. The author principally refers to the case of the returning veteran, but also to the traveler who comes back from a foreign country or the emigrant who returns to his native land. I find a correlation between the homecomer and the indigenous anthropologist who lived in a different country than his own.

The killing of the cock on the foundations of a house was common to me before the time I went to the field, but the complex context in which I experienced it gave it another dimension. I participated in an event that has mostly stopped taking place in greek towns and I afterwards wrote down about it in my diary. While writing about it (I noted then in brackets that I had the impression I was doing my research within an exotic tribe), it sounded bizarre in my ears. Up until the moment of going to the field I thought of it as an ordinary event: we kill a cock for good luck, for happiness. No one poses more questions. Writing about it, its cultural particularity and its "Greekness" emerged. Many times I had this sentiment of unusual exoticism. This experience of strangeness with my return to Greece was reinforced by the fact of undertaking the research in a small village. Being equally in a village situated within a religious frame, this sentiment of strangeness was accentuated; I was unaccustomed to some behavioral patterns. I returned "at home" for the research, but "at home" represents Greece and the greek culture in the wider sense of the term. Ouranoupolis, linked to a superficial knowledge, since my childhood, as a place of vacation, was nevertheless a new space for me with its proper particularities. Having grown up in a Greek town at about 400 km distance, having also lived for several years in the second biggest town of Greece (Salonica), I had a general knowledge of interpreting the patterns of the Greek culture and was more accustomed to the behaviour of people that live in towns. As Alfred Sch tz rightly puts it, in his article regarding the *stranger* that meets a new culture, "the culture of the approached group has its peculiar history, and this history is even accessible to him. But it has never become an integral part of his biography, as did the history of his home group" (Sch tz, 1944b: 502). Ouranoupolis was never an integrant part of my biography.

My return to Greece made me at the same time a homecomer and a stranger. The familiarity with the Greek culture allowed me not to have to learn everything from the beginning like an apprentice. At the same time, due to the geographical and cultural distance I had acquired by living in Belgium, I sometimes felt as a stranger to my own culture. Going back and forth to the two countries gave me a double and mixed identity, belonging at the same time "by half" to both of them, thus making me a *halfie*[4] anthropologist, neither exclusively indigenous nor exclusively stranger. These two identities were also attributed to me by other people: in Belgium I was "the Greek" and in Greece I was "the Belgian". This double identity helped me out in various circumstances during fieldwork and I could accentuate either the one identity

[4] The term *halfie*, introduced by Kirin Narayan, indicates people with a mixed identity that belong to two cultures. This situation is created either by an economic emigration, by an emigration for educational purposes or due to the origins of the parents (Gefou-Madianou, 1998: 379).

or the other depending on the situation found. Having this hybrid status I could be an *insider* at some context and an *outsider* at other.

The familiarity and knowledge of the Greek language was another feature towards my benefit. Speaking the mother-tongue is incontestably a big advantage to the anthropologist. It does not slow down the process of fieldwork. Foreign anthropologists can meet difficulties in understanding the subtlety of the language of the country they are doing their fieldwork in and as a consequence the subtlety of the actions of the subjects they are studying. My personal experience of learning the French language has taught me that the mastering of a language is not for granted (unless one is bilingual). I learned the French language in Belgium for educational reasons –not during fieldwork– and even though my stay was much longer than an anthropological research in a foreign country, I am not sure I can successfully do some of the following things proposed by Alfred Sch tz: "In order to command a language freely as a scheme of expression, one must have written love letters in it; one has to know how to pray and curse in it and how to say things with every shade appropriate to the addressee and to the situation" (Sch tz, 1944b: 505). It is not always evident that a non native anthropologist can reach the same level of knowledge of another language and an in-depth immersion.

The most important factor that favoured the distancing and objectification of my data was the original and unexpected way of the writing of my fieldnotes. Each evening I wrote my diary and then directly emailed it to Yves Winkin, my doctoral supervisor. He had proposed this idea to me "à la Margaret Mead[5]" and it spontaneously developed into a constitutive epistolary relation which started in 1998 when I stayed in Ouranoupolis continuously for eight months. Had I not been committed to sending my diary, it would not have been written with such perseverance, it would not have been so detailed and filled with explanations as well as emotions. My notes would have mostly concerned my research on tourism and pilgrimage whereas now the whole aspect of quotidian life in Ouranoupolis emerged as well. During the first months of my stay there, I wrote, among various other things, to my supervisor:

> I know already where one can buy tomatoes without hormones, the best cucumbers, who is ill in the village, the best beaches, which women were authorized to visit the Russian monastery, how many times one has to roll a leaf of eucalyptus to scare away the mosquitoes … (Diary, 1 July 1998).

This imaginary interaction with my supervisor, sometimes inhibitory, made my text clear, organized, live, vibrant and comprehensible to someone who is from a different sociocultural background. As a result my diary had a double function, besides being the instrument of taking down notes. By means of disclosing "the secret life of my fieldnotes"[6] to my Belgian professor, a posterior distancing turned out to be most effective. Writing the diary in English helped me distance myself from the data and aided me come out of my cultural universe. When I could not find a word in English due to its socio-cultural connotation, in my attempt to explain it by paraphrasing it, the whole socio-cultural background hidden in the word would emerge directly on paper. A denaturalization of local knowledge was taking place at the same time of writing my diary. My notes were filtered in relation to their

[5] Margaret MEAD (1977), *Letters from the Field, 1925-1975*, New York, Harper and Row.
[6] In an article entitled "The Secret Life of Fieldnotes", Roger Sanjek proposes anthropologists to show and circulate fieldnotes. *Fieldnotes: The Makings of Anthropology* (1990: 187-270).

"Greekness" long before the analysis of the data had even begun. This explicit way of writing led me to understand how I perceived the Greek world. Being obliged to translate and search for explanations on cultural patterns made me reflect on "Greekness". The identity of the reader played a fundamental role on the way I edited my fieldnotes and as a result my electronic journal permitted me to elude the pitfall of the evidence that entails indigenous research.

I had a much smaller diary as well, which I carried with me for jotting down notes in Greek, almost simultaneously with fieldwork, before transcribing them in detail in English. All interviews are also written in Greek in an attempt not to loose the authenticity of the words of the interviewees and be able to reflect on the translation at a later time. Reading in retrospect the Greek diary, I have the sentiment of being really involved, feeling very close to the people I was writing about. For example, when I read the interviews of the women pilgrims, I identify with them; I feel like their close friend in whom they confided their most personal problems. On the contrary, reading the whole (English) diary, it feels like reading a text written by someone else. It feels as if moving away from the events, as if they are seen through a cadre. I can reflect on my anthropological life as if in the third person. The English version of my diary helped me situate my anthropological life without being emotionally involved. It gave me the possibility to put a distance between my writing and myself.

I also had a third diary written in French. The first time I went to Ouranoupolis in 1997 for a short stay coincided with my first contact with the field. I still remember my first day of fieldwork. I returned to my hotel, I opened my brand new diary, I ordered coffee and I started writing my first fieldnotes with joy and pride. Looking back, the only answer I can give for writing my first diary in French is that it gave me the impression of being the "foreign" researcher/anthropologist that came from Belgium in order to accomplish a research regarding Greece. I had not yet reconciled with the idea of doing research at home, as if indigenous anthropology was not respectable anthropology. I had the impression that by writing in French, I would become the foreign anthropologist with a distant and objective stance.

Another issue that made me consider the frontiers between subjectivity and objectivity and helped me objectify the subjective process of self-consciousness was the fact of being observed. I became an observed –by my male professor– observer and the revelation of my fieldnotes to him made me reflect on the nature of the anthropologist who was at the same time "observed" while observing. Yves Winkin had the possibility to visualize my research in its subjective state, before I was in the position to filter and objectify my results. A double work of reflexivity had to take place. Not only had I to analyse the way I reacted to my own observations, but also to my own observations observed. I had to discern with honesty and audacity my counter transfer, as the ethnopsychoanalyst Georges Devereux refers to the way the observer of human facts reacts as a human being to his own observations (Devereux, 1980: 16), but also discern to my counter transfer observed.

Where should I note the observations of my counter transfer? Should I keep a more personal diary with the emotional or sentimental situations, the failures, the pleasures or the censorships? If so, it would be complicated to fix the frontiers. At which point would I pass from one diary to the other? At which instant the personal narrative would stop being part of the research? Would I reveal the difficulties I encountered? Did I want to show an image that was not close to reality? In that case,

what would be the aim of the principal diary, a supposed objectivity? These questions tormented me. Added to the fact that talking of myself by myself, seemed arrogant. Although in the beginning of the research I had difficulty in choosing what to include and what to exclude in my fieldnotes –since they would be read by my male supervisor– and I questioned myself on the frontiers between my personal life and my life as an anthropologist, I soon realized that these two lives could not be separated during the research. As such, I succeeded in inserting my personal life through an anthropological frame. I equally realized that the backstage events, which are more than often left out of the ethnography, are of the same importance of the frontstage ones. At the course of writing the diary, I excluded the separation of the private and the fact of including myself in the fieldnotes came naturally. Had I not had a reader, I would probably not have included myself in the narrative text. For this reason I also became "object" of my research, and including myself in the narrative I became the "other". I became an integrant part of my study and I learned to use myself as an ethnographic tool since the writing of my diary and to be reflexive from the very start of the research and not only during the writing of the final ethnographic text.

Although I had found ways to defamiliarize the familiar as an indigenous anthropologist (by living in a different country than my own, by writing my diary destined to a non-Greek), I was found in a difficult condition of distancing myself from the orthodox religion and its rituals. Knowing the common knowledge rules concerning the ways to be religious, I often omitted to write fieldnotes in the beginning of my research. During the women's pilgrimage, everything was so natural, so self-evident, that finding ways to distance myself came gradually and belatedly. Like most Greeks, I am officially Christian orthodox, not really involved in religious practices, not a church goer, I above all celebrated Christmas and Easter when I was at a young age –mostly with my mother and less with my father–, and I have always participated in religious rituals like baptisms and marriages. What interests me in religion is the aspect of rites of passage in various rituals and what drove me to this research was my fascination of the complex act of pilgrimage. The first time I realized that I could not see certain aspects of my world of reference regarding the orthodox religion was when I read the book about the pilgrimage in Tinos by Jill Dubisch, *In a Different Place* (1995). For the American anthropologist, Greece is "a different place" in that it is "seen as both the cradle of Western civilization and at the same time Oriental and hence somewhat mysterious". According to the author "although Greeks are Christian…, they are Eastern Christians, whose religious beliefs and practices may confound, and sometimes outrage, Western notions of religion" (Dubisch, 1995: 254). In the beginning of her research the writer was confronted with practical problems and did not know if she should kiss the icons or if she should make the sign of the cross. Genuflection was antithetical to her American notions of self and dignity. Her hesitation to make the sign of the cross signaled me the difference that separates me from a non-orthodox anthropologist. According to authors that reflect on indigenous anthropology, the comparison between ethnographies that approach their scientific object from the outside (foreign ethnographers) and from the inside (native ethnographers) is a strategy that helps towards a direction of a more complete representation of social reality (Gefou-Madianou, 1998: 396).

This confrontation with the American anthropologist, together with various encounters in the field with people from different religions made me conscientious of the fact that each one judges according to his proper history and politico-religious identity. For example, during an interview with a Dutch protestant priest, concerning

the veneration of the saints' relics, I was confronted with an attitude that I had not come across before undertaking the research:

> I find it very strange. I would never do it myself and I could not imagine other people kissing a part of my leg, was I to become a saint. But on the other hand, people also like kissing their favourite pop-stars, so it must be a kind of need (Diary, 17 July 1998).

Encounters like these, aided me in treating the familiar as anthropologically strange, suspending my preconceptions and looking at them through the eyes of a non-orthodox. The reactions of non-orthodox, researchers or not, were a way of assuring distance and detachment. I was not totally a stranger to orthodoxy, to the point of not knowing how to make the sign of the cross, but neither a profound believer who would analyze the pilgrims' practices from the point of view of faith. The distance that I have towards my religion, facilitated me to hold back in relation to religious events and keep an exterior point of view.

Published in 1970, Peggy Golde's collective volume *Women in the Field: Anthropological Experiences* was the first study to take into consideration the importance of the **anthropologist's gender** during fieldwork. It gave the anthropologists the opportunity to consider the influence of the researcher's gender on the ways it could affect the results of the research. The authors of this volume, all women, signaled the importance of the difference in gender as well as age, colour, family status.
Two decades later *Gendered Fields: Women, Men and Ethnography*, gave the opportunity to male ethnographers to treat the question of the identity of the ethnographer. Patricia Caplan, one of the editors of this collective study, suggests that most men find autobiographic writing more difficult than women (Bell, Caplan & Wazir, 1993: 19).
As far as research on Greece is concerned, the authors of the volume *Contested Identities: Gender and Kinship in Modern Greece* (Loizos & Papataxiarchis, 1991), do not discuss how their own feminine or masculine identity influenced their research although it is oriented towards gender and identities in Greece. Only Jill Dubisch makes an allusion of the importance of the gender identity of the researcher and the need of confronting the personal feelings about gender roles in her article "Gender, Kinship, and Religion: Reconstructing the Anthropology of Greece (Dubisch, 1991: 46). She remarks that "Not only does every fieldworker have a gender identity acquired as a member of his or her own society, but this identity interacts in complex ways with gender in the society being studied. Such interactions can, when recognized and analyzed, lead to important insights (Dubisch, 1991: 32).

> Sorry, for the confusion regarding your sex! I, too, had made the assumption that since you were working on Athos (it wasn't clear to me that you weren't actually working on Athos but on its margin) that you were a man. Also, you have to forgive the ignorance regarding your name, it is not immediately clear for this ear that it's feminine (Diary, 10 September 1998) (E-mail communication with an American anthropologist).

–Being a woman– and choosing Mount Athos as a subject of research created some complications concerning my gender; people often assumed I was a male researcher. Being a woman, I was led to work on the fringes of Mount Athos, at the village in the immediate proximity which is enormously influenced by the existence of the sacred world. At first, I was frustrated and thought of it as a disadvantage, often considering the things I could not do and not the things that I *could* do. Being a woman next to Mount Athos intimidated me in the beginning. I always had to justify the reasons I chose this place for research and not some other pilgrimage site where I

could easily have access. I mostly regarded as a handicap the fact of not being able to enter Mount Athos and I was often annoyed because daily discussions with the locals, the pilgrims, the tourists, everyone, always concerned themselves with the particularities of Mount Athos. I had not taken into account that the fact of being a woman could influence my research. I had not considered that my feminine identity was going to be revealed as being constructive and lead to me to important insights.

–Being a single woman– without a family (whether parents or husband), not a tourist, nor someone who was working at the tourist industry, and coming to live in Ouranoupolis was an unprecedented event. For a small group of people it was never understood. During fieldwork I had participated at the XIV International Sociological Congress in Montreal and at some point in the course of my absence two couples, relatives of mine, had stayed at my house. When the landlord realized it, presuming they were unmarried couples he demanded that they pack and leave. Added to that, on my arrival from Montreal he asked me to move out because illegitimate sexual relations took place at his rented house and also insinuated that my trip was connected to drug traffic. The psychological stress that awaited me since my return from Montreal, the lack of hospitality towards my relatives, the search of a solution if I moved out, the impatience to see whether the community was to be in my favour or not, created a very painful experience for me. At the same time though, this event taught me how to transform an unexpected disaster into an anthropological analysis. This experience introduced me to various insights since it was part of the milieu I was studying. I became aware that following a situation of conflict, the anthropologist discovers many things that he would never have found out in normal situations.

In retrospect this event produced a rich source of data. Firstly, the dimension that the incident took connotes that I was an integrant part of the community of the village and not a passenger or another tourist that nobody cares about. I had to fight to keep up a positive image of myself. If the village was not in my favour, it could be fatal for the continuation of my research. Secondly, the landlord's comportment aimed at accentuating the religious character of the village which is situated in the proximity of Mount Athos, wishing to show that he was part of it. There definitely exists a conservative character concealed behind the mask of tourism but it cannot be generalized that the community of Ouranoupolis is that rigid and austere. Thirdly, I have reasons to believe that he wanted to raise the rent, menacing me that if I do not move out, he would triple the rent. If I was a male researcher, I believe all this would not have taken place. My reaction was dynamic and firm and at the hearing of consulting my lawyer, he did not disturb me again. Moreover, I had the pleasure to see a big movement of solidarity by the locals in my favour. Being a native anthropologist, it was significant for me to have good relations with them in order to be able to return to the village without being stigmatized.

–Being a single woman– it was natural that the locals wanted to situate me in a family context since family is a very important institution for Greeks. Of noticeable importance was the two-week visit of my mother; it, at least, removed some doubts concerning my origins. She also played an important role during the boat pilgrimage. I was not regarded as a solitary woman by the other pilgrims and it was even easier to start a conversation been accompanied by her. The various captains and crew were happy to meet her and know more of my origins. Apart from accentuating my respectability, the presence of my mother facilitated the deepening of some social relations. It was with her that I confronted the first foreign tourists for interviews, something that I kept postponing for the following day. She was, on the other hand,

initiated to anthropology and demystified the reason I had to actually live in a different place than my hometown in order to do my research.

–Being a single woman– at the age of twenty-nine, still at university and not having children was worrying for my informants. I had a couple of indirect and premeditated, by the father or mother, marriage propositions. The following extract of my diary is between a captain and myself during a pilgrimage.

> Captain Kostas asked me how many languages I speak and if I was engaged. I was a bit perplexed at this double question, but he continued. I have a son who is not married and half of this boat belongs to him (Diary, 27 June 1998).

–Being a single woman– meant that I had to be positioned towards rumors concerning imaginary sexual relations. Two months after I had departed from the village, I received a telephone call from one of my informants: "Giannis died of cancer and the secret will not be revealed". The secret would be the name of a monk with whom I had a supposed relation, according to the local resident. The incident sounded amusing at first, but then it shocked me. My informant continued:

> "In small society, things like that can happen. I think that some men did not like your liberality. It disturbed them; they prefer that women wear the veil" (Diary, 14 February 1999).

Another rumor insinuated again a supposed relation I had with a captain for six days and six nights on his boat with my family searching for me in vain. After some similar events I did not know how I was supposed to behave or where the line of demarcation was. Sometimes simply talking to men was sufficient to lead some people to the conclusion that I was related to them. These rumors had disturbed me. Fieldwork had an effect on me and reciprocally I had an effect on it. The anthropological research is not done independently of the researcher and it is not possible that the anthropologist observes from the outside. I was included at every moment. A photographer that was working in the village was, on the contrary, not happy that nobody talked about her. Surely being an anthropologist is a more exotic category than a photographer. Sometimes it was not easy for some of the locals to understand the necessity of such a research, as shows the following remark made by a captain:

> The only thing I understood about you is your name, that you have five or six diplomas and when you have nothing to do you come to Ouranoupolis (Diary, 6 May 1999).

–Being a woman– may be a potentially limiting factor in the female researcher's efforts to work in societies characterized by a high degree of sex segregation, tell us Soraya Altorki and Camillia Fawzi El-Solh, in the introduction of *Arab Women in the Field: Studying Your Own Society*. Gender restricts exploration mainly to the world of women (Altorki & Fawzi El-Solh, 1988: 5). This appears to be also true of relatively less sex-segregated societies like rural Greece, add the authors, but they refer to researches done by Ernestine Friedl and Juliet Du Boulay in the 70's. Certainly the situation has changed thirty years later. It was definitely not the case of my research. I share the remark made by a male non-local photographer while witnessing a conversation between myself and a local working at the pilgrims' office. The latter, insisting that it is forbidden to take photos at the port, wished to confiscate my camera and went as far as wanting to arrest me.

> If you were a man, it would not have been possible to do the research. There would have been rivalries all the time with other men, and moreover you would not have been able to have access to houses and speak with women or have coffee with them (Diary, 4 May, 1999).

–Being a woman– I had a double advantage in the world of women as well as in the world of men. With the passing of time and a certain detachment I realized I was very close to women and equally close to men if I chose; on the contrary for a male researcher it would have been more difficult to be that close to women. Principally I had an almost unlimited access to the world of women, especially during the pilgrimage where ninety percent of the participants were women. It was so natural to start a conversation with them that the crew thought that I always met some old friends onboard and we were exchanging our news. Katina, my principal woman informant introduced me to spaces frequented by local women: friends' houses, family houses (during coffee morning hours), visits at the hairdresser's, shopping at mini-markets, church ceremonies, funerals, commemoration of the dead, cooking, making of traditional carpets. Her age of thirty eight allowed me to meet people from her generation, as well as her parents' generation and through her two children I met the younger generation. I became that person to whom women could talk to, confide in and find relief. For all these women, whether local or pilgrims, it was less inhibitory to talk to another woman than if I were a researcher of masculine sex. As for men, they almost always want to talk to a woman. I had no problem in approaching them and they responded to my questions regarding the tourist industry and pilgrimage (since mostly men worked at places that had a connection with tourism and pilgrimage) but also took the initiative to talk about their personal life. Without doubt, there had been games of dominance and power from both sides.
Nevertheless –being a woman– using, exploiting and profiting of my feminine identity, no place was excluded to me and people were open and available.

–Being a woman– I transgressed the frontiers of Mount Athos once. The subject of interdiction and the eventual transgression of Mount Athos was something I heard of very often. "Find a moustache and a hat… and we'll give it a try during the harvesting of the grapes" (Diary, 10 September 1998). –Being a woman– and working so close to Mount Athos, I was often proposed to secret visits to the monastic peninsula or at least to transgress the borders. During one of the cruises, at which the tourists have the possibility to see the monasteries at a 500 metre distance, captain Dimitris was so eager to show me the piece of wood with which the monks raise their provisions, that he approached at a five metre distance from the rocky mountains. He was very proud and happy for having taken such a risk, transgressing 495 metres of interdicted space. I experienced the "thrill of transgression" as called by David Sibley in *Geographies of Exclusion* (Sibley 1995: 32)[7].

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I do not regret having done my research in Greece, my natal country, even though this decision was not taken spontaneously. On the contrary, it was a very constructive experience for me as an anthropologist and as a person. In the beginning of my studies my decision was held up by the classic and traditional image by which the anthropologist should travel away from his homeland. In the course of the research

[7] In my ethnography, a part of a chapter is devoted to the enchantment of interdiction and transgression of the borders.

I was influenced by auto-ethnography: the idea of doing the research "at home" and including myself in the ethnographic text. My way of thinking and writing was directed towards reflexivity and "reflexive knowledge" as Rosanna Hertz calls the outcome of reflexive social science. The importance is on the "statements that provide insight on the workings of the social world *and* insight on how that knowledge came into existence" (Hertz, 1997: viii). Nevertheless, the reflexive manner of writing the ethnography can create fragile frontiers between talking of oneself and talking of others and I always had in mind not to reach beyond these bounds. Writing the ethnography in a reflexive manner, I learned to analyse the ordinary and quotidian events with a distanced eye and insert in my ethnographic text the perturbations that I provoked in the community, which are an important source of information. The researcher should treat the perturbations as the most significant data, says Georges Devereux (Devereux, 1980: 16).

I surely committed errors, as every anthropologist, going to the field with my personal beliefs, being a rather more cosmopolitan woman compared to the other locals. Rarely had I to conform to the rules of the village in order to be accepted as a single woman and that was mostly with some people of the old generation. It was important for me to keep my "honor" intact, since Ouranoupolis for me was not a small exotic and far away village on the map, but a place where I would often go back to. I could not avoid the social responsibilities. I also committed some errors despite the fact that I am Greek and a member of the Greek culture but that brought me a lot of insights.

My aim was to evaluate the positive and negative results that derived from my indigenous identity and my gender as well as the role that played the different insights I had. In this paper, I proposed the perspective of a woman insider. This does not mean that another perspective, of someone who is male and new to the Greek culture would not contribute pertinent results. The two possibilities would contribute to make a progress in the literature of this domain. My results only present one part of the truth. "There is no single complete view of society… Each perspective expresses one aspect of reality" (Altorki & Fawzi El-Solh, 1988: 21). "The advantage of male over female and indigenous over non-indigenous, or vice-versa, will to a large extent depend on the situational context in which the respective researcher finds her- or himself" acknowledge the authors Soraya Altorki and Camillia Fawzi El-Solh (1988: 21).

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